Bo Lindell

The history of radiation, radioactivity, and radiological protection

PART 3. 1950 – 1966

THE LABOURS OF HERCULES

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The History of Radiation, Radioactivity, and Radiological Protection

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Translated by Helen Johnson

through Snabböversättare Sverige AB

Translator's note: Any views expressed herein on any subject have, to the best of my knowledge, been expressed in the manner in which they are expressed in the original text and are not necessarily my own.

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FOREWORD

THIS THIRD VOLUME of my series on the history of radiation, radioactivity, and radiological protection looks at what happened in a wide variety of areas during 1950–1966, the period in which the Swedish radiation protection pioneer Rolf Sievert made his major international radiation protection achievements. It is a continuation of '*Pandora's Box*', 1996 (*English translation, 2019*) and '*The Sword of Damocles*', 1999 (*English translation, 2019*). The extent to which the various chapters will appeal to you as the reader will come down to prior knowledge and interest; no-one is likely to gain the same knowledge and experience from all of the chapters but there is, hopefully, something for everyone. (*Readers may also wish to refer to Prof. Lindell's foreword to the English translation of the book series, see Pandora's Box*).

The Hercules in the title is obviously Rolf Sievert, who was a big man in all senses of the word. His exploits include the creation of the medical radiophysics and his many achievements in this field, his design of innumerable ingenious ionising radiation measurement instruments, his work to effect the first Swedish Radiation Protection Act and a Swedish radiation protection authority, his early investigations into the natural radiation in our environment and the gamma radiation from the human body, his pioneering achievements for international radiation protection and the establishment of the International Radiation Protection Commission (ICRP) as early as 1928, his hard-earned success in creating financial resources for ICRP's continued operations and, to add to that, the steps he took to improve the Commission's work methods, his role in the establishment of the UN's Scientific Radiation Committee and in the work of said Committee, and his initiative regarding the organisation of the Royal Academy of Sciences' research activities in Upper Norrland, northernmost Sweden.

I myself have not endeavoured to depict the personage of Rolf Sievert as a *human being*; you will perceive some of his traits by reading the descriptions of the events. However, you will find an extremely accurate picture of what Sievert was like as a person in the last chapter of Hans Weinberger's book called *Sievert: Enhet och mångfald* [Sievert: Unity and Diversity] (1990), the fact that Weinberger had never even met Sievert making it all the more impressive.

As the narrative approaches the present day, a greater amount of material is available, and a greater number of interesting details jostle forward to be told. During the early periods, the situation was dominated by a relatively small number of pioneers, and the development of radiation protection was limited to a few countries. As time goes by, more and more people become involved and significant achievements are made in more and more countries. More organisations and societies are born. The material becomes so extensive that documenting it in full would require rather a lot of shelf space. So, I have managed to limit the depiction, choose between events and quotations and turn the selection into one that provides an adequately-animated-yet-not-too-unfaithful picture of what did actually happen. Necessity dictates the subjectivity of such a selection, which is exactly as it ought to be in order for a partisan author to be able to project the very picture he wishes to create. I would like to think that I am moderately impartial, but the more the story enters into periods of time and contexts in which I have personally been involved, the greater the risk that I, despite my best intentions, am unable to avoid placing particular emphasis on my own achievements. Bearing this concern in mind, there is also a risk of my underreacting. I have attempted to navigate between these extremities. And, in spite of everything, there is an advantage in being able, in the words of *Ensign Stål*[*], to say in many cases: 'Well, I can tell Sir about it if he would like because I was there at the time!'

Another aspect of the subjectivity is that the selection of events is largely dependent on my own experience. I know more about what has happened in the Nordic countries than what has happened in Central Europe and much less about events in Asiatic countries. The portrayal is still very much international largely because I have been on international assignments myself.

Another problem lies in the clash between the quantity of information and the readability thereof. From one extreme, the book would simply offer an irritating list of details and names. To the other extreme, the book would become easy reading – but not the source of knowledge that I was seeking to provide. It has also been a matter of deciding the right direction to take, which has not been easy. Once you have said A, the expectation is that you will then go on to say B. It is almost impossible to objectively draw the line as regards that which ought to be included; in practice, this line has been drawn when it comes to things that I have no immediate knowledge of myself.

One further problem lies in the lack of consistent epic development that can maintain the reader's interest from start to finish. Such a development was present in '*Pandora's Box*' and '*The Sword of Damocles*'. Unlike the aforementioned, '*The Labours of Hercules*' deals with a number of areas, each of which certainly does contain an interesting development, but these developments do not form the same sort of coherent whole. To make these sections easier to read, I have been obliged to incorporate information from times that have also been discussed in other sections. This has fractured the overall chronology and the overall portrayal may appear a little confusing like a kaleidoscope. The fact that each section has become more independent will hopefully make up for this.

I have intentionally included a number of anecdotal sections that are not entirely inessential to the story; I think they facilitate the reading of a text that might otherwise have involved more than the desired effort, but I also think they help to give the reader an insight into the lives, temperaments and environment of the main people and thereby provide them with a better understanding of what happened.

I am once again grateful for the help I have received from many colleagues at the Swedish Radiation Protection Institute, particularly for the support and interest from its Director General, Lars-Erik Holm. The Radiation Protection Institute has made a financial contribution towards the publication of the book. Generous contributions have also come from the Riksbankens Jubileumsfond (the Swedish Foundation for Humanities and Social Sciences) and the Marcus and Amalia Wallenberg Foundation. Without these contributions it would not have been possible to publish the book.

I am also very grateful for the encouragement I have received through the interest shown in me by my *[Swedish]* publisher, Atlantis, as well as the expert help I have received from their editor Tomas Blom and the publishing company's production manager Björn Wahlberg. A special thank you goes to a number of people who have assisted me by giving points of view and advice and, where relevant, have also been kind enough to permit me to quote them. I would primarily like to mention Bo Aler, Hans Brunner, Arrigo Cignia, Anders Fröman, Olle Gimstedt, Mats Harms-Ringdahl, Arne Hedgran, Bengt Hultqvist, Karl-Erik Larsson, Stefan Lindström, Jacques Lochard, Ingemar Lund, Sven Löfveberg, Marko Ninkovic, Richard Osborne, Lars Persson, Jan Rydberg, Anneli Salo, David Sowby, Lauriston Taylor, Jack Valentin, and Rune Walstam (a good friend and colleague who is unfortunately no longer with us). All of this assistance has helped to improve the book. Any errors that remain will be down to my own good self. Finally, the biggest thank you goes to my wife, Marrit, whose incredible patience has endured while I have been writing this.

Sollentuna, February 2003
Bo Lindell

[*] Prof. Lindell refers to Johan Ludvig Runeberg's epic poem *The Tales of Ensign Stål* (1848 and 1860), depicting humble and dutiful soldiers of the Finnish war (1808-9) when Sweden lost its eastern territories to Russia.

1. THE OPENING CEREMONY

THE KYNDEL STRING QUARTET was playing the Allegro from Beethoven's 5th Symphony in A major and the invitation-only audience in The Institute of Radiophysics' new high voltage hall was listening attentively or impatiently, depending on each person's level of musical interest. Otto Kyndel[*] and his three colleagues sat on a high platform next to the big x-ray tube that signalled the right of the high voltage hall to exist. Below, behind the seven-metre-high voltage generator, sat the venerated guests, the foremost being H.R.H. The Crown Prince Regent *Gustaf Adolf* (1882–1973), who would become king of Sweden in six months' time.

Rolf Sievert[†] often said that one ought not to allow the best to be the enemy of the good although he seldom lived up to his maxim. In anticipation of this day, 8 May 1950, he had put in extra efforts to make the opening ceremony of the high voltage hall a real festival at which only the best was good enough. *Dagens Nyheter* had heralded the ceremony:

> On Monday, The Institute of Radiophysics at Karolinska Sjukhuset is opening a distinguished high voltage hall and an emanation laboratory. The new equipment will allow Professor Rolf Sievert and his colleagues to experiment to find a new and remarkable method of using x rays to treat deep-lying tumours. [The facility] enables extremely fast and intensive irradiation, the biological impact of which is to be studied. The intention is also to experiment on mice and small mammals because using the apparatus can achieve a similar effect to that of an atomic bomb explosion and they want to study its impacts.

I had been looking forward to the ceremony with mixed feelings. The high voltage hall had been my place of work and my area of responsibility since the day in the summer of 1948 when I, not yet in possession of a Master's degree in Engineering, had been employed by Sievert. At the time I thought the hall was enormous and up in control room I felt as though I was the captain on the bridge of an Atlantic steamer while looking down at the great cascade generator that would create voltages of up to 1.2 million volts.[‡] Bearing in mind the substantial sums that the plant had cost and the rapid results that Sievert evidently expected, I felt laden with responsibility and was considerably anxious about being the person who was carrying this responsibility virtually single-handedly.

Of the funds that Swedish Parliament had granted for atomic energy research in Sweden, the largest individual amount had initially gone to Sievert's institute. Sievert was a master of obtaining money for different projects. As well as grants from The Atomic Committee, The Knut and Alice Wallenberg Foundation had been the main support for the operation. The high voltage hall had been inexpensively built by obliging building contractor *Anders Dunder*. Significant contributions had also been made by *Sievert's Kabelverk*. Sievert had promised the military partners a facility that could imitate the gamma radiation from an atomic bomb with the help of an x-ray tube that would produce very intensive x rays

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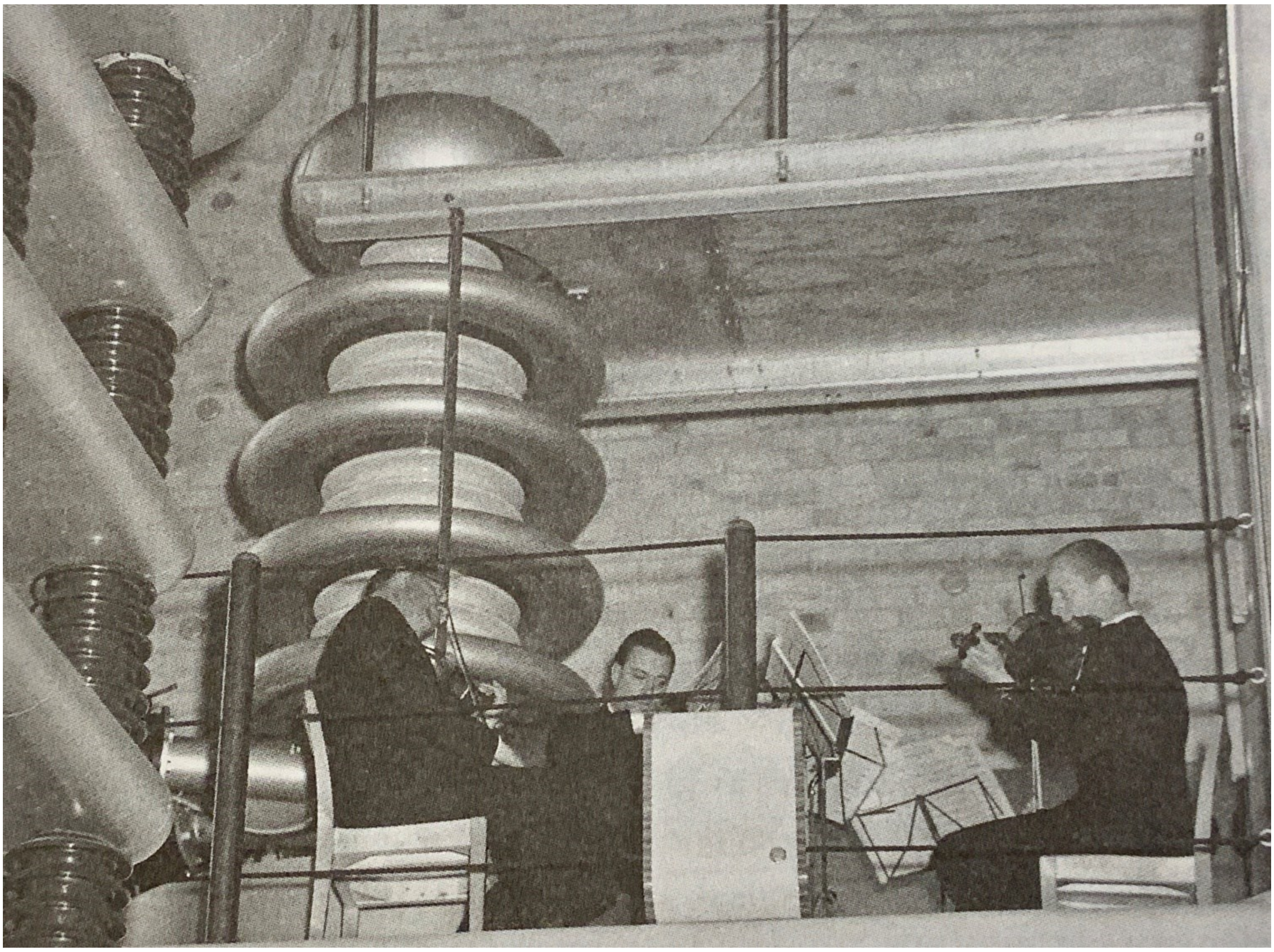
[*] The violinist and orchestral leader of the Swedish Radio Symphony Orchestra Otto Kyndel (1904–1983) had formed the Kyndel Quartet in 1941, a string quartet which would become one of the most distinguished in Sweden.

[†] The 'father of Swedish radiation physics' Professor Rolf Maximilian Sievert (1896–1966) was in charge of The Institute of Radiophysics next to Radiumhemmet in the grounds of Karolinska Sjukhuset [the Karolinska University Hospital]. He is the main figure in this book. The prequel to the current story has been recounted in my previous books, '*Pandora's Box*' and '*The Sword of Damocles*'.

[‡] The high voltage hall was described in detail in '*The Sword of Damocles*'.

for less than one hundred thousandth of a second. He had promised the civilian partners a facility that would be able to destroy cancerous growths inside the body far better than before.



The Kyndel Quartet playing next to the large x-ray tube at the high voltage hall's opening ceremony on Monday 8 May 1950. Photo: *Svenska Dagbladet*.

The high intensity of the x rays would make it possible to answer the question as to whether the equivalent intense radiation from atomic bombs could have any biological effect that was different from the one experienced from the use of x rays in the healthcare services. This idea was not unreasonable; our bodies do also have mechanisms at cellular level to repair injuries. The reasoning was that if these mechanisms required a certain time period, there might not be enough time for the repair processes to efficiently handle multiple injuries caused if the radiation energy were emitted for a very short period. An extremely educational book about the effect of radiation on human cells had recently been written by the British radiation biologist *D.E. Lea* (1910–1947) and it lent support to the hypothesis that the intense radiation would have a more powerful biological impact. Sievert had done some experiments himself with The Institute of Radiophysics' biologist *Arne Forssberg* (1904–1975) which pointed in the same direction. But we were not yet certain.

Designing the x-ray tube to not just facilitate biological experiments but also to be used for cancer treatment was an expensive idea of Sievert's. The idea was that the anode of the tube would be shaped like a truncated cone so that the non-existent tip of the cone lay outside (under) the x-ray tube. If the cathode of the x-ray tube consisted of filaments around the cone, the x rays that were formed when the electrons from the filaments hit the anode could fundamentally be expected to form a conical beam of light that would converge in an area immediately below the imaginary tip of the anode cone. That exact spot is where concentrated x rays would be created. Sievert thought that it would be possible to lay a patient beneath the x-ray tube and adapt the position so that the focused x rays would hit the tumour. The device was costly and impractical due to the required dimensions. The 'x-ray tube' consisted of a steel cylinder that was one metre in diameter and of the same height. Above this cylinder rose a tower of large porcelain isolators that were crowned by a metal lid which was covered with a rounded aluminium casing. A metal rod hung from the lid and through the porcelain tower, supporting the conical anode at the bottom in the middle of the steel cylinder. 144 filaments were fitted around the anode. The anode and the filaments obviously had to be in a vacuum, which meant that the air had to be pumped away from the cubic-metre volume that was surrounded by the steel cylinder and the porcelain isolators. Creating a high vacuum in this volume was technically speaking a considerable problem which we did succeed in solving purely by virtue of Sievert's workshop manager, engineer *Axel Berggren* (1901–1998), who was a particularly skilled technician and my closest colleague. My other colleagues in the work done in the high voltage hall were engineer *Gunnar Eklund* (1925–) and the older electrician *Johan Albert Sundquist* (born in 1892) who would go on to work with Sievert until almost the age of 71. The enormity of the project was demonstrated by the fact that the 144 filaments drew a filament current of 3 000 amperes, which was delivered by a 16-volt submarine battery placed in a special extension outside the high voltage hall. Enormous condenser batteries could be charged up from the cascade generator by means of a large boom that was lowered from the ceiling. When another boom was lowered between the condenser battery and the top of the isolator tower, a spark flew over to the x-ray tube with a bang and a brief pulse of electrons from the filaments was accelerated towards the anode, generating x rays.

My first assignment had consisted in working with Gunnar Eklund to solder the wires in the coupling plinths that would be positioned in the control room. After that, I would experiment to find out how the 144 filaments for the cathode of the x-ray tube would be fixed. By the time I was employed, Sievert had already obtained wolfram wires for that purpose; the problem was that the nickel cable shoes would become too hot if they were welded directly onto the wolfram wires. It was therefore a matter of experimenting with double welding with adequate cooling between the wolfram wires and the cable shoes. It was the biological effectiveness of the brief, intense x-ray pulse that interested The Atomic Committee, but Sievert wanted to kill two birds with one stone by also using the conical beam of light for tumour treatment. His enthusiasm and impatience meant that for this purpose, he had already obtained a clinical treatment table with different adjustment options long before the x-ray tube was ready to be tested. His enthusiasm had rubbed off on *Radiumhemmet's* little manager *Elis Berven* (1885–1966), who walked occasionally down to the high voltage hall to see how the project was coming on. Unfortunately, things did not go to plan; we had great difficulty in getting the x-ray tube to function at high voltages.

However, the difficulties were completely forgotten on that Monday 8 May 1950, with Sievert creating a ceremonious atmosphere. The Crown Prince and the most prominent guests sat in front of the high voltage generator, but in addition, every available nook and cranny had been accounted for as regards chairs for scientists from other institutions as well as Sievert's personnel; a total of 260 people had been invited.

Sievert spoke following the initial greeting from the Vice-Chancellor of *Karolinska Institutet*, *Hilding Bergstrand* (1886–1967). He described the medical radiophysics activities that he had built up over the past thirty years and the way in which the premises had grown from the first 5 square metres on Fjällgatan in Södermalm into the 850 square meters that the Institute of Radiophysics was able to utilise in the grounds of *Karolinska Sjukhuset* in 1938. And additional premises had now been added: the high voltage hall and the emanation laboratory. Sievert reminded everyone of the risks involved with the coming 'atomic age':

> From having no more than around 40 radioactive naturally-occurring elements available for use [*], it is now possible to produce more than 700 different ones, many of these in quantities which, where the biological impact is concerned, are thousands of times greater than the quantities that were previously available.
>
> Before 1945, a lethal x-ray dose at a distance of one metre could not be achieved even with the most powerful of devices until after several minutes of irradiation. The

[*] Sievert ought to have said 'nuclides' and did not literally mean 'elements'.

> equivalent effect can now be created with an atomic bomb in fractions of a second and several kilometres away.
>
> But even if you completely disregard intentional usage for the purpose of causing harm, the new means are dangerous. After all, we are dealing with rays whose presence cannot be detected directly with our sense organs and whose effects do not usually make themselves felt until a few weeks, months or even years later – and where genetic effects are concerned, maybe not until after a century has passed.

Following Sievert's address, Crown Prince Gustaf Adolf opened the new laboratories. The Kyndel Quartet then finished off with *Presto scherzando* from Haydn's symphony no. 34 in D major. The guests then took a tour around the institution in groups. Sievert was obviously the one who escorted the Crown Prince.

The laboratory, which was new in addition to the high voltage hall, was intended to 'milk' radon from a radium preparation. Radium (radium-226), whose activity (the number of radioactive disintegrations per unit time) is reduced to half over 1600 years through radioactive disintegration, is transformed into the radioactive noble gas radon-222 when it disintegrates. The radon has a half-life of 3.8 full days.

Along with the radon, helium is also formed and ejected with high energy. Such helium atom nuclei with high kinetic energy are called α-particles (alpha particles) and constitute α-radiation. Radium is thus α-emitting.

If you store radium in a sealed vessel, you can make use of the radon that is formed and pump it into a glass tube which can be sealed. Within a few minutes to a few hours, such a glass ampoule containing radon will also contain the short-lived radioactive daughter products[*] that are formed when the radon disintegrates. Since the lifetime of the daughter products is much shorter than that of radon, they will shortly afterwards be in radioactive equilibrium with the radon, i.e., undergo as many radioactive disintegrations per second as the radon. After a couple of hours, a radon preparation therefore contains the same activity of each and every one of the short-lived radioactive substances that are in the ampoule as from the radon itself.

The aim of producing glass ampoules containing radon was to have a preparation with the same penetration capacity γ-radiation (gamma radiation)* as the radium but with a shorter lifetime so that it became completely non-hazardous after a few weeks. The strong, γ-emitting radium is adorned with borrowed plumes: provided the radon remains along with the radium, the γ-radiation from radium comes mainly from the daughter product bismuth-214 (previously called radium C). The radium itself is only very weakly γ-emitting and the radon is only α-emitting.

The first and actual daughter product of the radon is polonium-218, previously called radium A. It has a half-life of around 3 minutes and therefore reaches approximately the same activity as the radon after quarter of an hour; it is then said to be in equilibrium with the radon. But polonium-218 is like radon-222 and radium-226, only α-emitting. However, its daughter product, lead-214 (radium B), emits both β-radiation (beta radiation, i.e., electrons with high kinetic energy) and γ-radiation. However, its γ -radiation has low energy. Lead-214 has a half-life of 27 minutes. It therefore is not in approximate equilibrium with the radon until a couple of hours later but, at the same time, its own daughter product, bismuth-214 (radium C), is built up to an equilibrium with the radon. This radium C brings with it the dominant γ-radiation, the one from radium preparations that has provided benefits in the healthcare services.

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[*] Actually an inappropriate expression since these nuclides follow one another in succession and therefore constitute a series of daughter, granddaughter, great-granddaughter, etc.

* γ-radiation is an electromagnetic radiation of the same type as x rays; the different types of radiation (α-radiation, β-radiation and γ-radiation) have been described in greater detail in 'Pandora's Box'. In this chapter, I am using the designations α, β and γ but after that will be writing out alpha, beta and gamma.

Nuclide:	Half-life:	Radiation:
radon-222	3.8 full days	α
⇓		
polonium-218 (radium A)	3.05 minutes	α
⇓		
lead-214 (radium B)	27 minutes	β, γ
⇓		
bismuth-214 (radium C)	20 minutes	β, γ
⇓		
polonium-214 (radium C')	0.00016 seconds	α
⇓		
lead-210 (radium D)	21 years	β, (γ)
⇓		
bismuth-210 (radium E)	5.0 full days	β
⇓		
polonium-210 (‘polonium’)	138 full days	α
⇓		
lead-206	stable lead	

It was Professor Failla* in New York who first had the idea that ampoules containing radon and its daughter products could be used for radiation treatment instead of radium. Such an ampoule would emit just as much γ-radiation per time unit as a radium preparation with the same activity as the radon. If, for example, you started with 1 gramme of radium in equilibrium with its radon and then pumped the radon over to a glass ampoule, the ampoule would contain radon with an activity of what was then said to be 1 *curie* (Ci). The old unit curie for the activity of a radioactive substance was defined as 37 billion disintegrations per second, as many as in 1 gramme of radium. Since the half-life of radon is only 3.8 full days, the activity after around two months would be only a few millionths of a curie, a non-hazardous quantity, and the glass ampoule that had contained the radon would then constitute no danger in the body of the patient.

Sievert had taken up Failla’s idea but he wanted a facility that would not be dangerous to the users since the idea did involve handling one gramme of radium, which was a significant amount. The task of constructing the pump device had gone to *fil. mag.* [MSc] *Agnar Egmark* (born in 1914), a very unusual man from Värmland who also served as radiation protection inspector under Assistant Professor *Sven Benner* (1900–1986) at the Institute’s Division C for the supervision of radioactive substances. *Svenska Dagbladet* had described the facility on 21 April:

> The device room, recounts Assistant Professor Benner, is separated from other premises by of one-metre-thick concrete walls but each small detail in the device can be observed from the control room through an installed periscope. So, the observer sits at the control panel, adequately protected throughout the procedure of producing the preparations. Radiation measurements have shown that, thanks to this system which has been automated almost as far as limitations allow, the work is significantly less risky than it is at similar facilities abroad.

* *Gioacchino (‘Gino’) Failla* (1890–1960), medical physicist in New York and a close friend of Sievert.

The new laboratory was called the ‘emanation laboratory’ after the old name of ‘radium emanation’ for radon. The facility was probably the first remote-controlled one in the world. On 1 March 1950, Sievert had employed a young physics student, *Inger Ragnhult*, as the ‘radon pumper’ and his skilful colleague, engineer *Rune Walstam* (1923–2002), had also assisted from time to time. In spite of all the precautionary measures, radon leaked out and those taking care of the pumping had to take a walk in Hagaparken afterwards to get rid of the radon from their lungs and clothes and allow the short-lived daughter products to wear off. In 1952, the newly-employed successor to Benner, Assistant Professor *Arne Hedgran* (1921–2009), ended up taking over the responsibility.

The use of radon rather than radium for medical purposes was something that did not last long owing to the appearance of artificially-produced radioactive nuclides. However, the radon from the emanation laboratory was also used by The Royal Telegraph Administration [Televerket] to search for leaks in cables, although this was not satisfactory from the radiation protection point of view. Nor was it appropriate for The Institute of Radiophysics to sell radon and be responsible for the safety aspect. The laboratory therefore did not last long. Hedgran convinced Sievert to close the operation down. For this to take place in the safest possible way, the Amersham Radiochemical Centre in England was contracted to send over experts to disassemble the facility, which went well. The radium would be used for radium-beryllium sources for research institutions. Professor *Jan Rydberg* (1923–2015) has said that he worked with this radium at the Defence’s research establishment.

The concrete structure around the facility proved to be so strong that concerns were raised during the final demolition of the building. The contractor had difficulty fulfilling his tender. It was more difficult to demolish than they had expected and they were finally forced to use explosives.

However, on the open day on 8 May, Sievert had more than the high voltage hall and the emanation laboratory to show the Crown Prince Regent, and something that was particularly close to his heart was the experiments he carried out using with fruit flies (the species *Drosophila melanogaster*). It was now undisputed that ionising radiation could cause mutations, and the fruit fly was a popular trial subject for the geneticists. Sievert had asked himself whether the risk of hereditary injuries also applied at very low doses of radiation. If so, could the reduction or elimination of the naturally-occurring radiation cause a noticeable effect in the form of reducing the risk? However, at the same time, it could not be precluded that a specific amount of radiation was vital. If so, lowering the reduced radiation environment would have a harmful effect.

In order to investigate this, after having already started experiments in the 1930s, Sievert had built up iron radiation screens with help of Rune Walstam inside which fruit flies were bred in a reduced radiation environment. At specified times, a suction device sucked up the bred flies to a sticky fly paper to which they stuck. The fly paper was in the shape of a round plate which rotated so that different sectors corresponded to different times. Afterwards, it was possible to calculate the number of flies that had stuck to each sector and compare that with the percentage of breeding in the natural radiation environment.

At the top of the device lay a heavy metal block which must have weighed 50 kg and which acted as a bung in the cylindrical cavity in which the flies were bred. It could be lifted up with the help of a steel cable which was operated by an electric motor. When the block was lifted high enough for you to be able to look down into the cavity, the motor was stopped by means of an arm from the block pressing on a circuit breaker. When Sievert and Walstam demonstrated the facility to Gustaf Adolf, Sievert happened to have placed a glass jar on top of the block, which meant that the block overturned when the glass jar collided with an obstacle and was crushed. This also meant that the current was not interrupted, and that there was a risk of the lifting device giving way and the heavy block falling back down to the radiation screen. And it just so happened that beneath the block was the head of the Crown Prince who was showing curiosity by looking down into the opened aperture. Luckily, Walstam realised the risk and had the presence of mind to quickly interrupt the current before there was a chance of an accident occurring.

Unfortunately, no definite conclusions could be drawn from these experiments. The effect of the radiation reduction, positive or negative, was not sufficiently significant to be proven.

On that memorable day in May 1950, the Crown Prince was also shown a fourth, less spectacular activity. In the high voltage hall there was an opening in the floor and a staircase which led down to a windowless basement room which was quite simply referred to as 'The Pit'. This is where another of Sievert's engineers spent his time, a clever instrument maker called *Bengt Håkansson* (1925–). Despite Sievert's great expectations for the high voltage hall, the emanation laboratory and the fruit flies, it was the activity in The Pit which would lead to the research and the discoveries which, along with the construction of the condenser chamber*, would lead the name 'Sievert' to become well-known throughout the world. In The Pit there was a device for measuring the γ-radiation that was normally emitted by the human body.

Our body contains various naturally-occurring radioactive substances, the most important of which are potassium-40 and radium-226. Radium, like calcium, is collected in the skeleton and since we do not exhale all of the radon that is formed there when the radium disintegrates, the presence of radium means that we also have the γ-emitting bismuth-214 (radium C) left in our body.

Potassium exists primarily in our muscles. Naturally-occurring potassium occurs in the form of a mixture of the isotopes potassium-39, potassium-40 and potassium-41. The two stable isotopes, potassium-39 and potassium-41, occur in percentages of 93.3 and 6.7 respectively. There is also a very small share (0.012 %) of the radioactive potassium isotope potassium-40. It is very long-lived; the half-life is 1.27 billion years.

The naturally-occurring radioactive substances are either so long-lived that they have been there since our planetary system came about maybe five billion years ago or are constantly newly formed in the atmosphere through nuclear reactions caused by cosmic radiation. Radium itself is not sufficiently long-lived but since radium-226 is part of a decay chain that is started by uranium-238, the existence of the radium is dependent on the existence of uranium-238, which has a half-life of 4.6 billion years. By contrast, potassium-40 itself has a sufficiently long half-life to have been there since the Earth was created. Potassium-40 can disintegrate in two different ways. The most common (89 %) is β-disintegration (disintegration while emitting electrons) to calcium-40:

$$ {}^{40}_{19}K \rightarrow {}^{40}_{20}Ca + {}^{0}_{-1}e $$

The other disintegration method (11 %) takes place through capturing electron and transforming them into argon-40:

$$ {}^{40}_{19}K + {}^{0}_{-1}e \rightarrow {}^{40}_{18}Ar $$

This is the disintegration that is accompanied by γ-radiation when the surplus energy is emitted. Argon-40 is a stable nuclide and the most commonly-occurring argon isotope. The body of an adult human contains around 140 grammes of potassium, which corresponds to an activity of around 4 000 becquerel (Bq)†. Mainly β-radiation is emitted at the time of disintegration, but γ-radiation is emitted in 11 % of the disintegration of potassium-40, i.e., that which occurs with the capture of electrons.

In The Pit, Sievert had had a device built to measure the γ-radiation from the human body, a 'whole-body measurer'. The radiation could be demonstrated through the ionisation that it created in air or other gases in what were known as ion chambers, enclosed volumes in which the ions that were formed could be collected using an electric field. To increase the measurement sensitivity, Sievert ensured the ion chambers consisted of ordinary pressurised gas tubes filled with nitrogen and carbon dioxide at an atmospheric pressure of 20 (around 2 000 kilopascals (kPa)).

The measurement device consisted of twelve such pressurised gas tubes arranged to form a cylinder around the one to be measured. The person was placed on a stretcher that could be pushed into the circle of pressurised gas tubes. The measurement took a long time and anyone not suffering from claustrophobia could take the time to switch off and sleep for a while. It was in this device that Sievert had once asked me to do some mental arithmetic so he could see whether my brain was radiating while I was thinking. Of course it was an unreasonable idea, he admitted, but those were the sorts of ideas that led to Nobel Prizes!

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* See 'Pandora's Box'.

† 1 becquerel (Bq) is now the unit for activity and refers to 1 nuclear disintegration per second.

Rolf Sievert demonstrates his fruit fly experiment to the interested Crown Prince Gustaf Adolf. Photo: Svenska Dagbladet.

Sievert was primarily interested in the level of radium in the body. With the same γ-activity, radium with its α-radiation would lead to a much higher dose of radiation than potassium-40 with its β-radiation. It was important to know the natural dose in the body, Sievert pointed out, since when the International Radiation Protection Commission (ICRP) was due to meet in London in 1950 for the first time after the war, it would be giving its opinion on recommendations for dose limits for those who worked with radiation. It ought not to lead to any serious risk for the person who received an extra dose of radiation during his work if the dose was not much greater than the variations of the natural dose, the ‘background radiation’.

The big problem was the different information that was received from those who had previously tried to measure the level of radium in the body. Radium had then been separated from the ashes of cremated people. Such a study had been carried out by *John Hursh* (1907–2003) and colleagues in Rochester, and another by *A. Krebs* at the Kaiser Wilhelm Institute for Biophysics in Frankfurt-am-Main.

In 1939 and 1942, Krebs had reported results which indicated that the natural level of radium in the body could be as high as tens of nanogrammes (i.e., tens of billionths of grammes). In 1950, Hursh had pointed out that the highest individual level published by Krebs was 40 % of the maximum permitted radium level recommended in the bodies of radiation workers at the time.

Krebs’ disturbing result led Hursh and his colleague *Allen Gates* to take measurements of the ashes of 24 cremated patients from the University of Rochester Medical School. The results they published in 1950 showed levels of radium that were just one hundredth of what Krebs had measured, an average of

0.12 nanogrammes. This was the discrepancy for which Sievert sought an explanation. Who was right? Krebs or Hursh? Or both?*

Using his pressurised gas device, Sievert was able to measure the intensity of the gamma radiation but he could not directly determine which substance in the body it came from, which is something that is now possible with the help of scintillation counters. So, to determine how much came from radium, he firstly had to find out how much came from potassium-40. For this purpose he had two water-filled rubber dolls made in the shape of bodies, called August and Augusta. To these he added potassium in the same quantity as in the human body, i.e., 140 grammes. By subtracting the measurement results he obtained from measuring August or Augusta from the result of measurements from people, Sievert was able to obtain a net value that he attributed to radium (or, should I say, radium C).

Sievert's measurements were disrupted by the cosmic radiation that could create showers of ionisations that affected the measurement result. He therefore attempted to screen off the cosmic radiation as far as possible. Such an attempt involved his having to erect a colossal cylindrical tank filled with water on the floor of the high voltage hall next to The Pit. A vertical cylindrical tube was attached to the centre of the tank and the people who were to be measured were lowered into said tube using a telpher in the ceiling. It was not easy to find measurement objects since the person to be measured barely fitted into the tube and could easily be affected by feelings of panic. The device was an emergency solution that was not particularly efficient.

The case was not improved by the advice that Sievert received from the Electrolux Professor *John Tandberg* (1896–1968), a man who was known for his tendency towards practical jokes. Sievert had mentioned to Tandberg that he was worried about the water in the big tank causing corrosion and possibly also the formation of algae. Tandberg, who was a recognised corrosion expert, suggested that this could be prevented by spreading a layer of plastic balls over the surface of the water. On his advice, Sievert purchased a bag of plastic balls and got Bengt Håkansson to climb up onto the tank to sprinkle the balls over the surface of the water. But the balls refused to float - they sank quickly to the bottom. Sievert sighed. 'If only I knew whether Tandberg had made a mistake or whether this was yet another one of his jokes!'

Bengt Håkansson then came up with an idea that Sievert thought was splendid.† There ought to be the space that was protected against the cosmic radiation by at least 50 metres of rock at the Henriksdal treatment works which was blasted into a mountain outside Danvikstull in Stockholm. On 26 January 1951, building contractor Anders Dunder tendered for a radiation-protected Laboratory inside the rock chamber. He described the work as:

> Demolition of existing timber building, cleaning of the rock surfaces. Moulding, formwork and reinforcement of foundations for water tanks. Transportation of these from the railway station to the workplace. Insertion, rust protection treatment and arrangement of the same plus filling with water. Iron construction to support water tanks. Masonite walls and framework to be erected in the room. The masonite surfaces will be painted.

On 30 January 1951, Sievert requested funds for the facility from The Atomic Committee. He then contacted *Stockholms Gatukontor* [the City Street Office], who kindly leased a section of a tunnel for the laboratory on 1 March.

The cosmic radiation had now been heavily reduced. All that remained was to screen off the gamma radiation that came from the rock walls. The radiation caused the ionisation of 10 ion pairs per cubic centimetre and second in the laboratory. In normal buildings, Sievert had shown between 3 and 30 ion pairs per cubic centimetre and second, 2.5 of which were from the cosmic radiation. In the Henriksdal

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* Hursh later carried out a comprehensive study of the level of radium in drinking water in the USA and found an average value of 0.04 picocuries per litre (pCi/l), i.e., by present-day measurements 0.0015 Bq/l. Krebs, who had moved to the USA, had stated several thousand times greater a value for the drinking water in Frankfurt. Measurements of drinking water in Sweden have usually shown radium levels of less than 0.1 picocuries per litre.

† Clearly stating in his first account of the Henriksdal Laboratory that the idea was Håkansson's was characteristic of Sievert's lack of desire for prestige.

Laboratory, the cosmic radiation was no more than a few hundredths of an ion pair per cubic centimetre and second.

According to Sievert's calculations, in order to lower the ionisation from the gamma radiation to the same value as that from the cosmic radiation, the measurement device needed to be surrounded by 1-metre-thick water tanks and then hold water that did not contain any disruptive radioactive substances. Sievert feared that the Stockholm water could be unnecessarily radioactive. He had been told that the water in the Thames had only 1/50 as many disruptive radioactive substances as water from Mälaren. The determined Sievert immediately ordered water from the Thames without hesitation and succeeded in getting Stockholms Rederi AB Svea to bring home 180 cubic metres of this water in the fresh water tanks of its London boats. He also persuaded *Mjölkcentralen* (Swedish milk-producing centre) to transport the water to Henriksdal in its tankers (the water was acceptable as drinking water). Finally, Sievert got the Stockholm fire service to draw hoses from the tankers down to the laboratory in the rock and there fill the screened cisterns with the help of its sprayers. Sievert arranged all of this in a short time by simply lifting the telephone receiver – possibly his most important research tool – a few times.

Thanks to these measures, the ionisation in the air that came into the laboratory became as low as 0.08 ion pairs per cubic centimetre and second. This would make it possible to measure gamma radiation from quantities of radium measuring 1 nanogramme in one single measurement and down to 0.2 of a nanogramme for repeated measurements. It ought therefore to be easy to observe Krebs' mean value of 14 nanogrammes.

Sievert had been in close contact with Professor *F. W. Spiers* (1907–1993) in Leeds for a while. The two competed to be able to measure small quantities of radium in the human body. Sievert published his first results in 1951. Along with the recently-employed *Bengt Hultqvist* (1927–2019), he also began measuring the intensity of the gamma radiation outdoors. Sievert and Hultqvist's first joint publication about the variations of gamma radiation was issued in 1952.

On 26 November 1951, Spiers, who had not had access to any rock cavern, wrote to Sievert:

> We had the equipment going with seven ion chambers during the summer. The local γ-radiation background was reduced to approximately 5 % inside the water screens and the background from the cosmic radiation was slightly reduced in that the soft component was removed from the water tank (30 inches). The presence of an inactive phantom in the measurement position reduced the background by an amount corresponding to 15 mμg [millimicrogrammes, i.e., nanogrammes] (I am writing this from memory) and this effect originated mainly from the [reduction of] the ionisation in the chambers beneath the phantom. We then studied the variation in the background; everything apart from 1–2 % of the background could be balanced by β-radiation activity in an [illegible]-made compensation chamber. The remaining background current was measured by integrating it over 15-minute periods. Fluctuations were observed that were several thousand times the statistical variations of the device and these (fluctuations) appeared to originate from cosmic radiation – probably in connection with solar flares, magnetic storms, etc.

So, Spiers had concerns that Sievert had escaped by moving his measurement device into the rock cavern at Henriksdal. Sievert was soon able to show that the gamma radiation from the human body – at least where Swedes were concerned – came mainly from potassium-40 and not radium. He had thereby confirmed that the results reported by Hursh and Gates were more representative, at least for Sweden, than the measurement values from Frankfurt. With the help of Bengt Håkansson, Sievert took extensive measurements from different groups: men, women, children and athletes (!), and was able to show that the amount of potassium-40 was greater in men than in women and the greatest of all in the athletes – potassium exists primarily in the muscles. This meant that the Nobel Prize winner *George de Hevesy* (1885–1966), a welcome guest at Sievert's institute, bantered: 'Yes, old Sievert … he's now shown that men are more muscular than women!'

2. THE INSTITUTE OF RADIOPHYSICS

LET US GO BACK A FEW YEARS in time. In 1949, the Nordic Society for Medical Radiology had celebrated its 30-year anniversary with its 16th Congress in Stockholm under the chairpersonship of Elis Berven. The end of the first half-century was now approaching and a new period with more modern equipment and new methods for both diagnostics and therapy. The tragedies and hardships of the Second World War had now passed and, to quote the Society's book of memories (Unné, 1984), it was now the case that 'the many restrictions vis-à-vis travel options had been alleviated and the economic situation in the Nordic countries had also really improved …'.

The minutes from the meeting give an idea of what was going on in Sweden:

> *Radiumhemmet*, The Institute of Radiophysics and its recently-built high voltage laboratory and the Institute of Pathology at *Karolinska Sjukhuset* were demonstrated to the Congress participants. A total of 65 lectures were held, 22 of which concerned therapy.
>
> *E. Berven* gave an inspired account of King Gustaf V's Jubilee Clinic which, as well as *Radiumhemmet*, includes the Institutes for Radiopathology and Radiophysics. *J. Heyman* gave points of view on the presentation of radiotherapy results. *R. Sievert* discussed a new principle for x-ray treatment involving appropriately designing the focus of the x-ray tube to obtain the maximum possible advantages. The 800 kV tube used in this connection was demonstrated to the Institute of Radiophysics, as was the equipment for measuring the level of natural radium in the human body. It permits the measurement of gamma radiation from radioactive substances of quantities corresponding to less than 0.005 of a microgramme of the total quantity of radium in the body. *H.L. Kottmeier* discussed different aspects of the radiation treatment of *colli uteri* cancer (carcinoma of the uterine cervix). Next followed an account of radiation treatment methods and results at *Radiumhemmet* using large amounts of material. *O. Halberd* talked about hypopharyngeal cancer, *O. Morales* about oesophageal cancer and H. Ahlbom about malignant testicular tumours and malignant lymphogranulomatosis, *B. Sylvén* talked about malignant melanomas and *G. Forssman* about giant-cell tumours in the skeleton.*

In 1950, Rolf Sievert's Institute of Radiophysics had developed from the few square metres of experimental space at his disposal on Fjällgatan in 1922 into the hybrid of a state-controlled body, research institute and academic institution which had gradually resulted from the 1941 Radiation Protection Act. However, the Institute of Radiophysics was not yet a radiation protection authority. The authority function was held by the Medical Board, which formally issued the permits required by the Act for work with radiation sources and possession of radioactive substances. The Medical Board was also responsible for the publication of key circulars such as those concerning medical examinations in radiological work (1950) and on terms for working with isotopes (1951).

Sievert's institution was the Medical Board's executive body and prepared the cases that required decisions. The Institute of Radiophysics was also responsible for the supervisory activities that were required. Some of the personnel were therefore national radiation protection inspectors.

The instruction that followed the Act prescribed that the Institute was to carry out scientific research within radiophysics and biophysics within the areas that could be of importance to the implementation

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* *Colli uteri* cancer = carcinoma of the uterine cervix, hypopharyngeal cancer = cancer of the pharynx, oesophageal cancer = cancer of the oesophagus and testicular tumours = tumours in the testicles.

of the Act. The instruction decreed that the Institute should have three control departments for its supervisory activities:

> […] a department for the control of x-ray facilities for diagnostics and for technical and scientific purposes, a department for the control of x-ray treatment departments and a department for the control of protection devices for radioactive preparations and for examining and assembling such preparations […]

Sievert called these control departments A, B and C. He had employed ph. lic. *Matts Helde* (1910–1999) as the person to take charge of department A, someone who in 1940 had entered into the military physics preparedness for which Sievert took the initiative after the war broke out. Department B was managed by Dr. of Philosophy *Robert Thoraeus* (1895–1970) who was Sievert's first academically-educated colleague. Thoraeus was originally a Master of Engineering specialising in ship building but had been an assistant to *Manne Siegbahn* (1886–1978) before Sievert employed him to deal with the mobile measurement activities at the hospitals in 1927. Finally, department C was led by Dr. of Philosophy Sven Benner who had come to Sievert from Stockholm University in 1930 where he had been a colleague of the then Assistant Professor *Gustaf Ising* (1883–1960). Hans Weinberger writes tellingly of Benner (Wein, 1990): 'Precision and the in-depth analysis of problems were what characterised Sven Benner. He was something of the physics laboratory's unofficial mathematician, and his substantial knowledge of languages made him almost indispensable to his colleagues'. Benner and Thoraeus were opposites. Thoraeus was careful to guard his territory, often with a certain amount of boorishness. Benner was so quiet and discreet that Sievert sometimes jogged him and told him he should make himself heard more.

As well as the control departments for the supervisory activities, Sievert's institution was also to have a biochemical-biological laboratory. This would be led by Dr. of Philosophy Arne Forssberg, whom Sievert had already employed in 1929. In his laboratory, Forssberg had one of the smallest work rooms imaginable; there was barely room for his desk and two chairs. Going there to visit was enjoyable. Forssberg was a captivating cooperation partner in most subjects. His own research included the effectiveness of chemical protection substances such as the amino acid cysteine on chemical processes in cells. His 1943 doctoral thesis had discussed the impact of radiation on a very radiation-sensitive algal fungus *Phycomyces Blakesleeanus*.

The professorship in radiophysics also came with a certain amount of teaching, something which Sievert did not particularly enjoy. He was therefore happy to transfer to his colleagues the lecturing on courses at *Radiumhemmet* where the participation was voluntary and the audience was primarily made up of *Radiumhemmet's* doctors. Since I enjoyed teaching and wrote a compendium in radiophysics at an early stage, mainly for the purpose of teaching myself, Sievert had already allowed me to take over his lecturing duty from 1950.

In the following years to 1955, he formally took leave of absence from his professorship and I was appointed as his replacement. I also held lectures at the Stockholm Workers' Institute, a rewarding task because of the great interest demonstrated by the audience.

Sven Benner turned 50 on 10 January 1950. The Institute of Radiophysics had a collection to which Sievert obviously contributed generously. The money collected was used to buy a large radio gramophone, a piece of furniture which, with television about to make an appearance, would soon be superfluous. Sievert had the idea that it would be appropriate for all of the personnel to record a gramophone record for the radio gramophone in honour of Benner. Quite by chance, I had taken part in a competition arranged by *Radiotjänst* (the Swedish public radio service) during my first two years with Sievert. You read out the start of a short story over the radio and the audience was invited to complete it. The result was very successful in my case – I came second the first time and won on the two following occasions, after which the competition came to an end. Sievert had enjoyed my contributions, one of which was about a radiation-measuring Professor who caught a spy off guard in the mountains. He now asked me to write a script with replies for everyone, with reference to Benner's excellence. I agreed and wrote replies that I thought were typical of those involved. Sievert read the draft and exclaimed: 'Well, that's good, bloomin' good - as good as you damn well get, and has that wicked touch!'

So we went to Svala & Söderlund in *Konserthuset* (the Stockholm Concert Hall) and everyone read out their contributions. I had Sievert rushing around the institution asking 'How's it going then, how's it going?!' and had others using their favourite phrases, such as Thoraeus with 'I hardly think so.' Different problems accumulated but the omniscient Doctor Benner solved them all. His indispensability was such that people reckoned he ought to have been forbidden to leave the institution for business trips and his holiday days ought to have been divided among the other radiation protection inspectors. The latter was a hidden gibe bearing in mind the extended holidays that the inspectors had, which in my opinion were justified by radiation risks that did not exist for them.

The war and Sievert's involvement in military physics had delayed the development of the radiation protection supervision, and the arrival of the Swedish National Defence Research Institute (FOA) in 1945 had been associated with discord and conflict. The Defence's Research Committee which was established in February 1943 with Manne Siegbahn as chair had looked at the way in which the Military Physics Institute (MFI) run by the physicists could be coordinated with other military technical operations.* In December 1943, the Research Committee had submitted to the government a proposal for the organisation of a Defence Research Establishment (the FOA). The two physicists (Siegbahn and Sievert) in the Research Committee had made reservations regarding this proposal. *Hans Weinberger* writes in his Sievert monograph (Weinberger, 1990):

> The fundamental conflict consisted principally of matters regarding the control and use of the research for military purposes. The opposition between scientific freedom and military structure had lain latent for most of the MFI's period of activity. The physicists, headed by Sievert, had succeeded in enthusing the politicians and the military enough to receive money to run largely undisturbed operations. When the operations were then to be incorporated into a larger organisation, they were obliged to assert the scientific freedom so that the concept of the MFI would not dissolve into thin air.

Sievert, who was used to getting his own way, looked on the military with disapproval. His criticism reflected a struggle for power. He wrote bitter words (Agre, 1989):

> The experiences from The Defence's Research Committee quite clearly show that military specialists do not comprehend the research; where there are divided opinions among the civilian members, they agree with and take the line that they, in their position as non-specialists, think best suits the military requirement of being able to take control.

The fact that Sievert felt ignored and disparaged is shown by a few lines in a draft letter of 14 November 1944:

> Why do the technologists and industrialists and, in recent times, also some of the military take every opportunity to cast suspicion on the person who started and led the military physical operations without any thought for his own potential benefits?

What Sievert did not realise was that industrialists and the military were frightened by his enormous energy and willingness to control and arrange and get things done his own way.

In 1950, when Sievert had opened his high voltage hall with pomp and circumstance, Elis Berven retired and was succeeded by *Hugo Ahlbom* (1900–1952) as head of *Radiumhemmet*. In November of that same year, *Gösta Forssell* (1876–1950), 'the father of Swedish radiology' and the first head of *Radiumhemmet*, passed away. It was the end of an era. Sievert, who respected both Berven and Forssell, these being the people who had given him considerable support, was troubled. He could no longer feel at home at *Radiumhemmet*. Ahlbom, however, who was in poor health, did not have the strength to cope with any major changes. Berven still had a big work room and still had considerable power since he still had considerable influence on the research funds. A memorial room with Forssell's furniture and other

* See 'The Sword of Damocles'.

belongings had been set up. The past was kept alive but the understanding of this gradually lessened. When *Sven Hultberg* (1907–1965) succeeded Ahlbom, the memorial room was cleared out and Hultberg did not particularly approve of Berven's presence in the building. On one occasion, I heard his irritated voice say that if Berven was not given access to any space at *Radiumhemmet*, he could always 'put up a tent on the lawn' outside.

However, in 1945, the atomic bomb had made Sievert forget his disappointment and opened up new fields for his abundant initiative. The high voltage hall was the grandest project but the measurements of the gamma radiation from the human body would prove to be the most rewarding. However, Sievert had had enough of the military physics research. There is nothing to indicate that he was interested in the nuclear weapons research at the FOA. On the other hand, he did devote himself heavily to research into the consequences of nuclear technology. Knowledge of the natural level of the radioactive substances in the human body was important to the assessment of the significance of radioactive contamination. It was fortunate that the most important of the naturally-occurring radioactive substances, radium and its disintegration products and potassium-40, could easily be measured owing to the gamma radiation they emitted.

As of 1955, Sievert's measurements started to be disrupted by the radioactive substances from the atmospheric nuclear weapon tests. However, they aroused international interest among people such as Sir *John Cockcroft* (1897–1967), head of the British nuclear research station at Harwell. In January 1952, Cockcroft wrote to Sievert and asked if he could send any scientists to Stockholm to discuss temporarily abnormal measurement results. Sievert refused and proposed that they wait until his equipment was fully up to scratch. However, in May, instrument physicist *Denis Taylor* came to The Institute of Radiophysics from Harwell for discussions with Sievert. And in October that same year, Sievert discussed exchanging measurement results with Harwell's head of radiation protection *W.G. ('Greg') Marley.*[*]

The unconventional Sievert was not to make do with usual laboratory measurements. In November 1950, he had requested a tender for testing from Swedish Aero, who gave a price of 200 Swedish kronor per hour[†]. One week later, Sievert applied for 3 000 Swedish kronor from The Atomic Committee for radiation measurements from aircraft, and in February 1951 he took measurements from aircraft over *Swedish Skifferoljebolaget's* [the Swedish Shale Oil Company's] radioactive slag heaps following the shale mining at Närkes Kvarntorp were he had previously sent Rune Walstam to take measurements on the ground. Sievert took the possibility of a nuclear war very seriously. He discussed preparedness measures at an early stage and in April 1951 proposed that they establish a special preparedness group. His nationwide measurement stations to detect radioactive contaminations early on are described in Chapter 13.

In no. 24 of *Swedish Läkartidningen* in 1951, a number of articles were entered under the common heading 'Radiation injuries, caused by atomic bombs and radioactive weapons'. Sven Benner wrote about 'Radiation from atomic bombs and protection options'. Arne Forssberg's article was called 'Radiation injuries caused by radioactive [!] radiation'. Finally, Sievert wrote about 'Radiation measurements during times of war'.

My first assignments for Sievert were purely practical, but I was still eager to learn something about radiation physics – radiophysics was not yet available as a subject at any Swedish university. With Sievert's consent, I spent a great deal of time in the library, which was still Sievert's private library and which was located in an extension from the staircase, a room of 11 m*4 m connected to the main building through an eight metre-long corridor with glass-doored instrument cabinets along the walls.

In the middle of the library stood black tables, which together formed one single large table area. This is where the institute meetings were held each week with Sievert as chair. The chairs around the table

[*] During the war, Marley, who was a well-known profile within international radiation protection, busied himself with high-speed photography and, because of this, ended up participating in the Manhattan Project in Los Alamos in 1944. In 1946, he came to Harwell which was an abandoned airfield at the time. In 1948, he formed the Health Physics Division there, which he headed. He later became head of the Radiological Protection Division of the UK Atomic Energy Authority's Health and Safety Branch. He became Assistant Director of the NRPB in 1971 just after the NRPB had been formed, with responsibility for Research and Development. He retired in 1973.

[†] To provide a context, in 1925 one British pound corresponded to 17.99 Swedish kronor, in 1935 to 19.40 kronor, in 1945 to 16.95 kronor, and in 1955 to 14.49 Swedish kronor. The corresponding exchange rates for 1 US dollar were 3.73 Swedish kronor in 1925, 3.96 kronor in 1935, 4.20 kronor in 1945, and 5.18 Swedish kronor in 1955.

were also black, as were all the bookshelves; Sievert's colour blindness meant that he always chose black or grey for furniture and instruments to be sure that he visualised them in the same way as everyone else.

The library was well-stocked for such a small institution. It included the most important major radiological magazines: *British Journal of Radiology*, *Strahlenterapie, American Journal of Roentgenology, Fortschritte a.d. Gebiete der Röntgenstrahlen, Radiology* and *Acta radiologica* and the newcomers *Nucleonics* and *Health Physics*. The stock of books was more limited but there were several useful handbooks such as Otto Glasser's *Medical Physics*, *Handbuch der Experimentalphysik*, *Handbuch der Physik*, Mattauch and Flammersfeld's *Isotopenbericht* and Landolt-Börnstein's *Physikalisch-Chemische Tabellen*. The monographies included Compton and Allison's *X-rays in Theory and Experiment*, Goodman's *The Science and Engineering of Nuclear Power*, Holthusen and Braun's *Grundlagen und Praxis der Röntgenstrahlungdosierung*, Lapp and Andrew's *Nuclear Radiation Physic*s and Pollard and Davidson's *Applied Nuclear Physics*.

In spite of access to the library, I initially felt isolated in terms of knowledge. What was at that time a clear generation gap made it difficult to discuss problems with the older people, who also did not have the time. Sievert also had no interest in theoretical matters, although he was happy to discuss instruments, radiation protection matters and international radiation protection cooperation. He was usually a good listener and was generous with useful advice. The one older person with whom I had the best contact was Arne Forssberg, who was very interested in discussing every matter that was linked to biological problems.

A clear improvement occurred when, in 1951, Sievert employed Bengt Hultqvist for the research into the natural radiation environment, and when Arne Hedgran from the Nobel Institute succeeded Sven Benner after defending his thesis in 1952, with Sven Benner having moved to Gothenburg. A little later on, the circle of cooperation partners was widened with the addition of Rune Walstam when we found common interests in medical physics.

In April 1951, a library committee set up by Sievert reported its conclusions on the future of the library. The committee consisted of Benner, Forssberg and Lindell. It proposed that The Institute of Radiophysics take over 15 of the library's 32 journals owned by Sievert. The Committee also enquired about rules regarding who should be able to use the library. This aroused an animated discussion at the institution's conference on Saturday 28 April. Persons present included Sievert as chair and Messrs Benner, Forssberg, Helde, Larsson, Lindell, Lorentzon, Thoraeus and Wahlberg and unusual to say the least, one woman, Inger Ragnhult. *Lars-Eric Larsson* (1920–1997), *Lars Lorentzon* (1905–1980) and *Thor Wahlberg* (born in 1914) were radiation protection inspectors under Matts Helde at control department A. Inger Ragnhult, who was employed for work at the emanation laboratory from the start, had become Fil. Mag. [MSc] and first assistant at The Institute of Radiophysics on 1 February. She would have liked to have been radiation protection inspector, but this was refused by the sometimes conservative Sievert, who thought that it would be inappropriate to have women on the supervisory trips for ethical reasons.

The discussion was recorded on an 'Agaphone', a magnaphone which preceded tape recorders; the contribution has not been edited but has been reproduced verbatim. It illustrates the gulfs between the academics and non-academics at the time. I am reproducing it since it gives you a keyhole view of the situation as it was more than fifty years ago.

SIEVERT: It's been suggested that the library should remain open to anyone and everyone at the institution. I think it should be open to … that the library should be open only to the academics and if others are to … of those who work at the institution are to do library studies here, this must be at the direct request of those in charge of the department or also after being given special permission – they must ask whether they can go through something here for a particular purpose. That's my opinion because we have a large number of engineers here who have lower-level technical qualifications. If everyone's able to come to the library and sit here they'll stay here for ever and the practical work will suffer. I think it's absolutely unworkable. On the other hand, if - for example with Forssberg we have a load of people who are less technically qualified with Forssberg as well, two engineers - if he wants them to look at something, all he has to do is ask them to do it, but as for them coming here to sit and read on their own initiative, I think that'll greatly jeopardise the work.

FORSSBERG: They won't do that.

SIEVERT: No, they won't do that, so there's no problem with us settling the issue because otherwise these young boys that we're taking in here, who we have here now, Eklund and Håkansson, who are also less technically qualified, they'll also think they can do it so we have to draw a line at the academics. I think it's absolutely necessary as it would otherwise lead to awful consequences. […]

LORENTZON: It presumably won't stop them going in to read when they've finished work …

SIEVERT: No, but think about it, when the work's finished we'll lock the library.

LORENTZON: Oh yes, I see.

LARSSON: Yes but, for example if I see to department B, someone like engineer Andersson, who will presumably continue with his studies as well as he can, I think it's a bit much setting such a … he always has a lot of travelling to do, there's overtime and suchlike and it may be that there are a couple of hours that he can sit there and read rather than spending his time hanging around smoking cigarettes.

SIEVERT: Then he asks the head of department: May I sit in the library and do some reading? And then he gets permission to do so. On each occasion. Because otherwise we'll be swamped. I can say without further ado that I can never agree to that, because if you imagine that, well … I can't be there all the time asking what they're doing, but if Eklund and Håkansson and Walstam and Jonsson had access to the library whenever they like during working hours – that'd be complete madness.

LARSSON: Yes, but don't they come down in any case and ask for information, for example …

SIEVERT: And besides, they'd have a bit of a job using the library in some respects because they have neither the linguistic knowledge nor the capacity to be able to assimilate what's here. If they want to sit for a while when it's … in out-of-work hours or if they have a day off, that's their business, but we can't go paying these assistants full wages which previously everyone would have laughed at their receiving and that … for research work and suchlike from which they really benefit in terms of their training and then also let them use working hours for their own studies. Only to the extent that they can benefit us mind you, i.e., us leaving them … they're not entitled to do that.

LARSSON: Yes but they wouldn't get the opportunity in any case because the 'supervisors' give them work in any case.

SIEVERT: You never have control over that. They … there's nobody who can monitor their work fully day in, day out. No, I don't think it's a matter of …

LINDELL: But don't you think they'll work better if they feel you trust them?

SIEVERT: Well, I don't for one minute think that anyone here believes that I don't trust them …

LINDELL: But if they weren't banned, do you think they'd abuse the situation?

SIEVERT: Well, banning … it's just that this is a library for the academics; there generally need to be certain conditions for using the library. Walstam's on the borderline now as he's taking physics exams now and then of course he'll be over … I wouldn't like to say whether he'll have an academic qualification.

The discussion continued for some time and covers twenty or so pages of notes that I wrote. After a while it introduced a new problem which was discussed with the same fervour. When would it be appropriate to get on first name terms?

SIEVERT: And, personally speaking, I can also say I've done something that fits in with the democratic spirit in that I consistently use first names with the academics whether they're women or men, but I don't use the first names of *anyone at all* except for academics; you have to draw the line somewhere because otherwise it gets … and if you have a system to follow, and I let people know that this is the system I think I should take the liberty to apply, at least there's no-one who can say 'Why on earth doesn't he use my first name?'

FORSSBERG: That's probably quite right, but this can never apply to the girls.

SIEVERT: Why's that then - surely Berggren and Mrs von Cronsteen are on first name terms and, I mean, if we get a female colleague who's an academic, I will use her first name straight away because she's one of our class. I really do think it's the right thing to do; I don't believe in that type of democracy. On the other hand, I'll be hanged if I'm going to go along with their idea that if they want an hour off they can be given it whether or not it's for anything special.

FORSSBERG: That's an extremely difficult problem.

SIEVERT: What, using people's first names? Yes, it's very difficult. But look, if you think about it … Haglund who's been here for so bloody long, I'd do that naturally; I can't use the first name of a non-academic without firstly getting on first name terms with Haglund. That puts Thoraeus in a situation because he isn't… he doesn't use Haglund's first name.

LARSSON: Yes, but he'll do so when he retires.

SIEVERT: Yes, and I'm telling you that's what I intend to do as well. But I'm *not* retiring, full stop! But look, this form of democracy of using first names to make them amenable is completely… On the other hand, I do respect their work as much as anyone else's. I think an instrument maker who does a really good job is far flipping superior to a Professor who does a poor job. That's where I think the democracy lies.

FORSSBERG: I don't have a problem in this case. I don't find any difficulty being in the company of people for years and still calling them Mr So-and-so.

SIEVERT: But they may have difficulty spending time in *your* company, he-he, if *they* think you should use their first names. Yes, well you see… I don't, err, really think it's wise... what do you reckon [to Wahlberg]?

WAHLBERG: Yes, I think it's really inappropriate, this thing whereby

SIEVERT: … that we Swedes are stuck with this titles thing, oh yes, abso-bloody-lutely.

BENNER: We *were* actually meant to be talking about the library issue.

When Benner had restored the meeting to order, the discussion about who would be able to use the library continued for a good while longer. How did this come about? The implication is that I obliged Sievert to make a decision that could not be interpreted as discriminatory. The end of the discussion, where my stubbornness must have been trying for Sievert, was as follows:

LINDELL: Wouldn't the easiest thing be to say that anyone at the institution may use the library if he can do so with the permission of the person who's in charge of his work. And as regards these academic personnel, they're in charge of their own work.

SIEVERT: Indeed.

LINDELL: … and so they'll go there themselves, and with regard to the others they have to ask

SIEVERT: Indeed. Lorentzon has been friendly, and Benner, in helping me.

LINDELL: But then you don't need to go drawing any lines anywhere to because if Håkansson wants to go, for example, he'll ask you, the person he's working for, if he can …

SIEVERT: Indeed.

LINDELL: … for the purpose of the work.

SIEVERT: Yes, but I don't want us to establish this in some way and tell them that this how it is, that they have to abide by certain standards; we must simply say that …

LINDELL: Yes, but then you don't *need* to establish anything. Just say: the library's open to anyone and everyone but they obviously can't go there if it hinders their work and the person in charge of the work must see to that.

LARSSON: We can call it 'the work library'.

SIEVERT: So, in other words you mean that they need to be given an answer to this question? But the easiest thing is to say that they have to ask first. Isn't that the easiest thing? I think it is.

LINDELL: Yes, everyone may use the library to the extent that he thinks his work allows.

SIEVERT: But they're not the ones who'll determine …

LINDELL: No, of course not, they can't - they have to ask you - Håkansson, for example. Or Thoraeus or Forssberg and so on.

SIEVERT: Yes, absolutely, that's what I meant.

LINDELL: But then you *can't* draw any lines.

SIEVERT: Well no, I don't think you need to draw any lines. Borderlines will form of their own accord but that comes from the practical application rather than the exact way of wording it.

LINDELL: In practice, the borderlines will form automatically due to the organisation.

SIEVERT: Yes. But … I think it goes without saying that they have to ask.

LINDELL: Certainly.

SIEVERT: I don't think we need to make any formal decision here. We've agreed on how it will be taken care of in practice.

Sievert had great respect for Manne Siegbahn with whom he had cooperated during the 1940s' mobilisation of the Swedish physicists for achievements of military interest, the work which led to the Military Physics Institute (MFI) and subsequently the Defence's research establishment. In autumn 1951, a large, new cyclotron was commissioned at Siegbahn's institution, the Academy of Sciences' research institute for experimental physics, commonly known as the Nobel Institute. Work on this had begun thanks to a subvention from the Rockefeller Foundation and it was completed with generous contributions from The Marcus and Amalia Wallenberg Foundation, the Nobel Foundation and the State. The design work was led by physicist *Hugo Atterling* (1911–1989). The new cyclotron had a pole diameter of 225 cm. When it was commissioned, it could accelerate deutrons[*] to an energy of 25 million electron volts (MeV).

With the new cyclotron, Siegbahn started high-priority attempts to produce the new elements with higher atomic numbers than uranium, including element 99. The attempts were initially unsuccessful.

[*] Deutrons are atomic nuclei of heavy hydrogen and consist of a proton and a neutron.

Element 99 was demonstrated in 1952 in the USA in the dust from the first hydrogen bomb explosion ('Mike' at Enewetak in November 1952) and was named einsteinium. The nuclide proved difficult to study because its solid compounds were rapidly destroyed by its radiation. However, in 1957 at the Nobel Institute, it was possible to demonstrate element 102 for the first time, which was given the name nobelium.

The Nobel Institute was nationalised in 1964 when Siegbahn retired at the age of seventy-eight. It was then named the Institute for Atomic Physics (AFI), but it was still commonly referred to as the Nobel Institute. The name was changed in 1988 to the Manne Siegbahn Institute for Physics (MSI). Siegbahn had an ability affiliate good physicists for further training at the Nobel Institute.

A few key people from there come into my continuing story, including *Sigvard Eklund* (1911–2000), Arne Hedgran, *Torsten Magnusson* (1907–1987) and *Curt Mileikowsky* (1923–2005).

An institution meeting with Sievert in 1951 discussed the selection of a dose limit, or 'tolerance dose' as was said at the time, for those working with radiation. The discussion had been prompted because Sievert was trying to achieve accord within the institution prior to a forthcoming radiation protection investigation. Sievert was chair of the meeting and other persons present included Benner, Egmark, Forssberg, Helde, Larsson, Lindell, Lorentzon, Thoraeus and Wahlberg.

The discussion is reproduced here as I, being secretary of the meeting, took notes from an Agaphone recording with no editing whatsoever.

SIEVERT: Yes, we started talking about this a bit last time, and I believe that if an investigation is to take place and any go-ahead is to be given for this and we're to get something carried through, we broadly speaking need to be singing from the same hymn sheet here at the institution. I don't for one moment mean that we should pretend to have one of those awful agreements that allows no other person to stand up and voice a different opinion, but I think if we *do* have different views on essential matters, we shouldn't go believing that this Committee that's been set up will be able to work on any specialist issue disputes because it'll be a Committee that sees things from the organisational point of view, and regardless of how this institution ought to become part of the Machinery of Government in the future and so on, we'd said that we'd like the tolerance dose to be established by an authorised agency. There *is* no such authorised agency apart from the Radiophysical, we know that, because if a medical institution is asked to express an opinion, it always centres around what we've said and the position of responsibility that they have; let's take the example of when Wahlberg started on the provisions and discussed the tolerance dose, what we would dare to allow a patient to receive, it was up to 100 röntgen* which they initially thought hadn't ought to be all that hazardous.

For example, they said a shoulder joint – 800 röntgen?! – and worse still *I* thought it was a few hundred; where were we, 800 röntgen - and then they came down to 0.1 röntgen, and this is an exact picture of how things are, i.e., there's no doubt that if we put a medic in the same position as us, he's going to be responsible for ensuring that no injuries occur, and he'll say how far down the dose has to go* and end up being no more successful than the rest of us. But *he* doesn't have such good conditions for judging it as we do. Now, this matter of the tolerance dose and then the need for inspections in different professional groups, we need to form an opinion, so – would you

* A 'dose' (actually exposure) of 100 röntgen (r) corresponds to an equivalent dose of around 1 sievert (Sv). 0.1 röntgen thus corresponds to around 1 millisievert (mSv). The unit röntgen was denoted by an 'r' for the period of time that this book covers, but by 'R' following the breakthrough of the international unit system (SI) in the 1960s.

gentlemen care to say something on the matter please? We've spoken a fair bit about the question of the tolerance dose so I … now we saw at the previous meeting that the tolerance dose is very closely connected with, or rather that the need for inspections is very closely connected with fixing the tolerance dose, but if we agree that someone should not receive more than 0.1 röntgen per week whether or not we're to *establish* this in some way, it doesn't need… I mean whether or not it's to be included in the regulations is a matter of secondary importance, but Helde can probably answer this with the support of the blood tests – what would you like to say: when do you see the more serious …

HELDE: I haven't finished it yet. I daren't write anything.

SIEVERT: Yes, but the order of magnitude?

HELDE: The order of magnitude is right.

SIEVERT: Well, I mean: you have no doubt that using 0.5 röntgen a week, for example, you'll get - you can count on blood changes.

HELDE: Statistically speaking, that's what it looks like.

SIEVERT: *Well*?

HELDE: Yes, well it's difficult to say what each individual person has received; in giving a specific risk class there's a variation width, both the personal sensitivity, a variation width in work technique … and all of this means it's difficult to say for certain, but you can say it in terms of statistics.

SIEVERT: So, in the order of 0.5?

HELDE: That order of magnitude, yes.

SIEVERT: Yes, and there are individuals who might have changes already at 0.2–0.3; in America they've found even … so?? Wahlberg?!

WAHLBERG: Well, since we've been speaking about one of those authoritative organisations …

SIEVERT: Yes, authorised agency.

WAHLBERG: … or whatever it was, it has to be said that it's surely not the intention for them to directly …

LINDELL: Responsible authority.

WAHLBERG: Responsible authority … for them to determine the tolerance dose but they will establish - they've asked us several times what's to be done in one case or another, and we say: we can't answer that, but if you're talking about the percentage of employees in this work and that work who could conceivably injure themselves… if *that's* what you're talking about, we can provide a more definite limit dose.

SIEVERT: Where do you think you'll find someone who's willing to state that number?

WAHLBERG: Maybe we can't. But the risk and the radiation protection devices have to be weighed against the practical requirements and the economic consequences. It can't be reasonable to pay something like one million *kronor* a year in radiation protection improvements to save 1/10 of those working with radiation and so on …

SIEVERT: (calculates quickly) three and three tenths of a person …

WAHLBERG: Yes, yes.

EGMARK: Regarding authorised agency so …

LINDELL: Responsible.

EGMARK: Responsible authority, yes, the Medical Board grants licences for all facilities, and regarding the care and appearance of those facilities, they understand just as little as they do about the tolerance dose, but the Medical Board is nevertheless the body that grants the licence.

SIEVERT: Yes, and they do so after hearing from us so their licence is purely a formality, it's simply that it has to be kept by an authority, the actual licence. No, I'm afraid we must work on all these disputes regarding the tolerance dose and who are to be inspected *internally*.

In 1950, the enterprising Sievert had created an organisation that he called the Atomic Energy Research Radiation Protection Council. In the annual report for The Institute of Radiophysics, he wrote:

> The Council consists of representatives of The Atomic Committee, AB Atomenergi, the Civil Defence Board, the Defence's research establishment, the Defence's Healthcare Board, the Defence Staff, the Medical Board and The Institute of Radiophysics, and the member of the Medical Board's Scientific Council in the subject of radiotherapy, the Assistant Professor and head of department at The Institute of Radiophysics' department for the control of protection devices for radioactive preparations, the Assistant Professor in radiation biology and chemist at the Institute of Radiophysics, one of the experts in nuclear physics and atomic energy appointed by the other members of the Council, plus the other people who may be unanimously appointed by the Council.

In his book about Sievert (Weinberger, 1990) Hans Weinberger writes: 'This distinguished Council was to assist the development of radiation protection when using atomic energy. The four different organisations connected with the military show the interest with which the Swedish Defence followed the development of atomic energy'. However, it was more likely that they reflected Sievert's interest in the military development and his desire not to be disregarded. In the absence of initiative on the part of others or in situations where the allocation of responsibility was uncertain or there was bureaucratic inertia, Sievert utilised the lack of action and was quick to take initiative without paying much attention to formalities. He wanted to be involved and he wanted to be in control. One advantage to be gained from his unconventional initiatives was that they created contacts across all areas of interest and brought together people who might otherwise never have met one another or even been aware of one another's existence.

The most memorable achievement of the Atomic Energy Research's Radiation Protection Committee was an assignment that was given to radiation biologist Arne Forssberg at Sievert's institution and Professor of Genetics at what was then Stockholm University, *Gert Bonnier* (1890–1961). Bonnier would also be part of the Swedish group of experts for the UN's Scientific Radiation Committee (UNSCEAR) and was one of the pioneers in genetic research in Sweden. He was the father of the racing driver *Joakim* (*'Jocke'*) *Bonnier* (1930–1972) who tragically lost his life in his thirteenth start at the 24-hour race at Le Mans in 1972 and was the brother of the publishers Kaj, Tor and Åke Bonnier.

Bonnier and Forssberg's task consisted of examining the genetic effects of atomic bomb explosions and the distribution of the radioactive substances. The two scientists issued a report on 19 April 1951. The report was discussed at a meeting of the Radiation Protection Committee that same day when Bonnier and Forssberg gave an account of the content and attached a two-page statement that Sievert read out. This statement was adopted by the Committee as its own and was signed by the thirteen members who were present.[*] These were significant people. Sievert had not made do with representatives of the different authorities, but had succeeded in gaining the interest of the managers. The first three points of the statement read:

> 1. Already relatively small additions to the radioactive[†] radiation that normally occurs in nature leads to an increase in harmful mutations, which for forthcoming generations will probably lead to some pathological changes which, although minor, are not completely insignificant from a social point of view, such as digestion disorders,

[*] These were Hugo Ahlbom (Professor of Radiology and head of *Radiumhemmet*), Sven Benner, *Albert Björkeson* (born in 1892, head of the FOA), Gert Bonnier, *Harry Brynielsson* (1914–1995, MD of AB Atomenergi), Sigvard Eklund (Head of Research at *Atombolaget* [the Atomic Company]), Arne Forssberg, *Torsten Gustafson* (1904–1987, Professor of Physics at Lund and adviser to Tage Erlander), *Axel Höjer* (1890–1974, head of the Medical Board), *David Lindsjö* (1887–1952, Surgeon General and head of the Defence's Healthcare Board), *Torsten Schmidt* (1899–1996, General at the Defence Staff), Rolf Sievert and *Åke Sundelin* (1913–1988, head of the Civil Defence Board).

[†] This phrase is no longer used; it is the source of radiation and not the radiation itself that is radioactive. The correct term is ionising radiation.

> allergies, cases of anaemia and disruptions to the endocrine functions (the hormonal balance).
>
> 2. A more serious risk of genetic injuries may be estimated to arise when the annual dose of radiation per inhabitant exceeds that which is received through the natural radiation, which on average gives an annual dose of around 0.5 r [approximately 5 millisieverts].
>
> 3. If at the time of an atomic bomb explosion (similar to those that occurred in Japan) over Stockholm, for example, you assume that 150 000 people receive a dose of radiation of an average of 5 r [approximately 50 mSv], from the genetic point of view this corresponds to 0.1 r per inhabitant in Sweden. Already some tens of such irradiation of the population could be estimated to lead to essential changes in the genetic material, which must be assumed to be likely to lead to considerable injuries to subsequent generations.

The statement concluded with a reservation:

> However, it must be expressly stated that the knowledge of human mutations under different circumstances, both spontaneous and radiation-induced, is not sufficiently complete for a safe assessment of the genetic effects at the time of atomic bomb explosion and the distribution of radioactive substances. However, these statements we are making represent what currently appears to be likely. A reservation must obviously be made for what may be unearthed by the further development of genetic research.

The 30-page report from Bonnier and Forssberg contained some particularly interesting information. The report maintained that the mutations caused by ionising radiation were no different from those that occurred normally. The radiation could thus not be feared to cause spectacular monstrosities.

During the discussion of the report and the statement, several members said that the statement ought not to be published. Sievert had given me the task of acting as secretary of the meeting, and my record notes reflect the substance of the discussion:

SUNDELIN:	Largely concur but have a suggestion as regards rewording for point 1 of the read out statement.
AHLBOM:	Concur also, but suggest further rewording of point 1. Think that the statement should not be published.
BRYNIELSSON:	Concur and also think it's unsuitable for publication.
SIEVERT:	The statement will be given only to responsible authorities.
EKLUND:	It ought to be possible to produce a second version that's suitable for publishing.
SUNDELIN:	The original statement ought to be labelled 'For official use only'. We probably also ought to have a version that can be used by civil defence instructors and is thus preferably not kept secret.
HANSSON:[*]	Also oppose publishing the statement in its current form; it would result in people being scared to have x-ray examinations.
SIEVERT:	The risks of x-ray examinations seem to me to be smaller than claimed by Professor Bonnier. It's no more significant than moving into stone houses in cities.
BONNIER:	It's a matter of judgement.
BJÖRKESON:	The doctors probably need to be reminded that they should exercise restraint with their x-ray activity.
HANSSON:	Particularly where non-radiologists but other specialists unnecessarily refer people for an x-ray examination.

[*] The hospital director and radiologist *Nils Hansson* (1888–1961) participated in the discussion but is not one of those who signed the statement.

BRYNIELSSON: Before we can make a decision on the statement, we must find time to read the report.

BONNIER: I expect to obtain information that may be useful for any reworking while on a visit to America.

The decision was to take up the matter of the statement at the next meeting in May. I have found no minutes from that meeting if it did take place. On the other hand, there is a stencilled version of the report from Bonnier and Forssberg labelled ‘For official use only’ and dated 19 April 1951. It begins with the statement signed by the thirteen members.

At the April meeting of the Atomic Energy Research’s Radiation Protection Committee, Assistant Professor Eklund also said that the site for the first Swedish nuclear reactor had now been determined as a rock shelter beneath Drottning Kristinas väg next to the Royal University of Technology (KTH) in Stockholm.

When the 16th Congress on the 30-year anniversary of the Nordic Society for Medical Radiology opened at the Swedish Society of Medicine in Stockholm on 10 June 1949, Professor Berven had said the following:

> During the initial years, the radiologists did not actually have a ‘home’ of their own. The radiological departments were situated in some room or other close to the medical or surgical departments, often in some basement or adjoining room. Those diagnosing the x rays were often called ‘medical photographers’, and when *Dr. Mygind* demonstrated a few x rays in Copenhagen in 1896, *Baastrup* remembered that the Professor of Surgery is meant to have said: ‘Yes, this may be amusing but it can never be of any practical significance.’ A few years later, a Swedish doctor said that radiation therapy was scientific humbug.

Berven is said to have had the subsequent pleasure of treating the sceptical doctor for a cancer in his face with a good result. Berven was a specialist in head and neck tumours which were treated at *Radiumhemmet* with ‘radium guns‘ or ‘radium cannons’, so called because, as opposed to contact treatment with a radium preparation, radium was used at a distance from the irradiated body and gamma radiation was ‘shot’ at the tumour to be destroyed. The radium guns at *Radiumhemmet* were introduced in 1923 by the ingenious radiologist *Erik Lysholm* (1891–1947), who constructed a supported cylinder in which available radium preparations could be temporarily placed and the radiation screened off in a way that was not possible in cases of contact treatment. The structure was considerably improved firstly by Sievert and then by Benner in that the guns were given cassettes containing special radium tubes. The cassettes could be transported from the place of treatments to a more protected place in a separate protection container.*

At the start of the 1950s, at the teleradium department in *Radiumhemmet’s* basement were two radium guns containing 3 and 5 grammes of radium respectively, a considerable quantity given that the price of radium was approaching 100 000 Swedish kronor per gramme. The guns were in separate rooms with thick walls to reduce the levels of radiation in the control room which lay between the irradiation rooms. However, in order to reach the control board, you had to pass the entrances to the rooms and there, the radiation exposure could amount to 100 milliröntgen (approx. 1 millisievert) in the hour before the situation was improved with strong radiation protection investments mainly by Rune Walstam.

Radiumhemmet was a world leader thanks to Berven’s skill in handling the radium guns. In this respect he was an artist who utilised his many years of experience as opposed to measurements and planning. However, at the end of the 1940s, according to the reported statistics the successful treatment result was shown to have been achieved at the cost of local radiation injuries. Berven then reduced the treatment times, thereby avoiding the injuries, but the treatment results were compromised to a considerable extent.

* More can be read about radium guns/cannons in ‘Pandora’s Box’.

In the late 1940s - early 1950s, I also took the initiative of participating in the doctors' rounds at *Radiumhemmet* to gain an insight into how the cancer treatment functioned. This was of course with Berven's go ahead – maybe a physicist was actually capable of forming an idea. So, Sievert's brilliance meant that his colleagues were viewed as people who might also be able to think up ideas themselves.

My visits to *Radiumhemmet*, not just on the rounds but also as a temporary replacement for Sievert where teaching was concerned, gave me good contacts with mainly the younger radiologists whom I also met at the lunches in the doctors' dining room where Sievert's institution and *Radiumhemmet* ate at the same table. It was primarily the friendly and knowledgeable *Lars-Gunnar Larsson* (1919–2009), who later became Professor of Radiotherapy in Umeå and Vice-Chancellor of Umeå University, and the animated *Olov Dahl* (1919–2003), who had close cooperation with us radiophysicists where new treatment methods were concerned. Another brilliant radiologist was *Sixten Franzén* (1919–2008) who had invented the aspiration biopsy at the end of the 1940s. This was a biopsy (taking a sample from living tissue) where you could use a fine needle to suck up cells from a suspected tumour. A diagnosis could be given without an operation being required. The method changed tumour diagnostics all over the world.

Berven was no stranger to new ideas, even if they were unconventional. When, while on his rounds, he questioned the treatment of a patient, the senior nurse happened to say that she had given the patient garlic capsules. She said it with all authority and Berven accepted her prescription instead of telling her off for lack of discipline. The discipline was otherwise strict at this time when it came to both personnel and patients. The authority of the boss was never questioned and the senior nurses, who had considerably more experience than the junior doctors, were strict and demanding. When the round started, all patients who were able to stand were meant to be standing to attention at the bottoms of their beds in a somewhat military fashion. Many people from the Professor with his assistant professors, junior doctors and those who were training, to senior nurses and other personnel of various ranks participated in the round. Everything was very solemn and authoritarian.

After a few years I stopped participating. I could not bear to become acquainted with the situation of the patients, knowing at the time that a malignant melanoma on the leg was synonymous with a death sentence. Trying to understand how it would feel to have your lower jaw removed to leave behind nothing more than a flaccid pocket of skin. Sometimes things went dark before my eyes when I put myself in the shoes of a patient and I was sometimes forced to sit down on the edge of a bed. Doubtless the nurses thought I was a wimp and I realised I'd done the right thing in choosing not to become a doctor.

I still remember a few details here and there. Berven said that drinking spirits was probably the cause of many cases of throat cancer. He said that he was always careful to rinse his mouth with a glass of beer after having drunk a 'snaps'. Heat could also cause cancer, said Berven. How else could you explain the fact that of the women from the northernmost parts of Sweden who came to *Radiumhemmet* to have lip cancer treated, the majority smoked pipes with a metal shaft - shafts that became hotter than shafts made of other materials.

In 1951, the head of the Ministry of the Interior called on specialists to propose how the work at Sievert's institution should continue. The specialists adopted the name of the 1951 Radiation Protection Committee. The first chair was Director *Nils Godenius*, although in 1954 he stepped down from the assignment and was succeeded by County Governor and Professor of Philosophy *Malte Jacobsson* (1885–1966), who was also chair of the Atomic Committee at the time. Other members of the Committee included Sievert, Helde, Sigvard Eklund and the doctor at the Södertälje general hospital, radiologist *Carl-Gustaf Sundberg* (1892–1963). The First Assistant Secretary of the Ministry of the Interior *Gunnar Olofsson* (1920–2002) was appointed as Secretary of the Committee.

It was not clear why Helde was present. There was disagreement between Sievert and The Institute of Radiophysics' supervisors as regards this. Sievert was obviously a member of the Committee but the radiation protection inspectors also wanted a representative. Sievert thought that this was unnecessary - he had a good enough overview himself, and he also thought that the inspectors were not knowledgeable enough. As an example, he mentioned that they would not be able to implement a clinical dosage plan (the person who could do it, the versatile Sven Benner, would leave Stockholm for Gothenburg in 1952). However, the supervisors' protests led to Matts Helde gaining a place in the investigation.

However, Sievert had many irons in the fire but had not spent much time on medical physics since defending his thesis; this task he transferred mainly to Benner and Thoraeus. When Benner had moved, the doctors at *Radiumhemmet* complained more and more often about not getting enough help from the physicists. When Sievert was on holiday at his country house in Tvartorp in summer 1952, emergency help was requested with planning doses for a patient with laryngeal cancer, i.e., cancer of the larynx. The treatment was to take place using one of the two radium guns.

This was when Sievert's supervisors could be useful in the first instance. They took great pleasure in taking the opportunity of referring to the fact that Sievert had called them incompetent when it came to planning doses. Sievert can jolly well travel up from Tvartorp himself was their tongue-in-cheek response. Sievert then called Rune Walstam and asked him to go over to *Radiumhemmet* and ask me for help as well if necessary.

At the time, Rune lived close to Brommaplan and I lived in Huvudsta. We both cycled to work and often cycled together from Huvudsta. One of the subjects we talked about while cycling was the lack of assistance received by the doctors at *Radiumhemmet* since Sievert had lost interest in medical physics. Sievert's telephone call was therefore gratefully received.

Rune Walstam and I looked at the proposed treatment with one of the radium guns. We did not think the distribution of the treatment fields was appropriate (three fields, one from the front and two from the side). Instead, we proposed to the responsible radiologist Sven Hultberg a treatment using two obliquely-directed fields for better dose distribution. Berven had retired in 1950 and had been succeeded by Hugo Ahlbom, although he was already ill when he started and died at an early stage. Hultberg was to succeed him as Professor and head of *Radiumhemmet* in 1953 and at the time of our conversation was a senior doctor and worked as head of the clinic. However, he did not dare to deviate from the conventional treatment technique, but did show an interest and wanted the treatment method to be investigated further. And so the more intensive planning of doses on an individual basis at *Radiumhemmet* began.

Rune Walstam became quickly involved in this. He had already made himself at home at *Radiumhemmet* in having taken radiation protection measurements. This input had originally come about because *Radiumhemmet's* doctors had complained to Sievert that they were not getting the results of the dose measurements previously taken by the personnel. They accused Berven of keeping the measurement results secret. Rune's input began by realising an idea of Sievert's for a new type of ionisation chamber for radiation protection measurements. The chamber consisted of three concentric cylinders of paper impregnated with graphite where the outermost cylinder was 30 millimetres long and 15 millimetres in diameter, and the ionisation took place in the two air volumes between the cylinders. The instrument thus actually consisted of two separate ion chambers with different volumes of air, adapted so that the outer chamber with the larger volume became ten times as sensitive as the inner, smaller chamber. The double chamber therefore had a total of one sensitivity area which covered a couple of powers of ten. If the outer chamber was discharged, the hop was that the inner chamber could still give measurement results. The double chambers were intended to be used in pairs in a plastic cylinder. The new instrument was described in a paper by Sievert and Walstam in 1951.

With the help of the double chamber, Rune Walstam began a careful, detailed examination of the radiation exposure of the personnel at Radiumhemmet. He realised that 'continuous direct dose measurements of the different workers over a long period [constituted] the only reliable method' (Walstam, 1957). The investigation culminated in 1953 with measurements of 160 employees. The result of the dose measurements were given to the personnel. Knowing which dose had been received and knowing when and why it fell or increased also helped to improve the radiation protection conditions.

The situation proved to be serious and that major radiation protection inputs would be necessary. Average weekly doses of 500 milliröntgen, corresponding to annual doses of 250 millisieverts, were not uncommon. Rune consciously investigated not just how large the doses of radiation were but also why they were large and which work activities were particularly hazardous. He proposed considerable numbers of improvements to work techniques and introduced technical tools such as mobile radiation screens to bring down the doses of radiation. Many of his proposals concerned simple solutions that no-one had thought of. One measure that was introduced in 1953 was to turn the beds on a ward around so that the patients lay with their feet directly facing the wall. This applied to the beds of female patients who were having gynaecological treatment with radium applications. The personnel who were caring for the women were exposed to less radiation when the beds had been turned.

The interest of the older doctors in radiation protection improvements was sometimes not as great as it could have been. Berven, like many other older radiologists, had sometimes tested the intensity of the radiation by irradiating his own arm and watching the skin turning red. His colleague, Chief Physician and head of *Radiumhemmet's* gynaecological department *Hans Ludvig Kottmeier* (1903–1982), had never been worried about picking up radium preparations with his fingers and had difficulty believing that it led to any risk because he had never been injured by doing so. Engineers *Lars Jonsson* and Rune Walstam nonetheless succeeded in bringing in major improvements to the radiation protection in the gynaecological department where the biggest problems lay. Among the effective protection devices that were noticed most often was the screened packing table for radium applicators and additional charging devices with whose help the ionising sources could be entered into body cavities into which an empty applicator had already been placed. The small, encapsulated radium preparations could sometimes go astray. Rune Walstam tells of such an episode in his history of the work at *Radiumhemmet* (Walstam, 2002):

> Three serious near misses by the gynaecological department at [*Radiumhemmet*] may be worth mentioning. […] When the third case occurred, I was called up early one morning from a ward. A vaginal cylinder containing 200 mg Ra (7 400 MBq!) could not be found when it was due to be removed. A quick look meant it was easy to ascertain that the source was neither inside the patient nor in the treatment room nor anywhere else on the ward. Valid instructions stating that nothing in the form of bandages, tamponades or other treatment material could leave the department before the radium check.
>
> The only thing that had happened was that the waste-paper baskets had been emptied! What happens to those then? The content goes directly to the hospital's waste combustion! At the double with Bo Lindell and Arne Hedgran with instruments at the ready! Already outside the building where the level of radiation was clearly raised. We got the machine-operating personnel to shut off the oil burners and rinse the ash with water. We soon found the applicator in the ash. We scooped it up and rushed up into the park (there were not that many buildings in the way at the time). We could not decide whether any of the four 50-mg tubes had broken in the soldering and in which case had leaked radon. We called on a glass blower who enclosed the whole thing in a glass tube which was sent for final storage in Studsvik.
>
> The patient in question had been admitted from a psychiatric department. She had got up in the night and peed in the waste-paper basket and pulled out the vaginal applicator. This incident led to psychiatric patients having to have a minder with them to monitor them when they had radium treatment.
>
> We physicists from The Institute of Radiophysics who were on duty were occasionally rung by *Radiumhemmet* when a radium capsule was missing. This was discovered when a control count of the preparations took place in the lead drawers in which they were stored in a lead-protected storage cabinet. The level of radiation was so high that it was difficult to use measurements to ascertain the location of a runaway preparation. Sometimes the capsule had fallen out of the lead drawer and was lying deep inside the compartment into which the drawer was pushed, visible when lit up by a pocket torch but difficult to get to. We then had to fish out the preparation using a ruler with something sticky on the end. Sometimes the preparation had fallen onto the floor. You therefore always had to be careful when entering the storage room. Unfortunately, the floor was patterned so it was difficult to see whether there was a small capsule lying there. We then had to bend down as far as possible so that it was easier to see whether anything could be seen sticking up from the floor. It was the same technique that I had used as a child when I had lost lead shot on the lino after playing a game.
>
> Another incident occurred as a consequence of ignorance. One of *Radiumhemmet's* engineers was aiming to wash a number of radium capsules and thought that only the best was good enough – he washed them in aqua regia, the only acid mixture that had the ability to dissolve the welds, thereby taking the risk of radium being released.

> Before he left Stockholm, Benner had performed planning dose experiments using a dose contour projector invented by Professor *W. V. Mayneord* (1902–1988). However, when Walstam had also been asked to assist with the planning of doses, he had the idea of producing wire templates to facilitate the planning of the dose when using the radium guns. Such a wire template was made of components for each treatment field, consisting of three brass wires, one of which marked the direction of the central beam and the other two were adapted to the contours of the cheek or jaw of the patient, for example. The components could be joined to form one unit which fulfilled two functions. Not only could isodose curves on transparent, flexible paper be fixed using the wires that marked the direction of the beams, you could also then read off the expected dose of radiation at different points, stated as a percentage of the dose on the surface. On this basis you could determine the requisite treatment times for each treatment field. At the time of the actual treatment you could also use the wire template to point the radiation in the right direction.
>
> Rune's wire templates made it possible to increase the doses of radiation in the tumours once again without seriously damaging the skin. The clinical result, which had diminished once Berven had decided to reduce the treatment times to prevent skin damage, improved once again.

Sievert in Stockholm was not the only person to develop Swedish academic radiophysics. A man in Lund who had devoted himself to radiation problems was John Tandberg, who went on to be given the title of Professor for his scientific work at AB Electrolux. The actual radiophysics with healthcare achievements in Lund was established in 1947 when physicist *Kurt Lidén* (1915–1987) became Assistant Professor and created an institute of radiophysics at the university. In 1964 he became Professor of Medical Radiophysics. Gothenburg was where *Karl-Erik Zimen* (1912–1998) started Sweden's first nuclear chemistry laboratory, which was managed by the Atomic Committee from 1945. In 1956, Zimen was given the title of Professor and was in charge of the chemistry department at the Hahn-Meitner Institute in Berlin from 1956-1957. Zimen was also interested in radiophysical problems but Sievert did not hold him in high regard. The radiophysical activities in Gothenburg which were associated with healthcare definitely came to fruition when Sven Benner left Stockholm in 1952 to become radiophysicist at *Sahlgrenska Sjukhuset* [Sahlgrenska Hospital] and in 1954 became Assistant Professor at Gothenburg University (and was given the title of Professor in 1966).

It has sometimes been said that the relationship between Kurt Lidén and Sievert was tense and that Sievert from had opposed Lidén's ambitions to also create an institute of radiophysics in Lund from the very start. However, the communication between the two shows little to support this rumour. Lidén wrote the following in a letter to Sievert on 1 December 1949:

> My good friend,
>
> Two months ago, Ebenius[*] wrote a letter to the board of the Cancer Society to apply for a grant of 4032.00 Swedish kronor for a junior research assistant to be employed at the radiophysics laboratory here in Lund. Since Ebenius recently informed me that you were the Secretary of the Society, I wanted to take this opportunity to mention the matter and at the same time ask you whether you know anything about whether the letter has been handled and any outcome. At the moment I have a man who is temporarily at the laboratory with funds from the annual grant. However, as you will understand, this can be arranged for only a few months.
>
> However, before conversion of the premises here began a few weeks ago, we did manage to complete the first experiments with the modified type of Bg-chamber, which is mentioned in Ebenius' letter. [Here follows a detailed technical description of the new chamber.] [†]

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[*] *Bertil Ebenius* (1902–1959) was the leading radiologist in Lund.

[†] Abroad, Sievert's small ionisation chambers were called the 'Sievert chamber'. The fact that Lidén did not call them 'Sievert chambers' but 'Bg-chambers' has sometimes been taken as an indication of disagreement between him and Sievert. However, this is a

Regarding the remaining research assignment mentioned in the letter, fluorescence – photocells, etc., your expert statement, in which I have now had the opportunity to show my interest and study, considered the electrometer device to be superior in that it is simpler and more reliable. This is no doubt the case in many instances but, in support of my ideas, I can refer to a completely new article in the *Review of Scientific Instruments 20* (1949), 711 (Oct. 1949), which describes an instrument for measuring radiation protection which in principle used the same combination that was proposed by me.

The Faculty of Medicine also appears to be finalising the matter on 6/12 and it will be interesting to see what the members have to say, particularly with regard to your statement since you are after all the only specialist among the experts and since you quite rightly criticised my weak qualifications within medical radiophysics.

With best wishes,
Your friend KURT LIDÉN

Sievert responded to this in a letter to Kurt Lidén on 3 December 1949 (the speed of the post at that time is enviable):

Brother,

Many thanks for your letter of the 1 of the same. As you will understand, I am very pleased that you are continuing to develop the condenser chamber method and we have just been discussing here whether or not we ought to make a few chambers as per your latest model. To the extent that I can do anything about this matter, I will be happy to put in a few words regarding the grant of 4,032.00 Swedish kronor, although I do think it might have been better if you had applied for the grant yourself, principally from the point of view that the physics laboratory ought to be as independent as possible.

I am pleased to see that you have not been offended by the fact that I was unable to avoid mentioning in my expert statement that your medical radiophysics qualifications fall short. As you will no doubt understand, I believe objectivity required this, albeit I would have preferred to not to have written that. You will probably still get the Assistant Professorship and I personally am convinced that you will be very good in that position.

I thank you for the previously-sent instruction for the Assistant Professor which I, like you, find dissatisfactory. I am surprised that someone from the University Chancellor's Office did not refer the case here, although that is probably because an instruction is considered to be much less important and they do not really want to cause trouble with it. When it comes to the crunch, a man who knows what he wants will still do what he wants. You can probably go some way towards independence before meeting any opposition. You have our support up here in any case.

I would be delighted if you could visit Stockholm from time to time since it is definitely important for the physicists who work for medical purposes to stick together. […]

With best wishes,
Your affectionate friend
ROLF SIEVERT

On 22 February 1950, Lidén is preparing his participation in the International Congress of Radiology in London later in the year and writes to Sievert for advice regarding suitable institutions to visit in England at the time of the Congress. He mentions Harwell, the Radiochemical Centre in Amersham and the Christie Hospital in Manchester, among others. Sievert writes a response as early as the following day (!):

Brother,

misconception. Sievert himself, like everyone in Sweden, called them 'Bg-chambers', a name that he introduced himself ('B' for 'Bärnsten' or amber, the chamber insulation material, and 'g' for 'graphite', the wall material).

Since the documents concerning the position of Assistant Professor in medical radiophysics at the Jubilee Clinic in Gothenburg have been referred to me,* I would be very grateful if you would send me details of the regulations for your appointment and what the same is called (exact name), the pay grade and the rank of the job at the university. I would be very grateful if you could send the details fairly soon.

Just received your letter. As far as I can see from the same, you have managed to include most of the institutions in England that are worth looking at. I think you would do well not to include any more of them but instead spend a longer time on a few of the visits. Perhaps you could include The Royal Infirmary (physicist Spiers) and visit the Cavendish Laboratory and Strangeways Laboratory.

With best wishes,
Your affectionate friend
ROLF SIEVERT

Lidén responded to this on 25 February:

Brother!

Thanks for the information in your letter of yesterday on suitable laboratories for study visits. I will attempt to fulfil your request for data about my job here.

The position was granted from 1/7/1947 in accordance with proposal 272:1947 with desired competence requirements stated on page 355. It was declared available at the start of the autumn semester, as is the custom with all new university teaching jobs, with a month in which to apply and 90 days thereafter in which to submit additional articles to add to your merits. The university statutes then contain the general regulations that apply concerning experts, teaching tests, the principles for assessing the competence and suchlike of Professors, Assistant Professors, etc.

According to my royal mandate, I am an Assistant Professor in Radiophysics at Lund University. Nothing else is said in this but the above proposal requires it to concern the medical faculty. The name ‘medical radiophysics’ is not sanctioned at the highest level, even though it has been used in a large number of letters pertaining to the case.

Salaries are paid according to the 1925 salary plan for professors, associate professors and some other teachers at the University, so they are unregulated posts (not areas that have a living expenses weighting) and, as you know, a report on these matters came out last autumn which will probably not go before Parliament until 1951. Associate professors’ salaries in Lund are currently 15 445 Swedish kronor per year with 500 kronor seniority allowance after 5 and 10 years. The final salary is then exactly the same as lecturers’ salaries in salary weighting area III. The pension contribution is then deducted but 9 kronor per child and month is added!

The position of the job at the university was the last point of your letter. One thing is that is clear is that it is an associate professor’s post that is the exactly the same as any other similar jobs in the various faculties. You received a copy of the instruction earlier, and it is a little strange that it expects an errand boy job to be done for the general hospital! The clinical associate professors (Ebenius, Edström, etc.) are all paid a Chief Physician’s fee by the County Council and for doing private business (with the research it is not up to much) – not one öre is paid for the radiophysics job. So, it has not yet been clarified whether this job would be theoretical or clinical; Ebenius obviously meant it to be clinical.

With best wishes,
Your affectionate friend
KURT LIDÉN

The fact that Sievert followed the development with such great interest when new university jobs came up – and in this connection institutions – in radiophysics was not due to fear of competition or with

* The job that Sven Benner started in 1954.

an intention to dominate. He was firmly convinced that radiophysicists with healthcare assignments must have an independent standing and not have to answer to any doctor. This rather unique independent standing was also given to the new radiophysicists, largely thanks to Sievert.

Physics and radiophysics flourished in Lund at the start of the 1950s. In 1950, the new physics institution was inaugurated in the presence of celebrities like *Niels Bohr* (1885–1962) and *Wolfgang Pauli* (1900–1958). Staff included Professors of Physics *Sten von Friesen* (1907–1996) and *Torsten Gustafson* (1904–1987). Staff also included physics Assistant Professor and subsequent Professor *Sven Johansson* (1923–1994), who would go on to become Vice-Chancellor of Lund University in 1970 and who was an expert in scintillation spectrometry. In this environment, Kurt Lidén's radiophysics institute grew to form a bridge between the faculties of natural science and medicine, where Kurt was given a Professorship in 1964. Many capable radiophysicists defended their theses with Lidén: *Carl Carlsson*, *Gunnar Hettinger*, *Holger Sköldborn* and *Nils Starfelt* (1927–2011).

However, cooperation problems were arising in Stockholm. I have already mentioned the first serious conflict when, in an emergency situation in 1952, Sievert's radiation protection inspectors refused to plan doses at *Radiumhemmet*. Matts Helde was the main person to feel displeased. Helde had worked at length but fruitlessly on a doctoral thesis. He put his incapacity down to Sievert, who he did not think gave him enough opportunities for research. Helde himself was probably the most at fault, having great difficulties making decisions and easily getting bogged down in fruitless speculations. My impression is that Sievert always encouraged and enabled personnel who took the initiative in research efforts. In 1953, Rune Walstam and I had taken up a lead in dose measurements of x rays taken by Lars Lorentzon and which brought unexpected results.

It was possible to explain the results by the compilation of the secondary x rays emitted from an irradiated body. A scintillation counter was needed to analyse the secondary radiation, but at the time we had no access to the sodium iodide crystals that were needed as detectors. We were always obliged to produce a crystal ourselves. The result was certainly small, but we succeeded. Nor did we have access to any electronic pulse height analyser. Instead, we led the current pulses from the irradiated crystal to an oscilloscope, the screen of which we photographed. We then read off the blackening on the film using a densitometer, thereby obtaining a radiation spectrum. We and Lorentzon were able to publish the measurement results of both the crystal and the ion chambers in 1953. I mention this because this work fell completely outside our ordinary tasks and because Sievert raised no objections.

One of Helde's research projects concerned an analysis of the results that had been archived from the blood samples taken at the obligatory medical examinations of people in 'radiological work'. Helde had seen a connection between deviations from a normal blood count and the irradiation to which in his judgement the people had been exposed. Helde's results had played an important role in the introduction of a special holiday extension for radiation workers.[*]

In 1953, Helde and *Thor Wahlberg* published a paper in *Acta radiologica* on the importance of the time factor to changes in the blood. This took place against Sievert's will. Sievert thought the paper was unscientific and could destroy the reputation of the institution. He later forbade the authors to state that a follow-up article came from The Institute of Radiophysics.

Helde and Wahlberg worked on the assumption that there was a critical volume for the biological effect of radiation and were therefore able to count on critical dose rates or rather doses for a specific period, the 'dose per second'. Their assumption largely followed that which Lea had stated in his book, but as far as I could see they had made a few calculation errors. However, this was of minor importance. To prove their theory, they had compared the blood count changes with the estimated irradiation and found a good correlation if it was the second dose that was specified. The weakness in the description was that the second dose for the personnel categories that had been studied was estimated rather than measured and that the conclusions of the authors depended on the reliability of one single measurement point. I thought Sievert was right, but the episode did not improve Helde's attitude towards Sievert.

[*] This is dealt with in detail in 'The Sword of Damocles'.

The possibility that the dose rate of the radiation (the dose per time) could influence its biological effect was not at all unreasonable. Sievert himself along with Forssberg had performed experiments to assess this, and the importance of the dose rate was one of the issues that Sievert had hoped would be answered in the high voltage hall. The dependency of the effect of radiation on the distribution of radiation over time was a well-known fact from radiation therapy where this fact was utilised. In 1944, radiologist *Magnus Strandqvist* (1904–1978), who later became Professor at *Sahlgrenska Sjukhuset* in Gothenburg, demonstrated an empirical mathematical connection between the dose of radiation required for a specific level of impact on the skin and the time over which the irradiation is distributed. As a consequence of the repair processes, one and the same dose is less effective if it is spread out over a longer time. In order for the radiation dose rate to also be important, it is necessary, just as Helde and Wahlberg assumed but did not quite manage to prove, to adopt critical volumes and times for rapid repair processes. It is now said that irradiation with a high dose rate can often be 2–3 times as effective as irradiation with a low dose rate.

Sievert had got involved in the Royal Academy of Sciences' (KVA) activities at an early stage. When he took vigorous control of organising the military physics research endeavours of the Swedish physicists in the 1940s, this took place in close cooperation with Manne Siegbahn and the Academy of Sciences' National Committee for Physics. In 1941–1945, Sievert was an executive member of the National Committee and in 1944 he was elected into the Royal Swedish Academy of Sciences (KVA). Thereafter, he was a very active participant in the Academy's work, often in matters which went far beyond radiophysics.

The matter that was closest to his heart was the KVA's efforts towards the research in Upper Norrland. A report on the organisation of the SMHI had proposed the consideration of the possibility of consolidating the geophysical, seismological and magnetic observations carried out at Riksgränsen and Abisko to a meteorological observatory. As a consequence of this, in 1944 the Department for Communications asked the most relevant institutions, i.e., The Academy of Sciences, the SMHI and the Nautical Chart Department to carry out such an investigation. The following year, Sievert took over the chairmanship of the investigation, which was called the Abisko Committee. Scientists like *Hannes Alfvén* (1908–1985), Gustaf Ising and *Harald Norinder* (1888–1969), 'the thunderstorm professor' were elected to the Committee. In his biography of Sievert (Weinberger, 1990), Hans Weinberger writes:

> Sievert was the central figure in the Academy of Sciences' endeavour to remedy the inadequate conditions in Norrland. Sievert was generally interested in aurora borealis research, probably attracted to this through his contact with Hannes Alfvén. As an active member of the Academy he also wanted to contribute his great asset – his entrepreneurial skills. Sievert pushed the matter of the Kiruna geophysical observatory from the initial plans to the final construction drawing by cooperating with politicians both locally and at national level. Sievert succeeded in bringing the Committee together to form a functioning group, despite the quite different backgrounds of the members. The actual investigation work was run largely by The Institute of Radiophysics where regular meetings took place in Sievert's work accommodation under what were sometimes less reserved circumstances. Sievert enjoyed their company and each meeting was concluded with a light meal, beer and schnapps.

The Committee agreed to centralise the activities into one single research station, a 'geophysical observatory', partly for financial reasons and partly because research collected under one roof would be more attractive to foreign scientists. The report that the Committee handed over to the Department for Communications in January 1947 (SOU 1947:6) said:

> The importance of international cooperation in research to a small country like Sweden cannot be overestimated, particularly because, owing to the war, our country was cut off from contact with the rapidly developing scientific activities in the large countries for a long time.

The initial thought had been to place the observatory in Abisko where there was already a natural science station, but it was shown that *Riksbanan's* power cables would disrupt the magnetic measurements too much. The decision was then to choose a location 8 km east of Kiruna.

In 1948, The Academy of Sciences set up an interim board for the research activities in Upper Norrland with Sievert as chair. In 1952 at the suggestion of this interim board, the Department for Communications established a board for the Academy of Sciences' research stations in Upper Norrland. At Sievert's suggestion, the former Prime Minister and Foreign Secretary *Rickard Sandler* (1884–1964) was made chair of the board.

In 1956, in anticipation of the forthcoming Geophysical Year (1957–1958), the government gave the go-ahead for the construction of the Kiruna geophysical observatory. Sievert's genuine interest is shown by the fact that he was chair of a special construction committee for the observatory. In 1961, Sievert took over the chairmanship of the board after Sandler and remained chair until his death. His efforts have been honoured with a portrait of him at the observatory painted by artist Erik Kinell. A replica of the painting hangs in the Radiation Protection Institute's conference room.

Sievert's plans also included the purposeful preparation of a suitable head of the Kiruna observatory. His choice was the capable Bengt Hultqvist who, surprised and thankful, accepted the assignment and began in 1956, to then also become Professor of Space Physics at Umeå University in 1967. After defending his thesis, Hultqvist had felt lost at The Institute of Radiophysics and had complained to me that there were so few people with whom he could discuss scientific matters. He felt isolated. It was then that he received the proposal from Sievert. He has something about this himself (Hultqvist, 1997):

> In Spring 1956, around the time I defended my thesis, Sievert asked me whether I could consider changing scientific subject and concentrating on space physics (or geocosmophysics as it was called at the time) rather than radiophysics, and taking over the management of the new geophysical observatory in Kiruna which was planned by The Academy of Sciences under his leadership and for which the government had just proposed to Parliament that the State ought to make financial contributions.
>
> The proposal suited me quite well. Not only was my situation at The Institute of Radiophysics less than-well-defined following the completion of the Ytong report, the offer of the opportunity to be my own boss was very tempting. I was 28 years old and Sievert was the only boss I had had. It was not his style of leadership that made me feel this way, but the fact that fairly substantial antagonisms always arose between different groups and leading personalities at The Institute of Radiophysics. Plus the fact that in my opinion, the activities at the Institute were not quite to my taste. I wanted to devote myself more to fundamental research, and that is what I would be able to do in Kiruna. […]
>
> For me, Sievert was something of a second father. The interest that he showed in my work and in me as a person was something that I had not come across before. He took me under his wing to some extent and spoke freely about his problems, and about his complexes as he called them. His ability to whet my appetite was incredible. He showed that he had an unusual belief in me. I therefore went to Kiruna with my dear family with hope in my heart. We arrived there on 25 May 1957 and immediately began to prepare for the opening of the observatory which was set for 2 July.

However, Sievert had various other assignments at The Academy of Sciences. In 1952 he became the KVA's inspector for the Swedish Natural History Museum's Department of Entomology. It was not an activity that was completely unknown to him; Sievert collected butterflies as a hobby. In 1953 in his capacity as inspector, Sievert was able to express his opinion in a few uncomfortable personnel conflicts that did not occur without some publicity.

In July 1953, the 7th International Congress of Radiology was held in Copenhagen, a city which at the time gave us Swedes the impression of being a big funfair. One might say that the composition of the group of Danes and Swedes who visited the real Tivoli together after the Congress opened on Sunday 19 July illustrates the fact that radiophysics and medical physics had a stronger position in Sweden than in Denmark. The Danes *Svend Dalgaard*, *Carl Deden*, *Bertel Jørgsholm* and *Olaf Petersen* were all radiologists, i.e., doctors, while of the Swedes – Olov Dahl, Matts Helde, Bo Lindell, Lars Lorentzon, Rune Walstam and *Karl-Johan Vikterlöf* – only Dahl was a radiologist, the rest of us being radiophysicists. Vikterlöf was initially a radiation protection inspector under Thoraeus, then going on to develop treatment methods for rotational irradiation with Olov Dahl and later becoming a leading medical physicist in Örebro. I have saved the targets from the shooting range and see that we won the shooting competition against the Danes.

Immediately before the Congress, a 'Secret Conference' was held at the Finsen Laboratory concerning a study that had been ongoing for more than three years and whose results were now to be reported. The study concerned the hazardous consequences to which the use of the x-ray contrast agent Thorotrast had been shown to lead. This contrast agent consisted of a colloidal solution of thorium dioxide. It was an excellent contrast agent and primarily had a much gentler effect on the patients than the alternative iodic contrast agents when it came to blood vessel examinations. However, once it had been injected into the blood vessels, it was excreted slowly and could thereby damage organs such as the liver. Around 1940, Thorotrast was the dominant x-ray contrast agent in blood vessel studies when performing a cerebral angiography, for example, i.e., examining the blood vessels in the brain. It had been used from 1929 in Portugal, from 1930 in Japan and Germany and from 1932 in Sweden and the USA. The risk of injuries was known right from the start but was initially underestimated, so the consensus was that the benefits outweighed the risks. Once it eventually became clear that the risks had been incorrectly assessed, the use thereof fell at the start of the 1940s and ceased in Sweden at the end of the 1940s at the same time as the first cases of cancer were reported.

The Danish study took place in cooperation with the British Medical Research Council (MRC) and the Harwell Atomic Research Centre south of Oxford. This cooperation later aroused suspicions that it was the reason why the Danes were very secretive about the Thorotrast study and that the British military interests were involved. There may be a grain of truth in this because one important aspect of radiation protection when manufacturing nuclear weapons and handling nuclear fuel is protection against plutonium, which is hazardous through its alpha radiation. At the end of the 1940s there was very little experience of the risks from the alpha-emitting substances in the body and the interest on the part of the British in the Thorotrast project was understandable. However, this did not really give any major grounds for secrecy. It is more likely that the Danes were eager to defend the duty of confidentiality of the doctors when it came to their patients. Their view on openness, 'It can only harm patients …' was in line with their general view because doctors at that time were very restrained with giving information to the patients. Patients were often not informed of the diagnosis of cancer, for example.

A large group from Sievert's institution participated in the Congress on Radiology. Apart from Sievert himself and the group of physicists from the Tivoli visit, Sven Benner, Lars-Eric Larsson and Robert Thoraeus also went to Copenhagen. The big Swedish radiotherapists were there of course: Elis Berven, Bertil Ebenius, *Gunnar Gorton*, *James Heyman* (1882–1956), Sven Hultberg, his Ludvig Kottmeier, *Martin Lindgren* (1910–1988) and Magnus Strandqvist, and the well-known names among the x-ray diagnosticians included *Sven Roland Kjellberg*, *Folke Knutsson* (1901–1993), *Knut Lindblom*, *Erik Lindgren* (1905–2005), *Wolfgang Magnusson* (1898–1982), *Olof Norman* (1911–1997), *Olle Olsson* (1911–1999), *Carl Wegelius* (1905–1988), *Sölve Welin* (1903– 1994) and *Åke Åkerlund*. There were obviously also Sievert's colleague from Lund Kurt Lidén as well as industry representatives such as *Georg Fredzell* (1919–2002, Georg Schönander AB), *Stig Grim* (Järnhs Electriska AB), *Bror Edvard Järnh* (1879–1956) and his son *Bertil*, *Albert Magni* (Elema-Järnh), *Nils Georg Schönander* (1894–1958), *Einar Wastenson* (Philips) and *Gustav Weber* (Elema).

Other countries contributed through many celebrities, particularly in the radiation protection field because ICRP and the sister commission ICRU* were to meet in Copenhagen during the Congress. A few names need to be mentioned. It may seem like a bit of a long list to go through, but on the other hand it may be interesting to have an overview of the key people who were active within radiology and radiation protection at the start of the 1950s. I have still left out a number of very prominent radiologists

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* The International Commission on Radiological Units and Measurements was also a committee that was established by the International Congresses on Radiology. The word 'Radiological' has now been replaced by 'Radiation'.

because I did not think they played any decisive role in my story seen from the radiation protection point of view.

The participant from Austria was Dr. *Jaroslav Zakovsky*, the head of the x-ray experiments centre in Vienna. From Belgium Professor *Zenon Bacq*. From Canada Dr. *André Cipriani* and Professor of Radiophysics *Harold Johns* (1915– 1998). From Denmark Dr. *Børge Christensen*, Professor George de Hevesy, Professor *J.C. Jacobsen*, Dr. *Hilde Levi*, Professor *Carl Krebs*, Professor *Flemming Møller*, Dr. *Jens Nielsen*, Professor *Flemming Nørgaard* and the head of radiation protection *Paul Rønne-Nielsen*. From Finland Professor *Sakari Mustakallio* (1899–1989). From France Dr. *André Allisy* (1924–2017) and Professor *Antoine Lacassagne*. From Germany Professor *Joseph Becker*, Dr. *Hans von Braunbehrens*, Dr. *Paul Dax* from Siemens-Reiniger-Werke in Erlangen, Professor *Hermann Holthusen* (1886–1971), Dr. *Walter Hübner*, Dr. *Robert Jaeger*, Professor *Richard Kepp* in Göttingen, Professor *Boris Rajewsky* (born in 1893), Dr. *Friedrich-Ernst Stieve* and Dr. *Felix Wachsmann*. From the United Kingdom Mr. *Walter Binks*, Sir *Ernest Rock Carling* (1877–1960), Dr. *Frank Ellis*, Mr. *Paul Howard-Flanders* (born in 1919), Dr. *Alan Jennings* (1923–2016), Dr. *John Loutit*, Professor W. V. Mayneord, Professor Joseph Mitchell (1909–1987), Mr. George Newbery, Dr. Ralston Paterson (1897–1981), Dr. *E.E. Pochin* (1909–1990), Professor *Joseph Rotblat* (1908–2005), Dr. *Warren Sinclair* (1924–2014), Mr. *Eric Smith* (1911–1998), Professor F. W. Spiers, Dr. Denis Taylor from Harwell, Dr. *Bernard Wheatley*, Professor *Brian Windeyer* and Dr. *Constance Wood*. From the Netherlands Dr. *Wybe Oosterkamp*. From Italy father and son Professors *Felice Perussia* (1885–1959) and *Aldo Perussia*. From New Zealand Mr. *George Roth*. From Norway Dr. *Finn Devik* (1916–1985), Dr. *Nelius Moxnes* and Dr. *Erik Poppe*. From Switzerland Professors *Hans Rudolf Schinz* and *Adolf Zuppinger*. From the USA Dr. C*arl Braestrup* (born in 1897), Professor *Austin Brues*, Professor *Simeon Cantril* (1908–1959), Professor *Richard Chamberlain* (1915–1975), Professor *Gioacchino Failla*, Mrs. *Patricia Failla* (1925–), Dr. *Alexander Hollaender*, Dr. *John Laughlin* (1918–2004), Dr. *Maurice Lenz* (1890–1974), Dr. *Leonidas Marinelli*, Dr. *Karl Z. Morgan* (1907–1999), Professor *Russell Morgan* (1911–1986), Dr. *Edith Quimby* (born in 1891), Professor *Robert Stone* (1895–1966), Dr. *Lauriston Taylor* (1902–2004), Dr. *E. Dale Trout* (1901-1977), Dr. *John Trump* (1907–1985) and Dr. *Harold Wyckoff* (1910–1999).

On Monday 20 July, the Nordic radiophysicsts were invited to Rønne-Nielsen's home for dinner and gained an insight into the development of radiation protection in Denmark. Radiophysicist Paul Rønne-Nielsen was Assistant Professor at the university's biophysics laboratory and in practice was responsible for radiation protection in Denmark (see Chapter 14). On this occasion, we Swedes met *Kristian Koren* (1911–1990) for the first time, someone who would go on to succeed Moxnes.

The International Congress of Radiology in Copenhagen in 1953 is remembered mostly for what happened during the ICRU and ICRP meetings, which are described in a later Chapter. During the Congress, Sievert gave a lecture in English with points of view on the organisation of radiation protection. Among other things, he said:

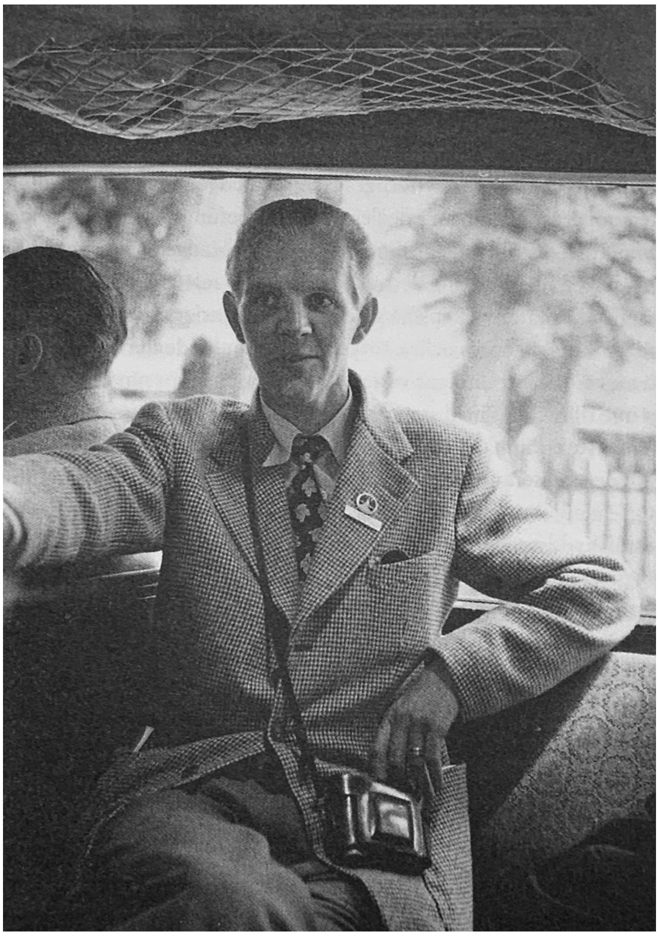
> […] One of the most important questions is: What amounts of ionizing radiations really are dangerous? We are aware that the answer to this question is dependent on whether we refer to actions on the skin, blood-forming organs or gonads, or to genetic effects. It is, however, still impossible to give any fixed data for threshold doses that will indicate the level at which the effects of radiation on man actually become dangerous in the various conditions met with in practice. Nor do we know how the doses which will be used as the highest permissible ones depend on the distribution of the radiation with time. […]
>
> There are reasons for being extremely cautious with ionizing radiations, since their effects have been observed in most cases to be delayed and cumulative, and at times to be capable of producing cancer. But we really do not know to what extent these observations are applicable when the human body has received very small doses with varying dosage rates. Many investigations also seem to indicate that individual resistance to small irradiations varies considerably, but at present there are insufficient statistical data to give a clear picture of this matter.

We must confess that our present knowledge of the action of small amounts of ionizing radiations on man is very scanty, and by no means sufficient to form a satisfactory basis for practical radiation protection work. […]

Radiation protection specialists are now often forced to give data and other information based on very weak foundations. They therefore have to apply large safety margins and have, indeed, been fortunate that, in medical radiology, cases of severe injuries during the past have not been very frequent.

We now have to face a new situation. It is no longer only a question of reducing the hazards by decreasing the irradiation of personnel as much as possible. We also must be ready to permit as much irradiation as, according to our knowledge of radiation hazards, does not involve risks of serious injury. This is very important if we are not to delay development in many fields, not least with regard to the use of atomic energy. […]

I am not quite sure whether or not I am right, but I think it is essential always to try to maintain a reasonable balance between the aim of the work and the radiation hazards to be permitted. In this respect radiation hazards do not differ from other hazards. In many fields we have to realise that we cannot go forward without paying something for our progress.



Rune Walstam on a bus journey at the Congress on Radiology in Copenhagen in 1953. Photo: Bo Lindell

3. THE HIGH-VOLTAGE THERAPY BREAKTHROUGH

The discovery of nuclear fission did not just lead to atomic bombs; the accessibility of new, unstable nuclides led to new sources of radiation that could be used for diagnostics and therapy in medicine. The search had already started for medical applications in the first few years following the Joliot-Curie production of the first artificial radioactive substance, the short-lived phosphorus-30. George de Hevesy along with Austrian chemist *Friedrich Paneth* (1887–1958) had determined the solubility of lead sulphide and lead chromate in water with the help of natural radioactive lead isotopes as early as 1913. They then showed that it was possible to use additives of radioactive substances as 'trace elements' to measure chemical reactions of extremely small quantities of the reacting substances, way below that allowed by conventional analysis methods. This laid the foundations for the trace element methodology for which de Hevesy won the Nobel Prize for Chemistry in 1943.

During the war, access to artificial radioactive substances outside the USA was obviously extremely limited. However, in 1945, radiologist *Folke Jacobsson* (1913–1984) at *Radiumhemmet* wrote an article in *Swedish Läkartidningen* on therapeutic experiments with radioactive nuclides. At the time, such nuclides had mainly been produced using *Ernest Lawrence's* (1901–1958) cyclotron in Berkeley. In 1938, Ernest's brother *John Lawrence* (1904–1991) and his colleagues at the University of California had started to treat leukaemia with radioactive phosphorus (phosphorus-32 with the half-life of 14.3 full days). Radioactive phosphorus, like all phosphorus, is enriched in the bone tissue. When it decays there it emits only beta radiation which irradiates the blood-forming bone marrow. Erik Lindgren described the treatment of a few patients with radioactive phosphorus in Sweden in 1944; experimental treatments were taking place at *Radiumhemmet* at the same time. Phosphorus-32 was also used in the form of radioactive plaques for application to the skin. At the Nordic Society for Radiology's 30-year jubilee in Stockholm in 1949, Magnus Strandqvist gave an account of experimental treatments with phosphorus-32 while Agnar Egmark of The Institute of Radiophysics was stating the risks of working with radioactive substances.

One example of the risks has been described by the author *Lars Gyllensten* (1921–2006), who was working on his doctoral thesis at *Karolinska Institutet* at around the same time. In the study of the importance of the thymus for the development of the immune system of new-born mice, rats and guinea pigs he used phosphorus-32 as a trace element. Phosphorus-32 emits a very high-energy beta radiation (the maximum energy is 1.7 million electron volts (MeV)), which has a range of several millimetres in body tissues and several metres in air. You must always be very careful when handling this substance. Gyllensten has written about his use of phosphorus-32 in his memoirs (Gyllensten, 2000):

> In the studies on the growth processes in the system, one of the things we used was labelling with radioactive phosphate which is taken up in growing tissues. I could sit for several hours a day and prepare guinea pigs that had been injected with such phosphate. We used doses that would now be considered to be quite high – in the end I referred to it as 'filthy radiation'. I eventually noticed that the small hairs on my fingers were disappearing and realised that this was caused by the radioactivity. This led to my interest in the biological effects of exposure to radioactivity and I read a great deal about these conditions – studies that led me to make a public stand against nuclear weapon armaments and nuclear weapons testing.

After the end of the war, nuclear reactors also became available for the production of radioactive nuclides. These could be used in medicine for both diagnostics and for radiation treatment and 'isotope laboratories' were set up in the hospitals. People used to talk about 'radioactive isotopes' (of the element that you were interested in), when they actually ought to have referred to radioactive nuclides. The activity that flourished with 'isotope diagnostics' eventually quite rightly ended up being called nuclear medicine. The first deliveries of radioactive nuclides to Sweden came from Harwell in England where the commercial production of radionuclides began in 1947. The activity was taken over by the Radiochemical Centre in Amersham in 1959. After that, it was also possible to receive radioactive nuclides from the Norwegian Halden Reactor (in operation in 1958) and the research reactor R2 in Studsvik (in operation in 1960).

The nuclide that initially aroused the greatest interest was iodine-131, which has a half-life of 8 full days. Our body normally contains around 25 milligrammes of stable iodine (iodine-127), approximately half of which is found in the thyroid gland. If you administer iodine-131 intravenously or by mouth, the nuclide always searches for the thyroid gland first where it emits its radiant energy, mainly beta radiation. However, iodine-131 also emits gamma radiation, which means that it is possible to show the radioiodine in the thyroid gland by means of various types of detectors outside the body, and also measure the quantity thereof. An important examination to perform with iodine-131 is therefore to measure the uptake to see whether the thyroid gland reacts normally. If you administer iodine-131 with a high rate of activity, i.e., many radioactive disintegrations per second, the delivered radiation energy can affect the function of the gland. This took place at an early stage for treatment of an overactive thyroid gland (thyreotoxicosis, also called 'hyperthyroidism'). One form of thyreotoxicosis is Graves-Basedow's Disease, which is also characterised by protruding eyes (exophthalmos). Thyroid gland cancer was also treated with iodine-131 at an early stage.

Sievert saw the development with some concern. His institution was responsible for radiation protection in accordance with the 1941 Radiation Protection Act – what would happen if the new radioactive substances were made available to all doctors? The use of x rays was strictly regulated and the sources of radiation were concentrated mainly at the hospitals, and then in specialist departments. The head of *Radiumhemmet,* Elis Berven, was keen to see radiation treatment centralised. In his opinion and that of many others, not until then would it be possible to maintain the high level of competence. Individual doctors and doctors at smaller hospitals would never get to see enough different types of tumour to be able to maintain a high level of competence. Sievert supported Berven's view; the existing tradition of centralisation facilitated the supervision of radiation protection.

But not everyone agreed. The private doctors and doctors at smaller hospitals who were interested in the new opportunities for diagnostics and treatment using radioactive substances opposed it; the one who was the most opposed of them all was the Chief Physician at *Centrallasarettet*, the central general hospital in Växjö, *Adolf Lindblom* (1898– 1973), who wanted to start radiation therapy using radioactive nuclides in 1949.

Sven Benner had tried to discourage him from applying for permission for this, but Lindblom did not want to follow the advice. The following events highlight the problems that existed during the transition period at the start of the 1950s. In a letter to Benner in October in 1949, Lindblom writes:

> […] Regarding your well-meaning advice to refrain from these treatments, I obviously cannot follow it. The experiences that I have gained thus far with these radioactive isotopes are of such quality that even the central hospital must start these treatments sooner or later. The most important area is not cancer therapy but primarily certain internal conditions, particularly specific blood diseases such as types of erythrocythaemia, leukaemia, etc. and centralising this treatment to the radiological clinics would be completely unthinkable from a technical point of view. Were we to send all of our lymphatic leukaemias which are currently treated at the central general hospitals throughout Sweden to the three radiological clinics, they would become crowded out with these patients alone and it would not be possible to take any other cases for some considerable time. I am keen on radioactive iodine at this particular moment because of a case of *struma maligna* [thyroid cancer] for which the only treatment is radioactive iodine. However, I do believe that the treatment with radioactive iodine will become common mainly in cases of Graves-Basedow, and were the experiences that exist in England regarding the Graves-Basedow treatment with radioactive iodine to prove to be particularly beneficial, all cases of Graves-Basedow would sooner or later be treated with radioactive iodine. The most important field of

work will obviously be radioactive phosphorus, which is currently the most widely used in a number of internal diseases. I am most certainly suggesting that it is not only our right but also our absolute duty to start this form of treatment as soon as possible. […]

With best wishes,
Yours affectionately,
LINDBLOM
P.S. Please tell our friend Elis that he should not lose any sleep over my isotopes.

'Elis' was of course Elis Berven. Benner, who was a conscientious man, travelled to Växjö on 7 December 1949 to inspect the work at *Centrallasarettet*. He found that, despite seeing little use, the workplace at which the radioactive substances were prepared suffered from heavy radioactive contamination. He also found that the measurement instruments were not functioning satisfactorily and could lead to substantial measurement errors. In March 1950, The Institute of Radiophysics gave its opinion on Lindblom's application to be permitted to use artificial radioactive iodine and phosphorus:

Since at the workplace in question there are no available personnel who have been trained in taking nuclear physics measurements and who have experience of such measurements in connection with internal treatment using radioactive substances, with reference to the importance of accurate measurements at the time of such treatments in order to ensure the correct dosage and prevent radiation damage, The Institute of Radiophysics is unable to approve this application.

On the basis of Benner's statement, the Medical Board rejected Lindblom's application. An angry Lindblom then rang Sievert and complained. He then wrote to Sievert on 11 September:

A complaint about the decision […] has been sent to the King. At the same time, we have asked the King for a comprehensive report on the need for isotope treatment in the country, in which we have particularly emphasised the fact that we think the Central General Hospitals ought urgently to be given the opportunity to use this excellent form of treatment. Centralising it to the Jubilee Clinics* is not possible for several reasons. […]

So far, a good 10 human lives have been saved thanks to isotopes, mainly in cases of leukaemia where all other forms of treatment have been exhausted. The result has been so fantastic that there is absolutely no doubt that the treatment has as such saved lives. So, we think it is impossible to forego this form of treatment without considerable detriment to the healthcare services. […]

I was even more surprised when you maintained that isotope treatment would not be appropriate since I lacked the competence to take care of similar treatment. You said that I was not practised in radium work. First of all this is incorrect, and secondly I must ask myself whether you are able to refer to any section of a law which entitles you to interfere in my field of competence. As far as I am aware, it exclusively concerns the Medical Board. It would be rather regrettable if The Institute of Radiophysics were to interfere with our competence. This would open the sluice gates for full policing on the part of The Institute of Radiophysics. Maybe The Institute of Radiophysics also wants to determine our dosing or our determination of the quality of radiation when treating various diseases, etc.? If such requirements were set by The Institute of Radiophysics, we radiologists would unite to oppose it. […]

* The Jubilee Clinics were the specialist cancer clinics in Stockholm (*Radiumhemmet*), Gothenburg, Lund and Umeå, which had come about with the help of funds from King Gustaf V's Jubilee Fund, which had been formed in 1928 to celebrate the King's 70th birthday.

> You said that you will try and prevent our work with tooth and claw. I can assure you that I will expend twice the amount of energy to obtain the licence I need. I see it as my duty as a doctor.

Sievert responded on 14 September 1950:

> […] Our inspection has shown that it is not possible from a radiation protection and dosage point of view to consider the work with radioactive isotopes undertaken by you are to be satisfactory.
>
> I also mentioned my personal view during my telephone conversation with you, which means that while there are no results of isotope treatment based on long-term, routine examinations, I think it is necessary to restrict isotope therapy to a few hospitals in Sweden where specialist personnel are based. It is obvious that for the time being, this form of treatment ought to be managed only by doctors who are conversant with therapy using radioactive substances, or else isotope therapy in Sweden may undergo a particularly adverse development. […]

Lindblom's response to this was 'I can only interpret this as an attempt by The Institute of Radiophysics to put a spanner in the wheel of our work and hinder the isotope work here at any price'. He was doubtless right in that Sievert begrudged any therapy with radioactive iodine and phosphorus in Växjö and that Sievert's view was supported or influenced by Berven, who did not approve of such activities outside the Jubilee Clinics.

On 10 October, following further talks with Sievert, Lindblom gave way. He sent an application to the Medical Board for a licence to do diagnostics using radioactive substances. When this application was granted on 26 October, Lindblom recalled his letter to the King. In September 1951, he announced that the radiophysics expert who would be relied on in future was Dr. of Philosophy *Arnold Guntsch* (1909–1982) in Växjö. The intention behind that move was to remove all the problems.

However, in October 1951, Sven Benner received details from a couple of doctors who wanted to remain anonymous showing that, although he had a licence only for isotope diagnostics, Adolf Lindblom was actually carrying out treatment using radioactive substances. A report showed that Lindblom was completely innocent. He had carried out diagnostic examinations of patients who were undergoing treatment on the x-ray therapy ward.

When nuclear medicine had become more established in Sweden, in August 1958 Adolf Lindblom finally obtained a licence to perform both diagnostics and therapy using phosphorus-32, iodine-131 and gold-198. An isotope committee was also eventually established at the general hospital to monitor the work.

As well as in accelerators such as Lawrence's cyclotron, it was possible to produce radionuclides in reactors by means of irradiating suitable stable nuclides with neutrons or as fission products in the reactor. In a report from the University of California in April 1950, Joseph Hamilton discussed twenty-two cyclotron-produced radionuclides and conceivable uses. Hamilton pointed out that the strontium isotope strontium-85 (half-life 65 full days) was suitable for studies of the metabolism in the skeleton because there was no suitable calcium isotope. He also warned against the use of the fission product strontium-89 (53 full days) because there was also a risk of obtaining the very long-lived strontium-90 with this.

The gamma-emitting nuclide which was the most suitable for irradiation from a distance, cobalt-60, could only be produced if there was a high enough activity in reactors with a very high neutron flux density, as was the case with the NRX reactor at Chalk River in Canada. Cobalt-60, and later caesium-137, a nuclide that is formed as a fission product in the reactors, would eventually replace radium, mainly for external radiation treatment but also in a large number of preparations for interstitial and intracavitary treatment. This option was discussed at *Radiumhemmet* at the start of the 1950s, but at that time a more accessible alternative was to use the high-energy radiation from accelerators or other million-volt devices. Sievert, who always endeavoured to make the impossible possible, saw this as a way of paying for a study trip to the USA for me, actually for the purpose of obtaining tips on how the x-ray tube in the high voltage hall could be made to work but, in order to facilitate the financing, also for the purpose of studying the available million-volt device.

There is no way that anyone who was not born in the 1920s could imagine what a trip to America meant just after the Second World War. Before the war, we 20 year-olds were too young to contemplate travelling to America (Jan Myrdal was one exception) but heard others recounting information about New York and Hollywood, about the American films they saw and books they read about the big country in the west, so far away that a journey by boat took at least ten days. Then along came the war and Sweden became an isolated island in war-stricken Europe. America was as inaccessible as the moon or Mars - a fantasy, a dream. During the first few years with Sievert, at the lunch table in *Karolinska Sjukhuset's* doctors' dining room I heard Professor Heyman recounting the trips to America that he had resumed to visit his daughter following the war. It was like listening to an astronaut recounting a journey to the moon. And I was about to go there! It was difficult to grasp, and the feelings I had would be unfathomable to people who were born after the war, people who do not see much difference between going to Florida and going the Canary Islands, and who know that it can sometimes be cheaper to fly to New York than it is to fly to Paris.

I travelled from Gothenburg on 6 September 1951. A Stockholm newspaper contained the following (with the high incidence of errors that you often find when something is written about something that you are familiar with yourself:

> The Swedish American Line's *Stockholm* left fully loaded with passengers on Thursday morning for New York via Copenhagen. [… From] *Karolinska Institutet* in Stockholm, Mr. Bo Lindell is travelling to study and purchase instruments for research into radiophysics and radio psychology and finally, the passengers also include the well-known Swedish middle distance runner Alf Holmberg.

M/S Stockholm was rather a small ship that carried no more than 400 passengers. At the time, it had no stabilisation devices and always rolled heavily in the swelling waves. Yet I found the Atlantic a bit of a disappointment since it never gave the impression of being an enormous ocean; let's face it, you can't see beyond the horizon. On the evening of Friday 14 September we glided in between Long Island and Staten Island and laid eyes on the pearl necklace of lamps along the beach pathways, and this is where we anchored overnight. By the light of dawn we could see the skyscrapers at the southern tip of Manhattan and the far-away glow of the Statue of Liberty, not nearly as dominating as I had anticipated.

A man from General Electric met me on the pier while I was viewing the unfamiliar sight of a throng of people, tooting cars, swearing dockworkers and tall buildings with wide eyes. He followed me to the hotel and promised to return after the weekend. On the Saturday evening I was wandering around on The Broadway and in Times Square, observing just how different things were compared to the tranquillity of Stockholm. I wrote home, astounded:

> There was a great throng of people wandering around, just like at *Gröna Lund* amusement park on a Saturday evening. As the reputation goes there were incredible numbers of neon advertising lights, but not arranged as a Swede might expect; there were a whole load of small neon lights as well as the large ones: it was not a street of illuminated advertising but a hubbub of entertainment. There are bars all over the town, almost one in every other building, and where there are no bars there are fruit shops or wine stores or there is some other outlet selling food or drink. People are constantly eating and drinking. Not only that, all of a sudden you can pass a shooting range that leads straight off the street just like at a Swedish funfair, or there is also an amusement arcade with no exterior walls facing the road, just as if you were at a funfair.

On Sunday I took a taxi down to Battery Park on the southern tip of Manhattan and wandered from there along Broadway up to Central Park, a 15-km walk including short diversions down side roads to get a feeling for New York. On the Monday morning, the man from General Electric came and guided me to Columbia University Medical Centre on West 168th street where I was to meet Professor Failla, who had promised Sievert that he would draw up a travel plan for me.

Failla was a Sicilian. When his father had died in his younger years, his mother took him to New York in 1906 when he was fifteen years old. He graduated from Columbia University in 1915 after doing a course in electronic engineering and came to the Memorial Hospital in Manhattan as a medical physicist with the task of developing methods for the use of radon as a medical radiation source. In 1916 he became an American citizen and, after having been a scientific attaché at the American Embassy in Rome as the First World War was coming to an end, he came to study under Marie Curie in Paris where he defended his doctoral thesis in 1923. He then returned to the Memorial Hospital where he remained until 1943, becoming Professor of Radiophysics at Columbia University where I visited him. By then, Failla was generally respected as a proficient and discerning scientist.

In my letter home I wrote: 'Failla is in his sixties, a very discreet and reticent but droll and calm man. Mrs. Failla is around twenty-five years old.' Sievert had warned me about the age difference between Failla and his wife Patricia. Sievert had said that Patricia had been one of Failla's students when he was a widower, and when they fell in love, Failla was so concerned about the age difference that he demanded that they wait for a year to see whether their feelings would remain strong. They had married after that.

Failla invited me home to a very pleasant lunch and then insisted on driving me to the nearest underground station. When we parted I thanked him for his hospitality. 'Don't get any ideas into your head,' said Failla, speaking extremely frankly. 'I didn't invite you for lunch on your own merits so you have nothing to thank *me* for. I hardly know you. I invited you as a way of acknowledging Doctor Sievert, so he's the one you need to thank, for the lunch and for the esteem he shows you. Sometime in the future, once I've got to know you, I might invite *you* to lunch.'

Failla was also hospitable at his workplace and spent a lot of time on me. He let me meet his colleagues and principally *Harald Rossi* (1917–2000), with whom I would later become close friends. But this was the first time I met Rossi and I made the mistake of thinking that he was the well-known *Bruno Rossi* (1905–1994) who had written books on measurement instruments and ion chambers. When Failla introduced me to Rossi, I therefore said that I knew of his books. 'Hardly,' said Failla drily, 'no-one has heard of this Rossi.' It was not exactly a flattering remark about poor Harald, but honesty was one of Failla's virtues.

I discussed Sievert's large x-ray plant with Failla and Rossi. Failla thought that it would be completely impossible to get the discharge tube to function for voltages higher than 300 000 volts (300 kV). He said that General Electric, having gone to a great deal of trouble a long time ago, had found out that it was impossible to make high voltage tubes with single potential differences for voltages higher than 300 kV. Above that, you had to partition the voltage, but Failla thought it would be difficult to get this to function. Lawrence in California had indeed managed to reach higher voltages, but it had taken him a great deal of trouble and it was not useable in practice. Copper was not suitable – stainless steel was needed. It was all going to be very expensive and Failla suggested that we get hold of a betatron instead and carry out the biological experiments using beta radiation.

The travel plan that Failla put together for me included visits as far out west as Minnesota and a total of sixteen institutions. It meant that I would be able to study the same device in several different places because, in practice, there were really only three different million-volt devices to choose from: General Electric's resonance transformers, Allis-Chalmers betatron and High Voltage Engineering's Van de Graaff generator. General Electric certainly did also produce a 15 MeV betatron but it was not thought to have any advantages whatsoever over the Allis-Chalmers betatron. General Electric was also in the process of supplying Dr. Robert Stone in California with a 70 MeV synchrotron, but the device was not exactly appropriate for *Radiumhemmet* because it was still at the experimental stage. I knew that a General Electric 30 MeV synchrotron had been installed a few years ago for Professor J.S. ('Joe') Mitchell in the radiotherapy department at Addenbrookes Hospital in Cambridge, but it was also one of the experimental devices and was not available on a commercial basis.

Why were we so keen to obtain million-volt devices for *Radiumhemmet*? In order to understand this, you need to comprehend the inadequacies of the sources of radiation that already existed there for the radiation treatment of cancer. They were the traditional ones – x-ray devices and radium preparations.

I have described the different forms of radiation treatment using radium in 'Pandora's Box'. It was mainly brachytherapy, i.e., contact treatment (the Greek *brachys* means short), either with the help of encapsulated radium preparations placed on the skin or as interstitial radium treatment (needles in tumour tissue) or intracavitary treatment where the preparation was fed into body cavities. Brachytherapy was thus limited to superficial tumours or tumours next to body cavities. With a device for teleradium treatment, you used several grammes of radium in a 'radium gun' for the purpose of penetration capacity deeper into the body at the same time as benefitting from the fact that the gamma radiation was not as strongly absorbed by the bone tissue as the softer x rays. Because the quantities of radium were relatively small due to its high price, the treatments lasted a long time and you were still obliged to keep the radium fairly close to the patient's body. As the intensity of the radiation from a geometrically small gamma-emitting preparation decreases in inverse proportion to the square of the distance ('the inverse square law'), you could, even though the diameter of the source of radium could be as great as 6 cm, still only treat fairly superficial tumours, mainly in the ear, nose and throat area.

So, the distance was what posed the big problem when using radium. With x rays, this was not the main problem – here, the intensity of the radiation was so high that you could use a greater distance and in doing so reduce the influence of the inverse square law. Instead, the lesser penetration capacity of the x rays was what made the treatment of deep-lying tumours more difficult. Deeper down, the radiation dose became significantly less than on the skin where the radiation entered the body. You could therefore not give a deep-lying tumour a high enough dose of radiation to be able to destroy it without irradiating it so strongly that the skin became damaged.

It was always a matter of finding new treatment devices with a radiation that had a good penetration capacity and a sufficiently high intensity to be able to use large treatment distances and reduce the influence of the inverse square law. The x-ray photons were expected to have the maximum penetration capacity at energies around ten of million electron volts (MeV). Such high-energy x rays can be obtained as 'Bremsstrahlung' from electrons that have been accelerated by a potential difference of 20–30 million volts (MV) as opposed to the peak voltages of 200–250 thousand volts (kV) which were used in common therapy x-ray devices. Such high acceleration energies can only be reached in special accelerators, which meant that the Allis-Chalmers betatron with 24 MeV acceleration energy was of particular interest. With a 24 MeV electron energy, the average energy of the x rays was 8 MeV, which could be considered to be pretty much ideal.

But there were benefits to be had even from more moderate energies. When the x-ray photons collide with a body and lose energy, some of the energy will be transferred to secondary electrons. For normal x rays with photon energies of less than 200 keV, the range of the secondary electrons is so small that the energy that is absorbed is not led away from the primary ionisation area. The maximum dose of radiation is always received in the skin. However, at high photon energies, the secondary electrons will carry away some energy towards the depth of the body, thereby sparing the skin. The maximum dose with are x rays from electrons with a few MeV of acceleration energy is received at a depth of a few millimetres.

With very high acceleration energies, this build-up of the depth dose from secondary electrons will place the maximum dose at a depth of a few centimetres at 24 MeV of acceleration energy and at ten cm for 100 MeV. However, the very high acceleration energies bring new problems, including the fact that the radiation can induce radioactivity and release neutrons.

In my opinion, the least attractive but most tried and tested treatment device was General Electric's resonance transformer. This contained a synchron electrical motor which powered an alternating voltage generator which generated 180-cycle alternating voltage. The generator was connected to the primary circuit of a high voltage transformer by means of a series condenser. The primary winding of the transformer and the condenser formed an oscillating circuit that was tuned to be in resonance with the alternating voltage of the generator. Thanks to the resonance, the voltage over the primary winding of the transformer exceeded the voltage from the generator by a considerable margin. The voltage over the secondary winding of the transformer was thereby transformed up to high values.

The x-ray tube, shaped as a long acceleration cylinder, had acceleration electrodes that were connected to sockets on the secondary winding of the transformer, which was easy to arrange because the tube was positioned inside the transformer tank. The earliest devices could accelerate the electrons with a voltage of 1 MV towards a transmission anode in the end of the x-ray tube. The first 1 MV device had been delivered to the famous Memorial Hospital in eastern Manhattan in 1938 and had been in operation since 1939. Unlike subsequent designs, it had an open x-ray tube that required continuous vacuum pumping. Another device with a closed x-ray tube had been used since the new year of 1950.

The medical physicist who showed me around the Memorial Hospital was called *Antonio Ferlazzo*. Where the resonance transformers were concerned, he quite rightly pointed out that the high voltage of 1 MV gave a misleading picture of the radiation quality. Most of the x-ray photons had a considerably lower energy than 1 MeV (one million electron volts), and it was only the peak voltage that was 1 MV – the voltage in the resonance transformer does pulsate. Ferlazzo said he preferred a 'cobalt gun'. One such gun was also being installed and would have a radiation source of 1400 curies of cobalt-60 from Chalk River in Canada, the only supplier of cobalt-60 in such compact form for the source to have sufficiently small dimensions. A diameter of 1.5 cm thought Ferlazzo.

The Memorial Hospital was also *en route* to getting an Allis-Chalmers betatron. Ferlazzo estimated that the building for this would be double the cost of the device itself. The procurement expense, so I was told, were 80 000 dollars for the resonance transformer and 120 000 dollars for the betatron.

At the Danish-born medical physicist Carl Braestrup's premises at the Francis Delafield Hospital on 163st street, not far from Failla's institution, I was shown the latest version of the resonance transformer, now for 2 MV. Another 2 MV device was temporarily in use at the Hospital of Joint Diseases, where I also had the opportunity to see it. At both of these places, the intention was to use the device for rotational irradiation and in both places the Malmö radiologist *Inge Gynning's* (1914–1986) article in *Acta radiologica* on rotational irradiation was ready for studies. Delafield had also ordered a cobalt gun with a radiation source of 1200 curies of cobalt-60. It would be supplied by the Canadian firm Eldorado Mining and Refining Co. The whole plant, including the installation, was estimated to cost 41 000 dollars.

I visited the well-known Machlett x-ray tube factory on 18 September. Proof of Sievert's worldwide influence was that I, an unknown pup of twenty-nine, was received by Mr. *Raymond Machlett* (1900–1955) himself and got to eat lunch with him and all of the leading men in his company. I was then shown around the factory.

Machlett was an old family company based on the artisan skills of a German glass blower, *Ernst Machlett*. E. Machlett & Son manufactured x-ray tubes but had problems in the 1920s owing to Coolidge's patent and monopoly on hot cathode ray tubes*. Ernst's son *Robert Machlett* (1872– 1926), who ran the firm, died of radiation injury in 1926. His grandson Ray Machlett, the person who looked after me, then formed a company for the production of neon tubes, the Rainbow Light Company, but was persuaded in 1931 to return to the manufacture of x-ray tubes. He formed Machlett Laboratories for this purpose, which moved to Springfield in 1934.

For the time being, Machlett did not manufacture any tubes for either General Electric's resonance transformer or the Van de Graaff generator which I would soon get to see at the Massachusetts Institute of Technology (MIT) outside Boston. On the other hand, the company did manufacture the acceleration tubes for the Allis-Chalmers betatron. At Machlett, they thought it was definitely superior to the General Electric equivalent. The betatron tubes were manufactured on a fairly artisan basis in a two-storey wooden villa next to the factory.

I asked Machlett's engineers about the problems with Sievert's discharge x-ray tube. They said they had no experience of such a thing but added that tubes over 400 kilovolts with a single potential difference were never made.

On the Thursday evening on 20 September, I took the train to Boston and got to experience the ingenious invention that was called a 'roomette', a cabin for one person* where in the evening you could transform your seat into a bed which covered the whole of the cabin area. On the Friday morning I visited High Voltage Engineering in the Cambridge suburbs. It had been formed in December 1946 with a licence to use *Robert Van de Graaff's* (1901–1967) electrostatic high voltage generator on a commercial basis. Before the Second World War, the Van de Graaff generators were large machines that required

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* See 'Pandora's Box'.

multi-storey buildings.* However, *John Trump* (1907–1985) at the MIT succeeded in reducing its size by enclosing the whole device in a pressure tank.

The Van de Graaff generator contained a charger that could provide a high voltage of 30 kV. This was used to charge the surface of an insulating belt that moved between two cylinders. The charging took place next to the lower cylinder, with the belt then transporting the charge to the upper cylinder where there was a discharge device that transferred the charge to the upper electrode of an acceleration tube. By ensuring that sufficient negative charge was constantly fed, this electrode received a high negative potential in relation to the earthed anode. The voltage over the acceleration tube went up to 2 MV in the device that was sold for medical purposes. The device was considerably smaller than General Electric's 2 MV resonance transformer, but one disadvantage was that the acceleration tube was open and required continuous vacuum pumping. The price was stated as being approximately 80 000 dollars.

Cambridge is on the Charles River opposite Boston and is one of the USA's main centres for education and research. The city, which was founded in 1630, was initially called New Towne but its name was changed to Cambridge in 1638 after the English university town. This is where Harvard University and MIT, one of the world's foremost technical universities, are located. John Trump was Professor of Electronic Engineering at MIT and the person I visited next of course. Trump met me in his laboratory with outstretched hand and the words 'I am John Trump.' To my European ears, this sounded like ostentatious self-assurance before I realised that this way of introducing yourself was in fact completely normal for an American.

In his laboratory, Trump had two Van de Graaff machines in use for x-ray irradiation and a third in a separate room not far away for electrons. In this physics laboratory at the technical university, Trump cooperated with doctors to use radiation to treat patients. For this he used mainly rotational irradiation, i.e., the patients had to sit on a rotating chair in front of the treatment device.

In order to check the position of the patient, Trump exposed an x-ray film with full field size to obtain an x-ray image of the relevant section of the body. The x rays that had been generated in the 2 MV device gave such good contrasts that the soft tissue of the body was also discernible. After he had taken this x-ray image, Trump reduced the beam to the narrow cone that would be used at the time of the treatment and exposed the film once more so that the image showed a darker area in the very place that the radiation would hit at the time of the treatment. While I was touring in the USA, people at many hospitals were talking with veneration and wonder about the good treatment results at MIT – the *technical* university!

I had the opportunity of discussing our own discharge tube with Trump and his colleagues. As before with Failla and the Machlett engineers, he thought it was impossible to get the tube to function without a voltage divider. He also advised against vacuum pumps with oil and prescribed mercury pumps. When it came to the material in the discharge tube, he thought highly-polished aluminium was the best, but there had to be microscopically glossy surfaces to prevent aluminium coming under the same umbrella as other less suitable materials. Steel could be used, but you had to be wary of copper.

Over the weekend I visited my uncle in the 'Swedish city' of Worcester. I was surprised at the provincial town atmosphere in American cities where a visitor from Sweden created a stir and made the news headlines in the local *Worcester Sunday Telegram*.

I then took the train to Washington D.C. I would have continued to Schenectady to visit General Electric's research laboratories, mainly the Knolls Atomic Power Laboratory† to study their betatron. However, permission was needed from the military authorities for this. Without my knowing, a full-on race was taking place between General Electric and Westinghouse for submarine reactors. I therefore had to wait a few days, something which General Electric facilitated through a telegram from its well-

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* In 'The Sword of Damocles', I have described the way in which the American physicist *Merle Tuve* (1901–1982) was obliged to call his laboratory outside Washington containing a 5 MV Van de Graaff generator an 'observatory' in order to obtain planning permission. Another early Van de Graaff plant (of 1.5 MV), the first for medical purposes, was commissioned as early as 1942 at the Haukeland Hospital in Bergen thanks to investments by Polar aviator and 'atomic engineer' *Odd Dahl* (1898–1993) and consultant *Sigvald Bakke*. This plant was in operation until 1970. Dahl then, along with *Gunnar Randers* (1914–1992), also became the driving force behind the Norwegian nuclear reactor programme.

† See Chapter 17 of 'The Sword of Damocles'.

known radiation physicist E. Dale Trout with an invitation to participate in the American Roentgen Ray Society's Congress in Washington.

Dale Trout was a very remarkable man. He was an employee at General Electric from 1928–1962. As well as his achievements as a designer of x-ray devices and the introduction of high voltage and telecobalt therapy, Trout carved himself out a unique position 'by virtue of his intelligence, indefatigability and integrity' to quote the Health Physics Society when he received their medal of honour in 1975. And, to continue the quote:

> During this period *[i.e., 1928–1962]* there was a dearth of physicists who would devote any time to radiology and its problems. Except for a very few institutions, not even the university medical colleges had qualified teachers. As a result, Dale Trout stumped the United States for at least three decades teaching us what to do and why – nor did it matter if we bought G.E. equipment. Thus, he served to develop this essential field far in advance of its recognition in the United States.

So, I arrived in Washington on Monday 24 September 1951 and on the train from New York I happened to fall into conversation with Failla's colleague, medical physicist Edith Quimby, one of the very few women in the field. Quimby started her career as an assistant to Failla at the Memorial Hospital in 1919. In 1923, she was the first to introduce a regular film dosimetry programme by cutting strips of x-ray film and enclosing them in black paper in small packets which were carried by the laboratory personnel. She was also a pioneer when it came to determining the doses of radiation for different configurations of radium needles. In 1932, she published a proposal for the most effective grouping of the radium needles, a proposal that formed the basis for Paterson and Parker's 'Manchester System'. She researched which doses of beta and gamma radiation were required to cause the skin to redden and thereby also became a pioneer where estimating the relative biological effectiveness (RBE) of the two types of radiation was concerned. When Failla moved to Columbia University in 1943, Edith Quimby went with him and started work on nuclear medicine at an early stage while simultaneously devoting even more time to radiophysics teaching and the clinical use of radioactive substances. I had heard of her as a co-author of *Otto Glasser's* (1895–1964) textbook *Physical Foundations of Radiology*, one of the few books on radiophysics which was available at the start of the 1950s.

Edith Quimby was a friendly lady who showed an interest in what was happening at Sievert's institution and in my own study trip. It was my first and only contact with her, but I remember it with appreciation.

The big Congress would take place at the Shoreham Hotel in Washington, the biggest hotel I had ever seen. I had not booked a room anywhere, which was a mistake since hotel rooms were not easy to come by in Washington. The Congress was also larger than any I had seen before, with around one thousand participants. Luckily, I met a young radiologist who had been given a double room all to himself right in Shoreham at a price he thought was too high. He was pleased when I offered to share the room with him.

Directly after arriving and before I had solved the room problem, I took myself off to the Shoreham Hotel to try and find Sievert's friend, Dr. Lauriston Taylor, the head of the Bureau of Standards' radiation laboratories, to convey Sievert's greetings and determine a time to visit the Bureau of Standards. I found Taylor in the big medical device exhibition hall. He was not difficult to find. He stood in the middle of the floor in an open area of the hall and was surrounded by a bunch of people in a way that made me think of a queen bee in her hive. There was no doubt that his personality made him the focus of attention in what was taking place. I ventured up to the group with a certain amount of dread to acknowledge Taylor and agree with him a time for the visit to the NBS.

I also managed to meet Dale Trout, who invited me to join him for breakfast in his hotel room the following morning, a new variant of the old French court's *lever*.

The thing that interested me the most in the big commercial exhibition was the 'cobalt guns'. Cobalt-60 with its two gamma ray quantum energies of 1.17 and 1.33 MeV would be pretty much an ideal source of radiation with an effective quantum energy that exceeded that which both the resonance transformers and the Van de Graaff machines were able to offer, despite voltages of several MV sounding impressive. A cobalt gun would also be reliable and more convenient to handle than the large machines. The only problem was that cobalt-60 was not yet available. I wrote about General Electric's cobalt device in my report home:

> GE is exhibiting its cobalt gun. The principle has been lifted from Benner's radium gun with a rotating plate. Non-GE-ers are maintaining that Eldorado's design is safer (mercury protection). The unit is handy anyway – as long as you can get cobalt. And in the latter case it is probably easier and cheaper to build the gun yourself.

I also visited Canadian Eldorado's stand at the exhibition. Here, they were offering their cobalt gun for 24 000 dollars for the device plus 24 000 dollars for the radiation source. The price breakdown was said to benefit the buyer since there was less duty on the cobalt than on the treatment device. It was also thought that cobalt-60 could be supplied with activities of more than 1 000 curies from Chalk River in one year.

Two months later in November 1951, the world's first two 'cobalt guns' were commissioned in Canada. The first, supplied by Eldorado, was installed at the cancer clinic at the Victoria Hospital in London, Ontario. The second, designed by the well-known medical physicist *Harold E. Johns* (1915–1998) and manufactured at the Acme Machine and Electric Co. in Saskatoon, was commissioned just a few days later at the University of Saskatchewan in Saskatoon. It remained in use until 1972.

I was not the only guest at the breakfast in Dale Trout's hotel room (at another hotel); Trout had also invited his young colleague *John Kelley*. We discussed the continuation of my journey and the visits to General Electric in Schenectady and Milwaukee. Trout also said what he thought about the Englishmen's linear accelerators, which he thought were at the experimental stage and needed a staff of competent engineers to function.

I also had the opportunity to accompany John Kelley to visit the Walter Reed Hospital in the Army and Navy Medical Centre. One of General Electric's 1 MV resonance transformers had been there for many years and they said they were very pleased with it.

On Thursday 27 September, I visited the National Bureau of Standards and was shown around by the knowledgeable Dr. *Scott Smith*. I also met Dr. *Margarete Ehrlich* and I took the opportunity to ask whether my notion that the 'Lippman film', i.e., photographic film with extremely thin emulsion, could possibly be less sensitive to energy variations than normal film during dosimetry because the blackening was caused only by secondary electrons from the surrounding medium. Margarete Ehrlich found the idea interesting but feared that the absorption of the electrons by the thin layer might perhaps show the same energy dependence as the absorption of the x rays in the usual film.

A man by the name of *Frank Day* was in the process of examining the energy dependence of different ionisation chambers. He asked about the possibility of being able to get hold of hundreds of 'Sievert chambers'* for his measurements. He suspected that they were less energy-dependent than the chamber supplied by Victoreen. If this were the case, he was interested in taking up production in the USA. It surprised me that the NBS did not have a good method for measuring soft x rays or for x rays with quantum energies of more than 0.4 MeV.

Over the weekend, I took the train to Schenectady. The city, which at that time had 90 000 inhabitants, was founded in the 1660s by a man by the name of Van Curler. A sign at the city border said that the city had been burned down by the French in 1690 and by Indians when the war between England and France spread across the ocean. I booked into the only hotel in the city which, as you might expect, was called Van Curler.

Since this was the one and only hotel, it must have been the very place where Gösta Forssell and Rolf Sievert had met in 1920 in a meeting described by Forssell with the following words:

> Our paths happened to cross in Schenectady, the stronghold of radiological engineering in America. There, in a hotel room high up among the stars, we remained

* I.e., Bg-chambers (see 'Pandora's Box').

> seated throughout the night, comparing our observations and our plans, and that is where the foundations were laid for a friendship and a companionship in the service of radiology that has lasted ever since.

Any such knowledge about this state of affairs had to my embarrassment escaped me and I was therefore unable to feel any appropriate sense of deference for the old hotel. Instead, I was brought into a conversation with a voluble older gentleman by the name of *Earnest L. Claiborne* who, according to his business card, was the hotel's Superintendent of Service. The 'N-word' is what was used in the common parlance of the time to describe Mr. Claiborne, and to my inexperienced eyes this rendered him an exotic being who was exciting to listen to. He gave me a short talk on how, in his view, the situation of the black people had changed radically with the Second World War. Black soldiers returned from Europe or the Pacific Ocean area with new experiences, full of pride and new-found self-esteem. The old discrimination has no future, predicted Mr. Claiborne. It does not exist in Sweden, said I with pride and naivety.

The following morning, I visited Knolls Atomic Power Laboratory, ignorant of its role in the Navy's development of nuclear submarines. There, I got to see General Electric's synchrotron which could accelerate electrons to energies between 20 and 70 MeV. The first synchrotron for medical use had just been delivered to Dr. Robert Stone in California.

A *synchrotron* is a particle accelerator in which electrons or protons describe circular paths in a circular vacuum tube by being deflected with the help of a magnetic field. The particle pathline has one or more cavities in which strong, high-frequency alternating electric fields give them an energy boost each time they pass the cavity. Because the strength of the magnetic field is simultaneously (synchronously) increased, the path radius can be kept constant. The length of the path is a complete number of wavelengths, which means that the particles reach the cavity in the same phase of the alternating electric field each time. Particles that have progressed further than intended owing to higher energy fall out of sequence and receive a smaller energy boost. This means that the particles automatically approach the intended energy. This self-stabilisation was simultaneously discovered in 1945 by the Russian physicist *Vladimir Veksler* (1907–1966) and the Californian physicist *Edwin McMillan* (1907–1991).

It was said that the advantage of the synchrotron was that the acceleration energy could be varied between 20 and 70 MeV and thus be adapted to the depth in the body at which the maximum dose from the generated x rays was required: from 1.5 cm at 20 MeV to 10 cm at 70 MeV. They also observed that at acceleration energies in excess of 30 MeV, the x-ray dose of the radiation on the exit side was greater than on the entry side. You could also make direct use of the electron beam with the synchrotron.

My companion, who was none other than the head of General Electric's laboratories in Schenectady, *Ernest Charlton* (1890-1980), thought that General Electric had not yet given its opinion on which type of device was the most suitable for medical radiation treatment: the operationally-reliable resonance transformers or particle accelerators such as the betatron or the synchrotron. Experiments are currently ongoing, he said, and experimental machines are being given to a few big clinics 'where eventually, following decades of research, a verdict may be reached.' That is when General Electric would start building the most suitable device for mass production.

At lunch, which was in a building that looked like a combination of a pavilion and a staff canteen, Charlton discussed the British linear accelerators. He thought their sole benefit lay in the high dose rate but that this benefit was visible only because no higher a dose rate than 100 röntgen per minute was needed for medical radiation treatment. He questioned their operational reliability but I took that with a pinch of salt; he was talking about competitors after all.

The group at the lunch table was indignant about the emergence of NATO and one man thought that the President ought to be brought before the courts for having allowed the USA to share defence secrets with other countries. The fear of Communism began to spread through Senator *Joseph McCarthy* (1908–1957) having held a number of talks over the past year in which he accused the government of being infiltrated with Communists. I was surprised at the levels of heat reached in the debate by well-educated engineers on this subject.

After lunch, Charlton took me to General Electric's factory further inside the city where I got to see a 15 MeV industrial betatron being assembled. With regard to the Allis-Chalmers betatron, Charlton said that in General Electric's factory he had produced the first one that was used in Chicago 'although they don't really want to recognise that now.'

Charlton's statement seemed incongruous bearing in mind that General Electric had chosen the acceleration energy 15 MeV. For the Allis-Chalmers 24 MeV betatron, the average photon energy (however that was defined) was 10 MeV, which is suitable for industrial radiography bearing in mind the absorption properties of iron. Malicious gossip in Washington had said that General Electric had disregarded the energy distribution and had therefore remained at the unsuitably low acceleration energy of 15 MeV for the purpose. This mattered less for the medical usage – of greater essence was the fact that General Electric's betatron had not yet been used anywhere for medical purposes.

From Schenectady I took the train to Chicago where I had agreed to meet John Laughlin at the University of Illinois' College of Medicine. Laughlin was the person who had written the most about the use of the betatron for medical radiation treatment and I had read a number of his papers. It was at the University of Illinois in 1940 that the inventor of the betatron, *Donald Kerst* (1911–1993), had designed the first device after an original idea by the Norwegian, *Rolf Wideröe* (1902–1996). Allis-Chalmers, which now manufactured betatrons on a commercial basis, cooperated with the university's scientists to develop its usefulness in medicine, the greatest use thus far having been for industrial radiography. There were just two other betatrons that had been in operation for a long time for radiation treatment, one of them with Laughlin in Chicago and the other at the University of Saskatchewan in Saskatoon in Canada, where its use had been led by radiophysicist H. E. Johns. However, in both of these cases, the device was a fixed assembly rather than a mobile one, the latter being something that was planned for the future.*

You might say that the *betatron* looks like a small cyclotron in that the circular pathline is accommodated within a vacuum tube between the poles of a powerful magnet. In the betatron, electrons are accelerated and the circular pathline is a result of the deflection caused by the magnetic field. However, unlike the cyclotron (and the synchrotron), there are no gaps or cavities where an electric field adds energy to the electrons. And nor is the electromagnet fed with direct current; it is fed with alternating current. When the strength of the magnetic field increases, an electromotive force arises and accelerates the electrons. While the magnetic field increases, the electrons are able to describe a very large number of revolutions in the acceleration tube. The kinetic energy that they then achieve, stated in electron volts, becomes numerically equal to the tension in volts which would be generated in a secondary spool with the same number of windings positioned between the magnetic poles. In this way, the betatron functions as a transformer but is more beneficial because it is not necessary, like it is in the transformer, to firstly create the high voltage over the secondary winding and then connect an x-ray tube. Instead, the electrons are accelerated directly in the tube. Rolf Wideröe himself had used the term 'radiation transformer' to describe the principle. Kerst has written the following about the name 'betatron':

> The name betatron, which is now *[1943]* in general usage among physicists, was chosen for the magnetic induction accelerator since it seems likely that the most useful applications of the betatron will involve the production of high-speed electrons or beta rays, as they are known in nuclear physics. When the Greek suffix, tron, is attached to the word beta, the name means the agency for producing high-energy electrons.

I met Laughlin on Thursday 4 October. He had just come back from the Congress in Washington. As for the others I visited on my travels, I had with me the special edition of *Acta radiologica* for March–April 1950, an edition which was dedicated solely to Sievert's radiophysics institution. It bore Sievert's endorsement: 'Doctor John S. Laughlin with the compliments of R. Sievert'. The gift completely

* Johns was also in there early with telecurie devices but is perhaps most known for his classic textbook, *The Physics of Radiology*.

dumbfounded Laughlin. '*He's* heard of *me*?' he asked in surprise. 'The man at the absolute pinnacle of radiophysics!'

Laughlin showed the Allis-Chalmers betatron which here, unlike the examples that were manufactured later, was assembled as a permanent fixture with a fixed beam direction. He had nothing other than praise to offer and thought that the betatron was the most operationally reliable of all the high energy devices. 'There are no parts that can go wrong and the circuit diagram is very simple,' he said. The betatron was controlled by a nurse and maintained by a man with a lower level of technical training.

When the betatron was started, you first adjusted the electron energy to the required value and then started the electron flow. After just a few seconds you had the full dose rate (I called it 'dosity' in my report home). The energy could be varied between 15 and 25 MeV, but the dose rate was low at the lower energies; at 15 MeV it was just half as much as it was at 25 MeV.

It was also possible to use the electron beam directly, but then you had to change tubes. Laughlin did not think it led to any advantages; the depth-dose curves were worse.

On the following day, I visited the University of Chicago where I was meant to have met medical physicist *Lester Skaggs* who was the most prominent advocate of electron therapy. However, Skaggs had gone away. I heard from his colleague that he had long been planning to procure a betatron for electron therapy and that he had money for that purpose as well as a special space for the device. Skaggs saw several advantages to using an electron beam. It could be adapted so that tissues behind the tumour were practically speaking completely spared. It was also known that the ionisation density increased to a maximum value at the end of the electron path. Here, the radiation could be expected to be particularly effective. However, one disadvantage lay in that the electrons in a beam would be spread so that this effect would be less pronounced, although this spreading was expected to be less at high electron energies, 20–40 MeV.

Skaggs had recently found that Allis-Chalmers could also deliver linear accelerators and at a lower price. He had therefore recently changed his mind and was now thinking of obtaining a 50 MeV electron energy linear accelerator, and Skaggs was now in California to see such a device.

On the same day, I received a handwritten letter from Dale Trout in which he apologised for not having been able to receive me in Milwaukee next week as planned because his father-in-law had died. Instead, I met John Kelley again on 8 October and was shown around the factory and laboratories at the General Electric X-ray Department. What was shown to me confirmed my impression that the 1 and 2 MeV resonance transformers were very reliable in terms of operation.

As well as General Electric's factories, Milwaukee also had Allis-Chalmers, which I visited the next day and where I met *Dane Scag*, the physicist who was responsible for the betatron design. He said that Allis-Chalmers first had intended to prioritise linear accelerators because these were cheaper and therefore might gain a larger market. However, the operational reliability had recommended the betatron. The favourite where radiation treatment was concerned was now the betatron for 24 MeV, but betatrons were also manufactured for higher energies and linear accelerators for 6 and 50 MeV.

For the betatrons, the experience of the devices that were used for industrial radiography indicated an average of four days' operational interruption per year. The first betatron designed for medical use was expected to be delivered to the Memorial Hospital in November (1951). One disadvantage to its use in Sweden was that the devices required 60 cycles per second alternating current. It would therefore be necessary to obtain a powerful motor generator for a potential cost of 10 000 dollars.

The visit to Milwaukee concluded my round trip of America and on 16 October I boarded *M/S Stockholm* for the journey home. It was not encouraging for Sievert to hear that the chance of the discharge tube ever being able to function at 1.2 MV as intended was very small according to unanimous American experts. At the maximum practically-achievable voltage of 400 kV, the x-ray yield was so low that all thoughts of radiation treatment were unrealistic. At his daily visits to the institution's workshop, Sievert complained in front of the instrument makers. 'I clean my pistol in the evenings', he explained. 'We must hope for the best!' was the somewhat ambiguous consolation.

If the message about the x-ray tube was negative, it made my information about the American high voltage devices all the more valuable. However, the image was not yet complete. There were European competitors, mainly the Siemens betatron and the British linear accelerators. It was probably Sievert who suggested that I supplement the trip to America with a trip around Europe. He had no difficulty convincing Elis Berven, who was in a position of power in that he was chair of both the Cancer Society in Stockholm and King Gustaf V's Jubilee Fund. Hugo Ahlbom, who succeeded Berven as head of *Radiumhemmet* in 1950, was in poor health and maybe had nothing against Berven taking initiative in the interests of *Radiumhemmet*. In any case, many forces were mobilised to organise my trip. Einar Wastenson, who was head of Swedish AB Philips' x-ray department, wrote to a number of firms in the Philips group to facilitate the trip, Berven wrote an introductory letter to Siemens, and Sievert contacted Professor Mayneord in London to request help with the planning. On 19 January 1952, I took the train from Stockholm to Hamburg. The whole journey would be undertaken by train and boat; the time of the passenger plane was not yet known.

Following a courtesy visit to *C.H.P Müller AG* in Hamburg, I continued to Göttingen to see a Siemens betatron for the first time. At the time, Siemens had not as yet delivered a betatron for medical radiation treatment. However, Göttingen had one of Siemens' first experimental 6 MeV electron flux betatrons with *Professor Paul* at one of the university's physics institutions. Paul, who was a nuclear physicist, used the betatron for physics experiments with electron beams. The access to an electron beam aroused interest among doctors in Göttingen, and some of them had carried out medical experiments together with Paul, including *Dr. Bode* at the skin clinic at the university hospital and Dr. Richard Kepp at the gynaecology clinic. They had changed over from doing what were originally purely biological experiments (studies of skin reactions) to the treatment of skin tumours.

As I had heard in the USA, owing to the spreading of the electrons, it was difficult to utilise the denser ionisation at the end of the electron pathways in a practical way. A narrow electron beam created doses of radiation that were rapidly reduced towards the depth in the body. The rule of thumb stated that the practical range of the electron beam in centimetres was approximately half of the electron energy stated in MeV. The depth dose distribution in the centre of the beam was no longer affected by the field size if the field diameter exceeded the practical range. You then obtained a dose maximum at a depth that was approximately 3/10 of the practical range.

Because the depth dose was affected by the field size and the spreading conditions, it was possible to appropriately spread the electrons before they hit the body, thus creating a desired dose distribution at the depth in the body. However, Kepp maintained that they were not looking to replace the normal radiation treatment with electron therapy; it was all about experiments that were facilitated by having access to the betatron.

Work was done under difficult conditions. Nuclear physics experiments were ongoing in parallel with the medical experiments,. The betatron was set up as a research instrument in a confusion of coupling hoses and instruments. Kepp also did not trust the physicists' dose determinations. 'It's like it used to be when working with x rays before the dose concept,' he said.

Göttingen would be the first place to receive Siemens' new 16 MeV betatron for clinical use. After that, the physicists would be given a new betatron for 25 MeV. The room in which the betatron would stand had been built without cooperating with Siemens in Erlangen. I found that no-one had taken into account space for physical measurements. The doctor I spoke to said: 'Physical measurements? All physical measurements are already done at the factory. When the betatron comes here it's just a matter of setting it up the and running it according to the depth dose tables just like a normal therapy device.' I was tempted to reply: 'Aha, just as it was in Sweden before 1920 before Sievert introduced the dose control?'

I was very warmly received in Göttingen and people did all they could to answer my questions. On one occasion, the generosity reached the point of embarrassment. I was invited to dinner by a friendly man who unsuspectingly showed me souvenirs from his visit to Norway as a German soldier at the start of the 1940s and told me how he had enjoyed the Norwegian natural surroundings and that he would like to return there with his wife. I also had the opportunity to state that the pubs still had young, arrogant students with duelling scars.

From Göttingen I continued to Erlangen, the hometown of *Siemens-Reiniger-Werke*. The growth of the Siemens group and different sections with changing names as time went on makes for a complicated picture. The group originated from the company which was formed in Berlin in 1847 under the name of *Telegraphen Bau-Anstalt Siemens & Halske* by *Werner von Siemens* (1816–1892) and *Johann Georg Halske* (1814–1890). The business in Erlangen started in 1877 when *Ernst Moritz Reiniger* opened an electromechanical workshop there. In 1886, Reiniger's workshop merged with Gebbert & Schall. Reiniger, Gebbert & Schall became a limited liability company in 1906 and in 1925, Siemens & Halske AG in Berlin became its principal shareholder. Following further growth, the company took on the name of *Siemens-Reiniger-Werke A.G.* in 1932. The company is now internationally known by the name of Siemens Medical Engineering.

Siemens-Reiniger-Werke had been interested in the production of betatrons since 1940. The first devices were subject to restrictions prescribed by the occupying powers and the 6 MeV Göttingen betatron was one of these. Siemens' most skilled designer of electromedical devices, *Dr. Konrad Gund*, was responsible for the design of and experiments with this. His ideas had led to a betatron of considerably lesser volume and weight than that which it had been possible to manufacture thus far.

One mistake by Siemens was to exhibit a prototype for 12 MeV at a Congress on Radiology in London in 1950 before the experiment had been concluded. When more than a year had passed without the factory having been able to produce a functional machine, the rumour started that the design was unsuccessful. One problem had been getting a sealed acceleration tube to function, which is why the company was forced to do experiments with open tubes connected to vacuum pumps.

These adversities appear to have hit Konrad Gund hard. When I visited him in Erlangen, he gave the impression of being under great pressure. He spoke using lofty words about the importance of the project to Siemens' reputation and about his own responsibility for glorious success.

At the time of my visit in January 1952, they were preparing to deliver the first medical betatron to Göttingen in April. The energy had been greater than the 12 MeV they had first counted on - it now reached 16 MeV. I naturally got to see prototypes in Erlangen. The most striking was the small size and mobility of the device. Siemens' experience of the clinical needs was clearly noticeable in the shape. Using a small handle wheel it would be easy to change the betatron from x-ray to electron irradiation.

The device would cost around 400 000 Swedish kronor and the price of the tube was estimated at 35 000 kronor. The betatron would firstly be manufactured as a series of ten, the first of which would go to Göttingen and the second to Heidelberg. I was told that an installation at *Radiumhemmet* would be so good for their advertising that they would probably be prepared to give precedence to a delivery there over other orders.

When I had returned home from my trip, it was not until 4 June that I got to read the following in *Stockholms-Tidningen*:

> 46 year-old Konrad Gund, who designed his 'betatron' in 1948, the first electron bowler in Europe and which has thus far cured numerous cancers at Göttingen's skin clinic, did not manage to immediately remedy a fault that had arisen on the device the other night. In despair, he turned on the gas tap and met his death next to his faulty invention. When his wife was informed of his suicide, she also took her own life.
>
> The director of Göttingen's skin clinic had asked Dr. Gund to come from Erlangen to monitor the 'betatron'. Before travelling, Gund had written three farewell letters and kept them in his pocket until he was sure that his invention was not living up to what it has promised. The whole of the scientific world had waited for it with bated breath. It was meant to use 15 million volts to destroy tumours 5 cm beneath the surface of the body.
>
> Dr Gund had been called to Göttingen several times to remedy inadequacies in the 'betatron' and this last time proved to be the final straw.

Konrad Gund, overstrung and weighed down by duty, had taken this tragic step unnecessarily. Not long after that, the Siemens betatron functioned perfectly.

One of the first Siemens 6 MeV betatrons was also in Erlangen at the university hospital where it had been used for experiments by the vibrant *Felix Wachsmann*. I visited Wachsmann, who appeared to be obsessed with a theory that high energy radiation must kill cancer cells on a selective basis more effectively than 200 kV x rays. I did not find Wachsmann's argument particularly convincing.

Over lunch with Siemens' directors and engineers, I was teased by an older industrialist who was criticising Sweden's action at the end of the war. 'When you were able to earn money by selling iron ore to Germany you had no objections,' he said, 'but when the wind changed direction you changed with it. And you're now ranting on about Germany's "crime" - cheap moralism!'

From Erlangen I continued by train to Switzerland where I would look at Brown Boveri's betatron. My first visit was to Professor Hans Rudolf Schinz at the Cantonal Hospital in Zürich. Schinz was an important man, an honorary member of the German Röntgen Society and leader of the Swiss delegation at several Congresses on Radiology. He was in charge of both the central x-ray diagnostics department and the radio therapy clinic at the Cantonal Hospital. He gave me not half an hour or quarter of an hour but exactly, pre-stated, fourteen minutes (!) for a talk. After having heard about my trip to America, this prominent radiologist, who must have had contacts all over the world, said in surprise: 'Ahh, hat mann auch in Amerika Betatronen?!', or 'Ahh, have they got the betatron in America as well?'

Brown Boveri's betatron was at Schinz' clinic. Brown Boveri & Co. (BBC) had been formed in 1891 and was one of the world' largest electrotechnical companies. Brown Boveri was working in much the same direction as the eight-years-older Swedish ASEA, with which it merged in 1988 to form ASEA Brown Boveri (ABB). The betatron was designed by Rolf Wideröe, a pretty remarkable man and pioneer with regard to various types of accelerator. Wideröe, who was born in Oslo in 1902 and went to school in Norway, studied electronic engineering at the technical university in Karlsruhe in Germany in 1920–1924. This is where he hit on the idea of the betatron in 1922, which he called the 'radiation transformer' because the acceleration tube corresponds to the secondary spool in a transformer.

At the time, Einstein's special theory of relativity was not generally known. However, when Wideröe was to calculate the speed of the electrons, which approached the speed of light, he was forced to accept the theory of relativity equations. The design of the 'radiation transformer', i.e., the betatron, appeared to be a very suitable work for a doctoral thesis. However, Professor of Physics *Wolfgang Gaede*, who was an expert in high vacuum problems, thought that the electrons which - let's face it - would be carried a very long way in total, would be carried for a distance, would collide with the residual gas which would be present even at the lowest achievable pressures.

The disappointed Wideröe then changed university and moved to Aachen to become a doctoral student under Professor *Walter Rogowski*, who specialised in cathode ray oscilloscopes. Here, Wideröe attempted to build a functioning betatron but was unsuccessful. Rogowski pointed out that he could not be awarded a doctorate for a device that did not function, Wideröe had to build something that worked.

In the search for new ideas, Wideröe then remembered a paper he had seen while in Karlsruhe. It was written by the Swede Gustav Ising and contained a proposal for an acceleration tube in which a travelling wave of high-frequency alternating voltages would accelerate electrons past a series of electrodes – a linear accelerator. However, Wideröe realised that the device proposed by Ising could not function in practice. Wideröe himself writes about this (Walo, 1993):

> The fundamental idea as such was very interesting, however. From this I then developed the so-called the 'driving tube', which was activated with high voltage and with which you could, using a suitable frequency and length, accelerate electrically-charged particles twice using the same voltage, that is to say once when the particles entered the tube and a second time when they left the tube because the voltage changed polarity across the tube at intervals and this made virtually no impression on the particles that were in the tube.

Wideröe succeeded in bringing this idea to fruition in the form of a linear accelerator where sodium ions or potassium ions (electrons would have required either tubes that were too long or alternating voltage frequencies that were too high) were first accelerated by a potential difference of 25 kV and then once again, at the next electrode, by 25 kV so that the total acceleration voltage was 50 kV. Wideröe had built the world's first linear accelerator. This led to a doctoral thesis which was published in 1928.

After defending his thesis, Wideröe found no immediate future in nuclear physics; he had no contacts with whom to take the accelerator technology forward in England or the USA - nuclear physics had not yet really been established at the universities and technical colleges. On Rogowski's recommendation, he found employment in the German heavy current industry, but when Nazism started making for an uncomfortable life he returned to Norway where he found work at a subsidiary of Brown Boveri.

Following the German occupation of Norway, Wideröe's younger brother Viggo participated in the resistance movement but was discovered in 1941 and brought before the German military tribunal. He was sent to prison in Germany and soon fell ill due to malnutrition and hard labour. Rolf tried to use his contacts in Germany to get his brother released but without result. However, in March 1943, he was sought out by some German air force officers who suggested that he accompany them to Berlin to help with a project, the nature of which they did not reveal. His assistance, they said, could lead to the release of his brother.

In Berlin, Wideröe found that the project concerned the construction of a 15 MeV betatron for the *Luftwaffe* in Hamburg. This work began at the same time as the heavy allied terror bombing of Hamburg. Wideröe found that C.H.F. Müller, the subsidiary of the Philips group, would be able to construct the betatron according to his instructions. The *Luftwaffe* maintained that the purpose of the betatron was to obtain better radiation treatment options, but Wideröe later found out that the real objective was to produce 'death rays', a new weapon that would be welcomed by the war propaganda. Luckily, this was a misjudgement based on lack of knowledge, and those who worked with the betatron were kept in ignorance of what the *Luftwaffe's* actual expectations were.

The Hamburg betatron started its work in summer 1944 but, following the allied occupation, it was moved to London as booty where it was used for industrial radiography. Wideröe returned to Oslo in 1945 where he was captured, accused of cooperating with the Germans and assisting with the production of the V2-rockets – an unfounded accusation. He was released after spending 47 days in prison; all Wideröe had worked on in Germany was the betatron which had never been intended for military use. To boot, chances are that his assistance had saved his brother's life.

Between 1945 and 1946, Wideröe was practically speaking jobless in Oslo. However, that did not mean that he was inactive. In January 1946 he submitted a patent application for a synchrotron without realising that McMillan had described the synchrotron in an article in *Physical Review* in 1945 and that Veksler had simultaneously and independently come up with the same idea. The dissemination of scientific information immediately after the war was not you would describe as efficient.

That same year, Wideröe received an offer from Brown Boveri to come to Switzerland and manage the production of betatrons, and this was something which tempted the company. It was the well-known Swiss physicist *Paul Scherrer* (1890–1969) who had encouraged brothers Theodor and Walter Boveri to start the project. Wideröe was given a completely free hand, something for which his explanation was that no-one else knew anything about betatrons. He chose to construct a 31 MeV betatron, bearing in mind the medical needs. Professor Schinz soon also came onto the scene and welcomed the betatron to his clinic at the Cantonal Hospital in Zürich. In April 1951 it was possible to irradiate the first patients. As early as 1952, the year of my trip to Europe, it was possible for Brown Boveri to deliver another two betatrons, one to the *Inselspital* in Bern and one to *Radiumhospitalet* in Oslo. The latter had been ordered by Medical Director *Reidar Eker* with the simple words 'We are ordering a betatron', without stating energies or other specifications. Wideröe did not successfully solve the problem of also getting the electrons out of the acceleration tube until 1956.

With the Brown Boveri betatron that I got to see *chez* Schinz, it was possible to work simultaneously with two x-ray beams in opposite directions. The device stood immovably in the centre of a large square basement with the two beams directed diagonally at two opposite corners of the room. The correct direction for the beam in a patient therefore had to be achieved by turning the patient into the right position. It occurred to me that major electrotechnical industries like Allis-Chalmers and Brown Boveri had succeeded in making reliably-operational but clumsy accelerators that could be used for radiation treatment but which were principally suited for industrial radiography, whereas it was concerns such as General Electric and Siemens that had experience of medical devices which could produce devices that were mobile and congenial to clinical use.

I also visited Brown Boveri's factory in Swiss Baden but do not remember whether it was there and then or later on when I got to meet Rolf Wideröe for the first time, who I found to be very friendly and unassuming but full of ideas and experience.

From Switzerland I continued to the Netherlands for a visit to Philips' laboratories in Eindhoven and a conversation with the head of the x-ray group, Dr. Wybe Oosterkamp. From there I continued by train via Paris to London. It was now time to study the British linear accelerators.

After the Second World War, there were a number of men in the United Kingdom who had special training in ultra-shortwave technology. They had cooperated in radar research during the war. Once the war was over, they expressed the desire to be able to continue together with their work in an area of research where their specialist knowledge could be useful. Their desire was heeded and one of their first tasks was to produce a linear accelerator for physical sciences research.

Wideröe's linear accelerator from 1928 had just two acceleration steps and he could not work with electrons because this would have required higher alternating current frequencies than those he had access to. In the British linear accelerators, it was possible to work with a series of driving tubes and accelerate electrons. If the driving tubes were connected to a radiofrequency voltage generator and the length of the tube was correctly adapted, the electrons hit an accelerating voltage at each gap between the driving tubes, while inside the driving tubes they were not affected when the electric field was in the opposite direction. *Luis Alvarez* (1911–1988) used the same principle in Berkeley in 1948.

The first British experimental example afforded a considerably higher electron intensity than had been expected and proved to be very useful for physical sciences research. Philips in England was then given the task of converting the device into a more solid execution. The renovated linear accelerator was then set up at Harwell. At the time of my visit, this device was the only one that was in operation in the UK. It was run with electron energies up to 3.8 MeV but usually 3.2 MeV.

In the meantime, attempts had been made to construct a medical radiation therapy resonance transformer for the Medical Research Council. The attempts had not been particularly successful and when news spread about the high intensity of the electrons from the linear accelerator in Harwell they began to look at whether or not it would be possible to build linear accelerators for clinical use. Physicist George Newbery at the MRC's radiation therapy research unit at Hammersmith Hospital in London was therefore sent to practice under *D.W. Fry*, the physicist who was responsible for the development of the linear accelerator at Harwell. At the same time, his colleague *Paul Howard-Flanders* (1919–1988) concentrated on examining device economics and the clinical requirements. His initial conclusion was that they ought to concentrate on linear accelerators that yielded electron energies of 4 MeV.

Flanders and Newbery drew sketches of what the device ought to look like and put their proposal to the Ministry of Health, which tasked two companies with the responsibility of producing a total of five devices. Two would be produced by Philips in Britain and three by Metropolitan-Vickers. The manufacturers were given a free hand as regards details but had to largely adhere to Flanders and Newbery's instructions. The dose rate of the generated x rays would have to amount to at least 150 röntgen per minute at a distance of one metre. However, when I visited England, none of these five devices had yet been manufactured. A few other orders had been prioritised instead.

Flanders and Newbery no longer thought that 4 MeV was a high enough electron energy. They wanted to get a 10 MeV linear accelerator for Hammersmith Hospital for their own use and had ordered one from Metropolitan-Vickers. The factory gave this device priority. By the time of my visit it had been tested for a year but not yet delivered. There had been no success in achieving 10 MeV – they had managed only 7.5 MeV. The hospital had therefore been forced to choose between having the device delivered as it was, for use at 7.5 MeV, or waiting for the device to be redesigned. I was told that the decision had been to have the device delivered immediately.

At Harwell, where they were very pleased with the old Philips-built accelerator, they still wanted a linear accelerator for nuclear physics research, this time with 15 MeV of energy. The order went to Philips again and when I visited, it was almost ready for a test run. So, Philips had prioritised the extra order over the two devices that were in the MRC's order. At the same time, an enquiry came from St. Bartholomew's Hospital, a forerunner in high voltage therapy, as to whether or not Philips could also manufacture a 15 MeV linear accelerator for clinical use. St. Bartholomew's had already asked Metropolitan-Vickers to build a 600 kV x-ray device for radiation treatment in 1934. The treatments with this device, the first high voltage device, began on 10 April 1937.

So, Metropolitan-Vickers already had experience of the requirements for medical treatment devices. The company had been formed when the big Vickers group took over British Westinghouse in 1919, which had recently left the Westinghouse group. The company was named the Metropolitan-Vickers Electrical Company. It initially concentrated on producing turbines, generators and motors but, with the impending Second World War, it would produce bombers, including the well-known four-engined

Lancaster plane. After the war, it also began to manufacture household articles such as ovens and refrigerators. From 1928, 'Metrovick' joined the Associated Electrical Industries (AEI) and from 1960, the business no longer used the name Metropolitan-Vickers.

When I visited Metropolitan-Vickers, I heard that the 10 MeV device for Hammersmith was a one-off occurrence and that the focus would then be on the 4 MeV devices which, unlike the Philips devices, could be carried using only one arm. Howard-Flanders and Newbery were very suspicious when I spoke to them and visited Metropolitan-Vickers in Manchester. They did not stand on ceremony and gave me to understand that for all they knew I could be an industrial spy for Philips, and it took a combination of patience and indignation on my part to dispel their suspicions.

This chilly reception, along with other experiences such as the death of King George VI on 6 February and walks in the February cold among the ruins in heavily-bombed areas of London, led me to heave a sigh of relief as I boarded *S/S Suecia* for my return journey to Sweden on 12 February. My trials and tribulations also included an attempt to obtain a cup of tea after my first dinner at the Strand Palace Hotel. The dialogue, which began when a baffled toady had fetched the head waiter, is an interesting bit of English to read:

> 'Is it correct, Sir, what I have been told, that you have asked for … tea … after your meal?'
>
> 'That is correct.'
>
> 'Sir, we pride ourselves that this is a good restaurant and that we do our utmost to satisfy our guests. So, since you asked for … tea … at dinner time, you shall be served … tea. But, Sir, when we go to this extreme to please you, perhaps you might be willing to meet us halfway and accept to have your … tea … served in a coffee cup, so that no one will see that we serve tea for dinner?!!'

At home on 20 February 1952 I wrote a short report and summarised my impression of the trips to the USA and Europe. The following conclusions are interesting to quote from my summary:

> Looking at the current development, it appears as though the betatrons in the range of 15–30 MeV would be the most suitable therapy devices. Indications against the use of higher energies (synchrotrons) are the inducted radioactivity and the cumbersome nature which characterises the requisite devices at least for the foreseeable future. In the range of 1–2 MeV, there is every reason to expect a far greater use of isotope guns rather than high voltage machines. However, owing to the world situation, the outlook for the near future, particularly regarding the possibility of continuously maintaining a great stock of isotopes, is fairly uncertain. However, the disadvantage of the size of the radiation source ought to disappear if enough uranium reactors with substantial flux density become available for peaceful purposes.
>
> The intermediate range of 2–15 MeV is somewhat debatable. Why not just go the whole hog you ask yourself. […] The only devices that have been designed from the start exclusively for medical purposes are the Siemens betatron and the British 10 MeV linear accelerator – and the 4 MeV linear accelerators to a certain extent.
>
> With regard to what I have seen and the information I have been able to obtain, it seems to me as though the most appropriate device for high-energy radiation treatment would consist of a betatron and possibly a cobalt gun – in which case we ought to keep our eyes open for the options offered by the latter. Of the betatrons, the Siemens one seems to appeal the most, although its operational data needs to be checked. The choice between which is the most appropriate out of Brown Boveri's and Allis-Chalmers' devices is fully dependent on the level of importance attached to mobility and dose rate.

My conclusions were realistic, but it was not until 1957 that they could become reality. Then, *Radiumhemmet* installed a Siemens 15 MeV betatron and a kilocurie device using cobalt-60 as a source of radiation (the prototype for Siemens' 'Gammatron I', developed jointly by *Radiumhemmet* and Siemens). Not until then were the reliability and performance of the devices trusted. Before that, the choice of device had been discussed in detail at a meeting in Blekinge in southern Sweden, for which

Elis Berven took the initiative, and on a study trip in Germany with the head of *Radiumhemmet* Sven Hultberg and physicists Lindell, Vikterlöf and Walstam, both of these activities taking place in 1955.

The use of kilocurie devices with cobalt-60 for distance treatment increased rapidly over the next ten years. In 1967, there were twenty such devices in use at the Swedish hospitals (*Radiumhemmet* went on to install another two kilocurie devices: Canadian Eldorado's 'Super G' and Siemens' 'Gammatron III'). In 1967, there was a 42 MeV Siemens betatron at the regional hospital in Örebro and 35 MeV betatrons from Brown Boveri at the regional hospital in Lund, The Jubilee Clinic at *Sahlgrenska Sjukhuset* in Gothenburg and The Jubilee Clinic at Umeå regional hospital. There was also a 5 MeV linear accelerator at *Sahlgrenska Sjukhuset* at the time.

A cobalt-60 decacurie device which was designed by Rune Walstam and me superseded the 3-gramme radium gun at *Radiumhemmet* in 1954 and another device, which Elema was selling at the time, was installed there in 1956. Elema's decacurie device was also installed at *Sahlgrenska Sjukhuset*, Umeå regional hospital, *Akademiska Sjukhuset* in Uppsala and the Regional Hospital in Örebro in the 1960s. However, the first to use cobalt-60 was Kurt Lidén, who had a radium gun converted into a cobalt gun in Lund as early as 1952.

After my return home, we succeeded in getting Sievert's large x-ray tube to function up to a discharge voltage of 370 kV, although this did not yield radiation doses greater than 3–4 röntgen (corresponding to around 30 milligray) in the imaginary cone tip beneath the tube. Radiation treatment therefore was impossible, although some biological experiments did take place on mice in cooperation with FOA scientist *Arne Nelson* (1910–1993) and on fruit flies in cooperation with geneticist *K.-G. Lüning* (1924–2004). This was a hard blow to Sievert and I personally saw it as a failure.

4. THE GENETIC ALPHABET

It was the English natural scientist *Robert Hooke* (1635–1703) who used a home-made microscope to discover that cork was made up of holes or blisters surrounded by solid walls. He called these holes ‘small spaces’, cellulae. At the end of the 1600s, other scientists found that all plants they looked at had a cellular structure. The Italian doctor *Marcello Malpighi* (1628–1694) in Boulogne started to make serious use of the microscope in his research and, using an enlargement of 180 times, succeeded in discovering the blood cells and seeing how veins and arteries were linked by capillaries. He was also able to study the growth of the foetus in the egg. In addition, he devoted himself to the anatomy of plants and published the book called *Anatomia plantarum* in two volumes in the 1670s.

The English doctor and botanist *Nehemia Grew* (1641–1712) dedicated himself to plant anatomy as a hobby and in 1682 published what is also a well-known work, *The Anatomy of Plants*. The cellular structure of plants was always well documented at the start of the 1700s.

However, it was not until 1831 that the English botanist *Robert Brown* (1773–1858) – the man who discovered the Brownian movement* – found that each plant cell had a well-defined nucleus. In the following year, the Belgian botanist *Barthélemy Dumortier* (1797–1885) discovered that plant cells could divide.

An even better microscope enabled studies which also indicated that animals and thereby also humans were made up of cells. This was definitely shown to be the case by the German biologist *Theodor Schwann* (1810–1882) in 1839. Perhaps the most obvious difference between the cells of animals and plants is that the animal cells have only a thin membrane as a partition while the cells of plants also have a solid cell wall with a supporting role. In 1858, the well-known German doctor *Rudolf Virchow* (1821–1902) formulated his theory that cells arose from cells through cell division (‘*omnis cellula e cellula*’) and thereby challenged the old abiogenesis theory which maintained that life and cells could arise spontaneously from nothing.

Not until 1875 did people first understand what happened when an egg was fertilised. Then, while studying sea urchin eggs, the German zoologist *Oscar Hertwig* (1849–1922) discovered that the male cell nucleus merged with the female. An improvement in microscopic technology made it possible to study the cell nuclei more closely. It was found that they contained thread-like structures that could be observed at the time of cell division. In 1879, German anatomist *Walter Flemming* (1843–1905) in Kiel was able to show that these structures were split up lengthways just before a cell divided and that the number thereof in each and every one of the new daughter cells was the same as it was in the mother cell.

The mysterious structures were named chromosomes in 1888 (from the Greek chroma = colour and soma = body) because you can see them better under a microscope if you apply colour to them. The option of being able to study the chromosomes under a microscope during cell division gave a good insight into the procreation mechanism of the cell. Strangely enough, until 1956 it was thought that man had 48 chromosomes, although the actual number was 46. It was Swedish geneticist *Albert Levan* (1905–

* An irregular movement that can be seen under a microscope and consisting of small particles suspended in a fluid, caused by knocks from the molecules in the fluid. The phenomenon was discovered by Brown in 1827.

1998) who, along with American *Joe Hin Tjio* (1919–2001), then showed that the previously-assumed number was incorrect.

Man's 46 chromosomes are divided into 23 pairs. During normal the cell division (mitosis) of a body cell, each chromosome is doubled and the chromosomes are divided into the two new daughter cells, whereupon each of which get a full set of chromosomes identical to that of the mother cell.

The division of gametes, meiosis or reduction division, is more complicated. When a male and a female gamete merge, the cells in the new individual receive a set of chromosomes where, of the chromosomes in each and every one of the 23 pairs, one comes from the father and one from the mother. This creates a random inheritance of the parents' characteristics as was shown for legumes by *Gregor Mendel* (1822–1884).*

During meiosis, which takes place in the testicles of the man and in the ovaries of the woman, the cell division takes place in two batches. The number of chromosomes is doubled before the first division, but a recombination of their structure also takes place with the chromosomes in each pair exchanging parts with one another, or crossing over. In the human being, an average of 2–3 such crossovers takes place per pair of chromosomes. This creates a biological variation. After this, two cell divisions take place in succession, resulting in cells with a single set of chromosomes, i.e., for the human being 23 chromosomes in each gamete. In the male, these daughter cells mature into sperm, and meiosis takes place constantly in the sexually mature man. In the female, the period of time for meiosis is more complicated and drawn out right from her foetal stage until she starts ovulating.

The American geneticist *Thomas Morgan* (1866–1945) used studies on fruit flies to successfully show that the Mendelian inheritance characteristics were controlled by the chromosomes. In the 1920s, he and his colleague were able to show that, under normal circumstances, pre-determined DNA segments were linked to specific locations on the chromosomes and always on the same chromosome. This genesis is usually called inheritance through *genes*. One specific gene thus corresponds to a specific section of a chromosome.

The fact that x rays can change heredity had already been shown in 1927 by the vibrant American geneticist *Hermann Joseph Muller* (1890–1967), a discovery for which he would go on to win the 1946 Nobel Prize for Medicine. The discovery had practical significance for plant breeding where in Sweden, *Åke Gustafsson* (1908–1988), Professor of Genetics at the Forest Research Institute of Sweden† in Frescati in Stockholm, became very active. Gustafsson was also a very colourful person who was seen at Sievert's institution now and again. He was also an author of fiction and poetry.

In the 1930s, the Swedish cell researcher *Torbjörn Caspersson* (1910–1999) showed that the chromosomes were made up of two different types of molecule: the albumin (proteins) and nucleic acids. It was initially and incorrectly thought that heredity was linked to the structural component of the albumin, twenty amino acids, which could constitute the alphabet that was needed to write the genetic code that controls life.

The nucleic acids – deoxyribonucleic acid (DNA) and ribonucleic acid (RNA) – were first thought to have too few elements to be able to form a genetic code. DNA and RNA are made up of building blocks called nucleotides. Because there are only four different nucleotides in DNA and RNA, it was considered very likely that genetic information was stored in the albumins which could in fact offer an alphabet with a full twenty letters. The sole function of the nucleic acids was thought to be that of keeping together the requisite structure.

However, in 1944, the Canadian-American virus scientist *Oswald Avery* (1877–1955) showed that pure nucleic acid, free from albumins, could transfer the hereditary code in a virus from one cell to another, so this focused the attention on the nucleic acids. In 1938, Caspersson had already put forward what was in those days an audacious hypothesis that the nucleic acids were what controlled the albumin production of the cells, and he now turned out to be correct. It had been wrong to believe that the four nucleotides would not be able to build up an alphabet. They could be combined. We can compare it with the Morse alphabet, which consists of more than fifty characters (letters, digits and punctuation marks)

* An account of Mendel's discovery is given in Pandora's Box.

† The Forest Research Institute of Sweden merged with the School of Forestry in 1962.

although it is made of just two building blocks: long and short signals. It went on to prove that the four nucleotides do create the requisite message in groups of three and thereby cannot form fewer than sixty-four different ‘letters’. It is more than enough to state which of the twenty amino acids the cells will produce to form proteins for different purposes.*

But it was still unclear exactly how the DNA molecule was made up. When this was revealed it was one of the most important research achievements ever. The main people behind the discovery were the unlikely pair of *James Watson* (1928–) and *Francis Crick* (1916–2004). Watson was a young American biologist who came to the Cavendish Laboratory in Cambridge in 1951 where the Briton Crick had been active since 1949. At the time, the Cavendish Laboratory was run by the well-known physicist and Nobel Prize winner Sir *Lawrence Bragg* (1890– 1971). Bragg was the world’s leading expert in x-ray crystallography and had developed diffraction methods that could also reveal the structure of very complicated molecules. The Cavendish Laboratory therefore had a special unit for x-ray diffraction studies. It was led by protein crystallographer *Max Perutz* (1914–2002), who mainly studied the structure of haemoglobin. Together with Perutz, Watson and Crick began a productive cooperation.

Crick was a bit of a one-off character. In his book about their research, *The Double Helix*, Watson writes that Crick often became worked up about his many whims, and when this was the case he spoke more vocally and quickly than anyone else and you could always tell where he was by his laugh, which was heard far and wide.

Like the Perutz group, Francis Crick also did research into protein structures. This did not mean that he was not interested in DNA – quite the opposite. Although it had been clarified during the 1940s that the genetic code of the cells must be in their DNA and not in the proteins, opinions were still divided among cell researchers. However, Crick was absolutely convinced that the genes, the structures that determine the hereditary characteristics, had to be found in the DNA molecule. The reason he did not throw himself into what appeared to be such a promising area of research was his sense of fair play. Crick was close friends with another scientist who worked with x-ray diffraction, New Zealander *Maurice Wilkins* (1916–2004) to be precise, who was at King’s College in London (not to be confused with King’s College in Cambridge). Wilkins had realised the importance of DNA very early on and also worked with endeavouring to reveal its structure.

The situation was further complicated by the fact that Wilkins, who was initially not particularly familiar with x-ray crystallography, cooperated with a skilful female crystallographer, *Rosalind Franklin* (1920–1958). Crick was not inclined to start his own research in competition with his colleagues in London. At the same time, he was frustrated because, in his view, the people in London did not realise how incredibly important it was to quickly find the structure of the DNA molecules. How could he get Maurice to realise that there was an urgent need for such an explosive piece of information?

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* Some of the mass in the chromosomes consists of proteins; DNA constitutes less than half of the mass. The text describes the important discovery of the structure of the DNA molecule. Simultaneously describing the very complicated make-up of the chromosomes as they exist during the cell division would have made the depiction unnecessarily difficult to read. It is enough to mention the most important elements in this footnote. The substance, chromatin, of which the chromosomes consist, is made of protein along with DNA. The DNA molecule can be released from the protein if the chromatin is treated with salt or weak acid. The protein consists of components called histones. Eight histones form a nucleus around which part of the DNA molecule is rolled. Groups of histones along with DNA and small quantities of other proteins are called nucleosomes, which you might think of as construction elements for the chromosome. While the DNA molecule has a diameter of 20 Å (i.e., 2 nanometres), the nucleosomes have a diameter of 100 Å (10 nanometres). The nucleosomes are packed together into a solenoid with a diameter of 300 Å (30 nanometres). The solenoid can then in turn be rolled around to form an even larger spool with a diameter of 2 000 Å (0.2 micrometres) and finally, this spool, at the cell division stage where the chromosomes are usually studied, can be rolled into a spool with a diameter of 6 000 Å (0.6 micrometres), which is the way the chromosomes are usually depicted.

Crystallography and x-ray diffraction studies

The spectral distribution of light can be studied after it has been deflected in a prism or after it has been diffracted in a structure of narrow columns, a lattice. In the latter case, light that has been deflected into different columns will be carried for different lengths. This means that the light will be intensified in certain directions where the light waves are in phase so that they strengthen one another. The connection between the angle of deflection U for the direction in which the light is strengthened, the column distance d and the wavelength of the light l is

$$d \cdot \sin\theta = n \cdot \lambda \ (n = 1,2,3 \ldots)$$

The difficulty with finding a lattice with a small enough column distance for examining x rays led German physicist *Max von Laue* (1879–1960) to think of the regular atom distribution in crystals and use crystals as a lattice. He was awarded the 1914 Nobel Prize for Physics for this.

The English physicists *William Bragg* (1862–1942) and *Lawrence Bragg*, father and son, shared the 1915 Nobel Prize for Physics for their x-ray crystallography work. Lawrence Bragg formulated Bragg's law in 1912 which resembles the expression for the deflection in simple lattices but now with d as a measurement of the distance (depth) between the plane of the atom in a crystal:

$$2d \cdot \sin\theta = n \cdot \lambda \ (n = 1,2,3 \ldots)$$

They now had a tool for two different types of investigation. Not only could they use crystals as a lattice for spectral analyses of unknown x rays, they could use known x rays to obtain knowledge of the distance between the plane of the atom in a crystal.

However, London and Cambridge were not the only places where there was interest in the DNA molecule. Perhaps the world's most prominent organic chemist at the time, *Linus Pauling* (1901–1994), was at the California Institute of Technology (Caltech) in Pasadena, and he had made it absolutely clear that he was also interested in the structure of DNA. Unlike Crick, Pauling had no reason to refrain from the research assignment with reference to Wilkins and Rosalind Franklin.

This was the situation when James Watson arrived in Cambridge. He came straight from Copenhagen on a research grant for the biochemistry of the DNA molecule. He was driven mainly by curiosity as to what the genes actually consisted of and how they functioned. In spring 1951, he participated in a conference in Naples on the structure of the large molecules found in living cells. This was where he met Maurice Wilkins and was impressed by the latter's lecture on research into the structure using x-ray crystallography, and particularly by a rough image that Wilkins showed of the DNA molecule. He tried making contact with Wilkins, but the latter did not seem easy to be to get hold of. Watson could not yet let go of the thoughts regarding what he saw as the key to the enigma of life. Better to let your imagination run wild and perhaps become famous, he thought, 'than that mature into a downtrodden academic who never took a risk with his thinking'.

His interest grew when on a subsequent visit to Geneva he listened to a lecture by Linus Pauling on a new model for the structure of proteins. The model showed a helix of polypeptides (chains of amino acids). This is what Watson has to say about it:

> Pauling's talk was made with his usual dramatic flair. The words came out as if he had been in show business all his life. A curtain kept his model hidden until near the end of his lecture, when he proudly unveiled his latest creation. Then, with his eyes twinkling, Linus explained the specific characteristics that made his model – the α-helix – uniquely beautiful. This show, like all of his dazzling performances, delighted the younger students in attendance. There was no one like Linus in all the world.

Watson was impressed once again. Would Pauling also reveal the structure of DNA? If so, it was a significant undertaking with major consequences. But if he did not succeed, a research assignment would remain that could lead to honour and renown. And there was nothing to indicate that Pauling's alpha helix for proteins could give any answer to the way in which the genes functioned in the DNA molecule.

Watson decided to teach himself to interpret x-ray crystallography images. In Copenhagen there were only biochemists. Watson hated chemistry. Pauling saw no need to travel. Pauling was far too big and egocentric a scientist to accommodate a biologist with a paltry knowledge of mathematics. Wilkins had appeared to be disinterested in cooperation so there was no temptation to go to London either. However, Watson knew that Perutz in Cambridge was working with the x-ray crystallography of large biological molecules such as haemoglobin. He decided to try and get himself accepted as a researcher at the Cavendish Laboratory. With the help of his Professor in the USA, he obtained recommendations and the necessary contacts to be able to start research at Cambridge.

Watson was willingly received by Perutz, who introduced him to Sir Lawrence Bragg who, as head, had to make the formal decision regarding permission to do research under Perutz. Watson took it for granted that the older man (Bragg was 61), whose big achievements had already taken place before the First World War, was in fact retired and spent most of his days at London clubs like the Athenaeum. This supposition proved to be incorrect, with Watson subsequently finding out that Sir Lawrence was anything but inactive.

However, those in charge of grants in the USA did not look favourably on Watson's move from Copenhagen to Cambridge. Watson was a biologist, they pointed out. He was not proficient in mathematics and needed to learn biochemistry in Copenhagen, he had no grounds for being able to do any meaningful research with Perutz. Watson saw no way out other than to find a biological research assignment that he did not intend to take seriously.

Already on the first day at the Cavendish Laboratory, Watson bumped into Francis Crick, a man who was not easy to avoid. To his pleasant surprise, he found that Crick, as opposed to the others, was obsessed by the secrets of the DNA molecule. They discussed Pauling's model for polypeptides. Could there be a similar model for DNA? It would be tempting to find it before Pauling if there were.

Watson and Crick started by compiling the things that were already known about DNA and the things that ought to be taken as a working hypothesis. The working hypothesis was that the DNA molecule, like Pauling's model for the polypeptides, was helical. However, one complication was that Wilkins had told Crick that the molecule had a larger diameter than could be expected for a single helix. The working hypothesis was then changed on the assumption that it was a matter of several helices wound around one another.

It was known that four nucleotides must be included as building blocks. Nucleotides are phosphoric acid esters of nucleosides. In turn, nucleosides are chemical compounds of a pentose (a simple type of sugar, either ribose or deoxyribose) and a nitrogen base. In DNA, it is a question of the four bases adenine, guanine, cytosine and thymine, which were designated A, G, C and T for the sake of simplicity.

All of the four nucleotides contain the same phosphate and sugar components. Watson and Crick assumed that these components were responsible for the strong chemical bonds. If it had not been for the constituent bases, the DNA molecule would have had a completely regular, monotonous structure. If the bases had always come in the same order, the microstructure would also have been regular, but in this case there would have been no explanation as to why different parts of the DNA molecule constituted genes that were responsible for completely different biological functions. The only explanation was that the bases came in a completely irregular order, an order that could store information.

Watson quickly learned the foundations of x-ray crystallography from Crick through their daily hour-long talks, knowledge that would otherwise have required the long-term study of specialist literature and textbooks that did not differentiate between essentials and details. Crick maintained that Pauling's success was due less to his undeniable mathematical knowledge than to his intuition and piecing together of building blocks for molecule models. Or, as Watson would put it: 'Worrying about complications before ruling out the possibility that the answer was simple would have been damned foolishness.'

Crick thought that access to Maurice Wilkins' x rays would save him at least six months, but this necessitated convincing Wilkins to cooperate. To Watson's surprise, Wilkins did agree to come to Cambridge one weekend. However, it turned out that Wilkins himself did not have access to the best x rays. Rosalind Franklin, or Rosy as the men slightly disrespectfully called her, suspiciously guarded all the images she herself had taken, images that were of better quality than Wilkins' own. Franklin had no intention of showing her material until a seminar in November. However, Wilkins did say that everything pointed to the DNA molecule being helical, but maybe three entwined helices.

Maurice Wilkins and Rosalind Franklin did not enjoy the best of relations. Maurice almost saw Rosalind as a subordinate who had taken too many liberties. While talking to Watson, he constantly complained that he could not control the stubborn, sharp-tongued Rosalind and Watson's book depicts her as an unruly assistant. What neither of them appeared to realise was that Rosalind Franklin was an established scientist who, despite her youth, had already gained an international reputation through valuable studies of the structure of carbon at high temperatures - scientist who considered herself to be on an equal footing with Wilkins.

Rosalind Franklin, without whose research results Watson and Crick would not have been able to solve the puzzle of the structure of DNA as quickly, deserves a few extra lines of mention. She was born in London in 1920, the daughter of affluent parents in the legal profession. She passed the Cambridge entrance exam in 1938 and began her studies there despite her father's belief that women ought not to dedicate themselves to academic studies. At first, he refused to pay for her education, and only once an aunt promised to pay the expenses did he relent. Rosalind graduated in 1941 and then worked as a doctoral student for the British Coal Utilisation Research Association (CURA). Before she was twenty-six years old she had published five important papers on the structure of carbon. She defended her thesis on physical chemistry in 1946, whereupon she spent 1947–1950 working in Paris at the *Laboratoire Centrale des Services Chimiques de l'Etat* where she studied x-ray crystallography and developed methods for x-ray diffraction studies of large organic molecules. In 1951, she was invited to join a group of scientists at the biophysics laboratory at King's College in London under Professor John Randall*. Randall gave her the task of researching the structure of the DNA molecule using x-ray diffraction.

Female scientists still had great difficulties being accepted in England as late as the 1950s, and Franklin's legal background did not make it any easier. At King's College, women could not even eat lunch in the same dining room as the men. The men tended to view Rosalind Franklin as an assistant rather than a scientist on equal terms. It is not surprising that she safeguarded her integrity and viewed Wilkins with mistrust and unwilling when he thought he had the right to view her results and considered her to be someone who was working for him. To make things worse, she was very skilful and succeeded in preparing her DNA samples in the form of finer fibres than anyone else and arranging them in parallel bunches to enable a study of diffraction.

In spring 1951, the cooperation between Watson and Crick was threatened when the eccentric and voluble Crick had an argument with Bragg, who questioned whether Crick was doing anything useful at the laboratory. Crick was now thirty-five years old and had not yet published anything of value. The fact that he was a doctoral student and ought to have a chance of defending his thesis was temporarily his saving grace.

However, it was not long before something occurred that would suddenly help Crick. A colleague, a crystallographer by the name of *V. Vand*, sent him a theory on the diffraction of x rays from helical molecules. Crick was interested, discovered an error in the calculations and took on the task of attempting to create a mathematical model himself. He succeeded in cooperation with Cavendish crystallographer *Bill Cochran*, who had also puzzled over the problem, and the two published the result in *Nature*. The triumphant Crick sent a copy to Pauling. His honour was saved and his position with Bragg secured.

Rosalind Franklin's November 1951 seminar was a disappointment for Watson who attended it. Franklin said that she had insufficient material as yet and that only further crystallographic analyses would eventually lead to the structure of DNA. She did not appear to have any time for Pauling's method of coaxing out the structure with a construction kit. She also rejected the proposed model that Crick and

* Sir *John Randall* (1905–1984) is best known as the inventor of the magnetron. The invention led to a more effective radar system which made a strong contribution to the allies' victory over Germany during the Second World War. The same invention also led to the subsequent development of the microwave oven when it was discovered that objects close to magnetrons were heated.

Watson showed her a little later. Her criticism reached Bragg, who forbade the continuation of DNA studies at Cavendish and warned Crick and Watson to leave the London researchers in peace.

With the DNA research blocked, Watson spent his time studying the tobacco mosaic virus (TMV). There was no DNA in the virus; instead, it contained ribonucleic acid (RNA), and Bragg had not said anything about RNA research. Watson began learning how to interpret the x rays using x-ray diffraction and got himself an x-ray camera to examine RNA.

In May 1952, the Royal Society arranged a meeting in London about the structure of proteins. Linus Pauling was invited to talk about his alpha helix. Crick and Watson hoped to be able to meet Pauling to try and find out how far he had got with his DNA research. And what would happen if Pauling visited Wilkins and Franklin and saw their diffraction images? But, to everyone's dismay, Pauling never came to the meeting. The American Ministry for Foreign Affairs had withdrawn his passport when he was about to fly from New York. The fear of Communism in the cold war and the pressure from the campaigning Senator Joseph McCarthy meant that Foreign Secretary *Dean Acheson* (1893– 1971) did not dare release an undisciplined Linus Pauling on London. The scientific world was astonished – a world-renowned scientist unable to participate in a meeting that had nothing to do with politics!

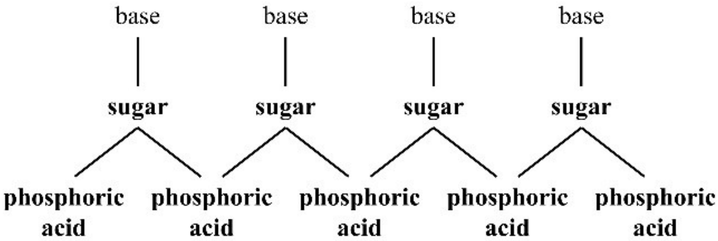
In June 1952, Watson's x-ray diffraction studies had convinced him that the structure of the RNA molecule must be helical. He was sure that this also had to apply to DNA. When Crick got to look at Watson's results, his usual enthusiasm came through and, despite Braggs' ban, the two scientists could no longer stop themselves puzzling over the DNA molecule. They read quantities of literature that might one way or another be able to give a clue. One of the many clues they came across was Chargaff's rule. *Erwin Chargaff* (1905–2002) was an Austrian biochemist who was Professor at Columbia University in New York. He had analysed the four constituent bases of DNA and found that there was always an equal number of cytosine and guanine molecules and an equal number of adenine and thymine molecules. This rule applied to all organisms that he and his colleague had studied on condition that the ratio between the quantities of cytosine and adenine molecules, and for guanine and thymine, varied between different organisms[*]. Watson attached no great importance to this observation, but Crick could not put dismiss the observation. Was it possible to draw any conclusion from it?

In July 1952, the International Congress on Biochemistry was held in Paris. To everyone's surprise, Pauling now resurfaced; Acheson had clearly been influenced by the many protests against the withdrawal of his passport. To the relief of the Cambridge scientists, Linus did not appear to have been thinking about the DNA problem; he spoke mainly about his alpha helix for protein.

In September 1952, a meeting was held in Stockholm arranged by Rolf Sievert and George de Hevesy for ICRP, ICRU and UNESCO's Radiobiology Committee. It discussed important questions about the impact of radiation on living cells, but the concept of DNA was mentioned only once (by *L. H. Gray* and only in passing). Otherwise, the problems were discussed at chromosomal or cellular level. The cream of the world's radiation biologists was present: Z. M. Bacq, L. H. Gray, Antoine Lacassagne, Raymond Latarjet, Hermann Muller and Cornelius Tobias and their Swedish colleagues *Lars Ehrenberg* and Arne Forssberg. The real importance of DNA was not yet known. It is interesting to note that the American Ministry of Foreign Affairs allowed the controversial Hermann Muller to participate in the Stockholm meeting just four months after having withdrawn Pauling's passport. Senator McCarthy had not yet achieved his notorious position of power as chair of the Senate's Research Committee.

In London, Rosalind Franklin continued to produce even better x-ray diffraction images of DNA but she refused to give them to Wilkins. However, she let it be understood that she now had evidence of the 'spine' of the DNA molecule; the actual deoxyribonucleic acid free from the bases did not lie in the centre of the molecule but formed its outer edge. This spine consists of an alternating chain of phosphoric acid and ribose (a type of sugar):

[*] In x-ray diffraction studies of organic molecules it is impossible to draw direct conclusions regarding the structure of the molecule from the diffraction images. Instead, the procedure is usually to try to verify an adopted model. Crick had developed a mathematical procedure to predict the diffraction image from helical molecules. If the actual diffraction image tallied with the one that was calculated, you had strong support to suggest that the model was correct.



This would mean that the bases (adenine, cytosine, guanine and thymine) must be in the centre of the molecule. But did DNA have more than one spine? And was it straight or helical? If the bases in the irregular sequence that could conceal a message rather than in a monotonous succession, it was a matter of finding not just the structure of the molecule but also the way in which the message could be brought out. Watson understood that the base sequence could be a template according to which other molecules could be characterised.

But even if the DNA molecule did carry the message showing which amino acids would be part of the protein that the cell produced, it could not act as a direct template for the protein production. It was now known that this took place outside the cell nucleus, beyond the reach of DNA. A messenger was always required for the message, a messenger that could be shaped with the DNA molecule as a template. One chilly evening at the start of November 1952, Watson sat in front of a coal fire in his cold room at Clare College and wrote with stiff fingers on a slip of paper which he then pinned up on the wall:

DNA → RNA → protein

The arrows did not mean that DNA would be transformed into RNA and RNA into protein - they simply indicated the chronological order. RNA would be formed with the help of DNA, and this RNA would in turn be able to leave the cell nucleus and in some way constitute a template to bring together the right amino acids for different proteins.

For the moment, Watson was alone with his thoughts; Crick was working on his doctoral thesis. He was now recognised as a capable colleague, even by Sir Lawrence, and was no longer perceived, in Watson's words, as 'a laughing talking-machine'. Bragg was also able to find consolation in that Crick, who was still difficult, had been offered a position for a year as a visiting researcher in the USA starting in autumn 1953 on condition that he had finished defending his thesis by then.

At the start of February 1953, the Cambridge scientists were shaken by the long-feared news that Linus Pauling really had created a model of the DNA molecule. A copy of his manuscript had been sent to Sir Lawrence, but the latter had not dared to show it to Perutz in case Francis Crick got to see it and it distracted him from his doctoral thesis. What Bragg did not know was that Pauling had also sent a copy to his son Peter, who was now a visiting researcher at Cavendish.

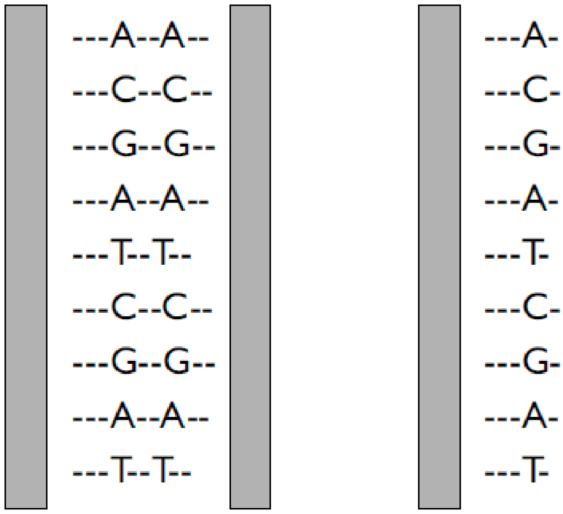
Watson was the first who got to see Peter's copy. To his great surprise, there was one very elementary error in Pauling's molecule model. 'If a student had made a similar mistake', writes Watson in his book, 'he would be thought unfit to benefit from Cal Tech's chemistry faculty'. However, his delight in that misfortune was dulled by the knowledge that Pauling, as soon as he had discovered the mistake himself, would not rest until he had found the right model. If Crick and Watson were to do so before him they would have to act quickly! They raised glasses to Pauling's mistake at The Eagle pub in the evening.

However, Crick was forced to give priority to his thesis. Watson, who now had enough information from Maurice Wilkins to convinced himself that the DNA molecule had a helical structure, decided to start following Pauling's example and experiment with a construction kit. Because the DNA research was still prohibited at Cavendish, he got in touch with Sir Lawrence and asked permission to do the experiment and to allow the Institute's workshop to produce the requisite model elements.

Bragg was convinced by Watson's argument and the threat that Linus Pauling would otherwise get there before him. The workshop started making models of the sugar and phosphoric acid molecules, which took several days. Watson started assembling the spine of the DNA molecule, the sugar and phosphoric acid chain, i.e., nucleic acid. He began to experiment with putting it in the centre of the molecule but eventually found this to be unreasonable. He then decided to build up a model with two helices in the outer edge of the molecule and on condition that the molecule had a diameter of 20 Å (2 nanometres). From what Watson saw from the x-ray diffraction images, the bases that must be packed in between the nucleic acid helices would be 3.4 Å thick. It was now a matter of trying to get models of the bases in there in such a way that the chemical bonds were reasonable.

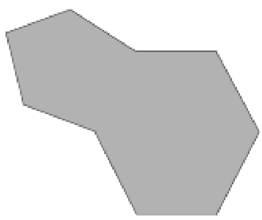
This happened in February 1953. The two helices in the model could now be checked against Franklin's x rays without Franklin knowing that the Cambridge scientists had them. The British Medical Research Council had set up a committee to review the research at Randall's institution at King's. Randall had always asked his scientists to write summaries of their progress. These summaries were compiled into a report which was duplicated and sent to the committee members. One of these was Max Perutz. Because the report was not confidential, Perutz showed Rosalind Franklin's x-ray diffraction images to Crick and Watson. Everything now definitely indicated that the assumption regarding the helix shape was correct.

In this connection, Watson was convinced that the DNA molecule consisted of two helical strands on the outside joined with the bases as bridges between the strands. It would require one pair of each base. On the assumption that like binds like, the reproduction mechanism would be explained. The one strand could have been a template for the formation of the other. The reproduction could take place by the two strands with their bases being separated and the two separated strands serving as templates for two new strands.

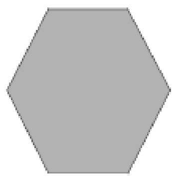


Because it would take a few days for the workshop to produce models of the bases as well, Watson sat at his desk in the evenings (he played tennis during the daytime) piecing bits of paper together.

However, there was one 'but'. The molecules of the bases are not the same size. Adenine (A) and guanine (G) have larger molecules than cytosine (C) and thymine (T). A bridge consisting of two adenine or two guanine molecules would thus take up more space than a bridge made of two cytosine or two thymine molecules. This would force the spine strands to bend here and there depending on the constituent parts of the bridge. You could draw a rough diagram of the surfaces of the molecules as multi-edged plates:



A and G



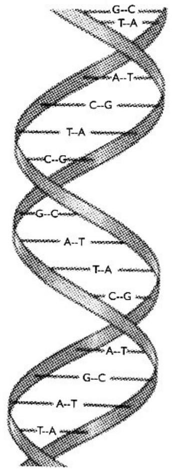
C and T

And then the solution suddenly appeared. Watson writes:

> When I got to our still empty office the following morning, I quickly cleared away the papers from my desktop so that I would have a large, flat surface on which to form

> pairs of bases held together by hydrogen bonds. Although I initially went back to my like-with-like prejudices, I saw all too well that they led nowhere. […]
>
> [I] began shifting the bases in and out of various other pairing possibilities. Suddenly I became aware that an adenine-thymine pair held together by two hydrogen bonds was identical in shape to a guanine-cytosine pair held together by at least two hydrogen bonds. All the hydrogen bonds seemed to form naturally; no fudging was required to make the two types of base pairs identical in shape.

The hydrogen bond meant that adenine was always paired with thymine and that guanine could only be paired with cytosine. Watson now realised the importance of Chargaff's rule. It was a natural consequence of assuming that two nucleic acid helices were linked with two base pairs like the rungs of a ladder in a helical rope ladder. There must then always be as many thymine molecules as adenine and as many of cytosine as of guanine. It was now easy to see that each helix with its bases could function as a template for a new helix with the corresponding but not, as Watson had initially thought, the same bases.



When Crick arrived slightly later, he immediately realised the consequences of Watson's discovery and was able to ascertain that the requisite bonds between the bases and the sugar of the nucleic acid fulfilled all known chemical requirements. It was now simply a matter of confirming that there was just enough room for the bases between the helices.

Wilkins and Rosalind Franklin were immediately informed of the new model and surprised both of Cambridge scientists by immediately accepting it. Watson's words now took on real meaning: 'It was too pretty not to be true.' All animosity between Franklin and the others vanished and she was now eager to show that her diffraction images gave the model strong support. Rosalind now found that she could discuss the problems as an equal and that her achievement had been important.

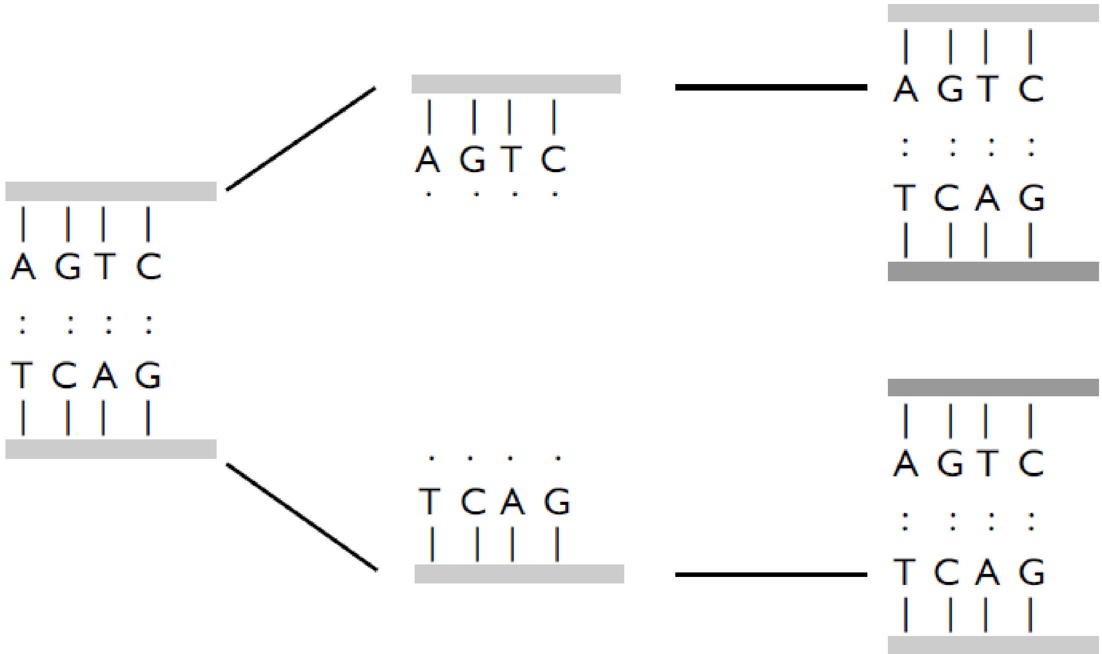
When Pauling heard the news he was impressed by the simplicity of the model and its considerable biological importance. Sir Lawrence Bragg was delighted that the discovery had taken place in Cambridge and not in Pasadena. On Wednesday 2 April 1953, Crick's and Watson's nine hundred word article was sent to *Nature*. It began with the words: 'We wish to suggest a structure for the salt of deoxyribonucleic acid (D.N.A.). This structure has novel features which are of considerable biological interest …'

After one year in the USA, Crick continued his research in Cambridge while Watson went to Caltech. After a while, Wilkins changed over to researching the nervous system. Rosalind Franklin, who found the environment at King's College too hierarchical and gender-discriminatory, moved to another laboratory and began research into the tobacco mosaic virus. Unfortunately, she was affected by cancer at an early stage and passed away in 1958 following repeated treatments at the age of only thirty-eight.

Crick, Watson and Watkins were awarded the 1962 Nobel Prize for Medicine for their discovery of the structure of the DNA molecule (actually that of the salt). By then, Rosalind Franklin had been dead for four years. Had she lived, the Nobel Committee would have faced a difficult problem – the prize could be shared between no more than three people. Would Franklin or Watkins have been the third prize winner in this case? It was generally thought that not enough attention was paid to Franklin's input.* Watson, who verges on insolence and is in any case condescending towards Rosalind Franklin in his book *The Double Helix*, does apologise in an 'epilogue'. He writes in 1968:

> Since my initial impressions of her, both scientific and personal, were often wrong, I want to say something here about her achievements. The x-ray work she did at King's is increasingly regarded as superb. […]Because I was then teaching in the States, I did not see her as often as did Francis, to whom she frequently came to for advice when she had done something very pretty, to be sure he agreed with her reasoning. By then all traces of our early bickering were forgotten, and we both came to appreciate greatly her personal honesty and generosity, realising years too late the struggles that the intelligent woman faces to be accepted by a scientific world which often regards women as mere diversions from serious thinking. Rosalind's exemplary courage and integrity were apparent to all when, knowing she was mortally ill, she did not complain but continued working on a high level until a few weeks before her death.

The discovery of the DNA structure made a substantial impact in genetics and microbiology. It was now evident that, by being split up into two separate helices, the molecule would serve as a template for the formation of two faithful copies (see the image) and would thereby be reproduced.



It would be way beyond the realms of this book (which is to provide an historical overview) to try to depict the very complicated mechanisms that enable each cell to fulfil its function on the basis of the information that is stored in the grouping of the several billion pairs of nucleotides in the DNA molecules. It would also forestall the events and discoveries beyond the period of time being looked at here.

No-one can have failed to notice that our mass media discuss more and more often the advantages and disadvantages that knowing which genes control our characteristics and diseases and defects can bring and possibly using that knowledge to find cures. It is, as one scientist has said, like knowing the way in which a radio device is constructed and always being able to repair it more effectively than simply giving it a haphazard shake.

* Evidence of this is that when I was looking for biographical data on Rosalind Franklin on the Internet, I found no fewer than 1 288 hits, a large number of which consisted of biographical paragraphs that were critical of the fact that she had been disregarded. This can also be seen as evidence that she was appreciated in spite of everything.

However, without going into details, it can be said that DNA molecules of the cell nuclei – with a total length of no less than a couple of metres (!) divided among the forty-six chromosomes – store the important instructions securely and only permit copies conveyed by RNA molecules with corresponding nucleotide sequences, assist with the processes whose objective is to build up proteins from amino acids whose sequence is determined by the message in the DNA molecule. This means that sequences of three nucleotides at a time, characterised by the constituent bases, give instructions regarding which amino acids in turn are relevant. Creating triplets of the four available bases (A, C, G and T) gives sixty-four different options, which is more than enough to choose from the twenty amino acids that may be relevant.

In chemical terms, genes are nucleotide sequences that determine hereditary characteristics by controlling the creation of important proteins. The proteins are made up of a large number of amino acids in a predetermined order. This order is determined by the base sequences in DNA, with each amino acid determined by a base triplet. One gene can consist of maybe a thousand nucleotides. Because there are more base triplets than are needed to determine the amino acids, some triplets can be used for other purposes. For example, the triplet ATG can be used as the START triplet to show where a gene begins and the triplet TGA (or any other) as a termination triplet (stop). The genes in higher organisms are thought to constitute just ten per cent of the total DNA, the majority usually referred to as 'junk DNA' with no known functions other than that of creating a reserve which may be useful if the body needs completely new proteins for future characteristics.

Where radiological protection is concerned, it is important to know that injuries such as cancer and hereditary changes may require a genetic change and that such changes (mutations) can arise if the base sequences in the DNA molecule are changed by bases being destroyed. In the sequence

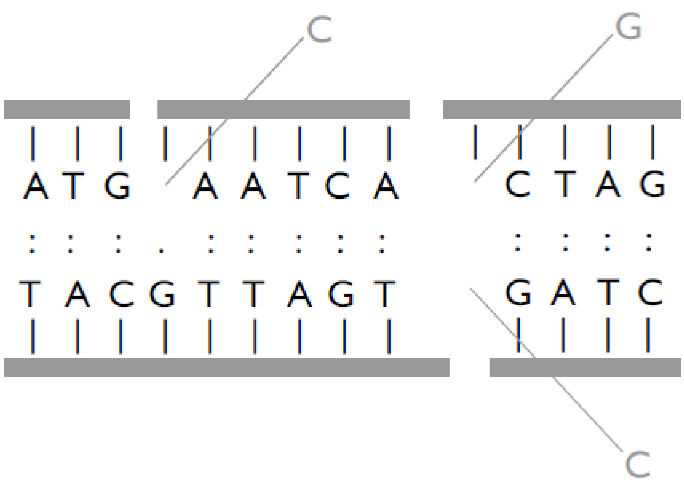
A T G C A G A A G …

a deficit in the sixth base (G) would change the sequence to

A T G C A A A G …

In the first sequence, the triplet CAG corresponds to the amino acid alanine, but in the changed sequence it is replaced by CAA, which corresponds to asparagine. And if the displacement continues throughout the gene, the message does of course become completely confused.

The ways in which ionising radiation can cause such injuries are either directly through the emission of energy in the DNA molecule or indirectly through a chemical influence. The latter takes place by means of radicals that have been released when the radiation has split water molecules. Radicals are groups of atoms which are part of molecules and which, if they occur separately, are extremely chemically reactive. Because two thirds of our body consists of water, reactions with the water in the body constitute the most common process in radiation chemistry. The water molecules (H_2O) are then split into the free radicals OH and H which in turn react with other molecules.



If an external influence uses radiant energy or free radicals to break off one helix (single-strand break), the break can be repaired. If at the same time one of the bases in a base pair (C in the above image) is knocked out, this damage can also be repaired because the remaining base (G) provides information on what is missing. If both of the helices are broken (double-strand break), these breaks can also be repaired. However, if both of the bases in a base pair (G–C) have been knocked out at the same time, the information has been lost.

However, the knowledge of these conditions was not widely known until going on towards the 1970s. It also took ten years for the discovery of the structure of DNA to be rewarded with the Nobel Prize. What happened after that ought to be mentioned here to complete the picture.

Human DNA contains a total of 2.9 billion base pairs. Some of these base pairs are part of the ‘junk DNA’ but the rest form genes. It was previously thought that there were close to 100 000 genes in a human being (it was sometimes said to be around 80 000, sometimes 140 000), but the estimate has now been reduced to around 35 000. Each gene thus consists of many thousands of base pairs.

In 1986, a project called HUGO or HGP (the Human Genome Project) was created to chart which sequences of base pairs that form part of human DNA. HUGO is an international, non-commercial multi-million dollar project in which twenty or so countries are participating. It was initially thought that the enormous task would be finished in 2003.

However, in the summer of 1998, HUGO was faced with competition from a newly-formed commercial company, Celera Genomics, which used a faster technique developed by a man by the name of *Craig Venter*, who had previously been employed at the American National Institutes of Health (NIH), which played an important role in HUGO. In March 2000 in an article in *Science*, Celera was able to show that the whole DNA of the fruit fly had been successfully charted, which showed that Venter’s method did function.

At the same time, a discussion was held regarding ethical research matters, mainly the risk of private companies having a monopoly on human genes. Around midsummer of the same year, a conspicuous press conference was held at which both the British Prime Minister *Tony Blair* and the American President *Bill Clinton* made their contributions. It was clear at the time that, apart from a few details, both Celera and HUGO would have finished with the charting of human DNA already in that same year. At the ceremony, Celera and HUGO said that their mutual feud had been set aside and that they intended to publish their results simultaneously, and that these would be kept available to everyone for research and thus not be withheld by private monopoly companies. However, a plan to publish the results jointly in *Science* failed to come off. Instead, HUGO’s results were published on 15 February 2001 in *Nature* and Celera’s results in *Science* on 16 February. The results are also available on the websites of these journals on the Internet.

The charting of human DNA is only the admission ticket to an enormous field of research. The next step is to use this information and seek to understand the way in which the genes function. It has sometimes been said that the knowledge of the DNA of individuals reflects a frightening genetic determinism. In actual fact, the ‘instructions’ in DNA do not define the ultimate individual. Only in exceptional cases such as the shape of the ear lobe or the colour of the eye is there a direct connection between the instruction and the result irrespective of the environment. In most cases, the instructions that are in our DNA instead create only our conditions to deal with the environment in which we will live. The way in which we are shaped as individuals is down to interplay between the environment and conditions.

5. BOMB TESTS IN THE PACIFIC OCEAN – RADIOACTIVE FALLOUT

In 1945 after the end of the war, the desire for international control of nuclear energy and nuclear weapons increased.* Following pressure from the British Prime Minister *Clement Attlee* (1883–1967), President Truman agreed to a British-American-Canadian meeting in Washington in November 1945. Truman's Foreign Secretary *James Byrnes* (1879–1972) had given research administrator *Vannevar Bush*† (1890–1974) the task of proposing measures for this meeting. Bush proposed that the United Kingdom and the USA send representatives to Moscow to discuss the exchange of information with the Soviet Union. This proposal was accepted in Moscow and the discussions led to the formation of an international atomic energy commission (UNAEC) under the UN.

Byrnes was looking for additional practical proposals and asked two committees to draw up some, one of them under Deputy Foreign Secretary *Dean Acheson* (1893–1971) and the other under the chair of the Tennessee Valley project, *David Lilienthal* (1899–1981). Their joint report ended up being called the Acheson-Lilienthal report. The proposal was put forward to the UNAEC by the American delegate financier *Bernard Baruch* (1870–1965) in the form of what is usually known as the 'Baruch Plan'. Its main aim was to create an international atomic energy organisation like the current IAEA. The majority in the UNAEC – although not the Soviet Union – accepted the Baruch Plan.

Meanwhile, in 1946 the USA had adopted an Atomic Energy Act which reflected the government's prioritisation of nuclear weapon production and which saw a civil nuclear power industry as something that was quite remote. According to this first Act, Manhattan District was being replaced by an Atomic Energy Commission (the AEC) with 5 Commissioners and with David Lilienthal as chair. At the same time, a Joint Committee on Atomic Energy (JCAE) was set up under the Congress to keep an eye on the AEC and arrange the 'public hearings' that could be required (the Committee was called 'joint' because it was common to the Senate and the House of Representatives).

In 1946–1947, the Soviet delegate for the UNAEC *Andrej Gromyko* (1909–1989) put forward a proposal stating that all nuclear weapons would be declared illegal and be destroyed. A future inspection would then be easier to accept. Gromyko's proposal was rejected by the UNAEC, which was unable to function after July 1948 and was formally ended in 1952. The cold war, an expression coined by Baruch in April 1947, prevented cooperation between East and West.

As early as summer 1943, when Robert ('Bob') Stone became head of the Health Physics Division at the Met. Lab. in Chicago and plans for the reactors at Hanford had started, the biological consequences for the Columbia River were discussed. The 44-year-old marine biologist *Lauren R. Donaldson* was employed to study the effects of radioactive substances on fish. The studies were not allowed to seem to be connected with the Columbia River but had to be carried out at the University of Washington in Seattle. The Applied Fisheries Laboratory was created for this purpose, with the name chosen to avoid attracting any attention. Donaldson was not informed of the actual purpose of the study until autumn 1944.

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* The events during the 1940s have been described in 'The Sword of Damocles' but are summarised here to provide a background for that which will be recounted later.

† Vannevar Bush played a significant role in the birth of the Manhattan Project and the manufacture of the first American atomic bombs. This has been recounted in 'The Sword of Damocles'. What is less well known is the fact that in 1945, Bush put forward the idea of using the new computer technology for hypertexts, i.e., texts where the reader can use repeated links to adapt the information or the report to his needs and desires or, to use the modern term, 'surf' the account.

The first thing to be studied was the hereditary effects over many generations on salmon that had been irradiated with x rays (radioactive nuclides were not yet available to study). The main reference was the *Biological Effects of Radiation* from 1936, edited by *B.M. Duggan*. Not until 1945 was a small aquatic biology laboratory set up in Hanford, which was run by du Pont but with Donaldson as a consultant. Neither the testing of the bomb at Alamogordo nor the bombs over Japan had caused noteworthy contamination of watercourses or the sea, but Operation Crossroads was planned on Bikini in 1946. The Seattle Laboratory's link to the Manhattan Project was then no longer secret.

In 1946, Bikini Atoll was inhabited by 162 people. It had been discovered in 1823 by Russian *Otto von Kotzehne* and consists of around thirty islands (of which Bikini is the largest) in an oval-shaped around 20 km * 40 km wreath around the atoll's lagoon. The Americans reached agreement with *Juda*, the head of the community (*iroiji*). Juda had consulted the Council, i.e., chiefs of the families. One American report says that 'the Bikinians, convinced that the Tests would be a contribution to world peace, indicated their willingness to evacuate [the atoll]'.

Crossroads was the first bomb test after the war and comprised the *Able* and *Baker* blasts on 1 and 25 July 1946 respectively. At the time of the test, the USA had access to only nine atomic bombs, all of them plutonium (such as the bomb, *Fat Man*, which was dropped on Nagasaki). Another year later there were still no more than 13 bombs. This was due to problems in Hanford with the heavy neutron flow in the reactors destroying material. The bomb that was used in the Baker test had a strength similar to that of the Hiroshima one and was detonated *under* the water. It raised a water column consisting of 10 million tonnes (!) of water and the 26 000-tonne battleship *Arkansas* was also lifted up in the water column. Photographs of the test impressed the whole world and made a great impact on both the world of fashion (the two-piece swimming costume) and the pop world ('An itsy-bitsy teeny-weeny yellow polka-dot bikini …'). Extensive marine biology studies were carried out on the area in 1947.

On 1 January 1947, the newly-established AEC took over the responsibility for nuclear weapons testing from the Manhattan Engineer District. The American Atomic Energy Act from 1946 led not just to the formation of the AEC but also to the formation by the President of an advisory committee for the AEC* and a military liaison committee made up of the Secretary of War and the Secretary of the Navy, and the previously-mentioned Joint Congressional Committee on Atomic Energy. The chair of the Advisory Committee was *Robert Oppenheimer* (1904–1967) who had become Director of the Institute for Advance Study at Princeton University after the war. The Committee members included *James Conant* (1893–1978), *Enrico Fermi* (1901–1954), *I.I. Rabi* (1898–1988) and *Glenn Seaborg* (1912–1999).

On 1 July 1947, the AEC said it intended to establish a test area for experimenting and testing nuclear weapons in the Pacific Ocean. The choice was now not Bikini but Enewetak Atoll, 300 km west of Bikini. Enewetak (Eniwetok) is on the outskirts of the Marshall Islands and was therefore considered to be safer for test detonations. It already had an aircraft landing strip.

Enewetak was discovered in 1794 by Briton Thomas Butler aboard the *Walpole* and consists of around forty islands in an almost circular wreath around a lagoon with a diameter of approximately 15 km. The series of tests that was prepared on Enewetak was called Sandstone and was implemented by means of three detonations (15 April and 1 and 15 May 1948), the largest of which had an explosive effect corresponding to around 50 thousand tonnes (kTon) trinitrotoluene (TNT).† A special radiation protection group called RadSafe monitored safety for the first time.

Not until the blasts on Enewetak had been completed in May 1948 did the AEC approve a proposal from the Applied Fisheries Laboratory to carry out a more comprehensive study of the radioactive contamination on Bikini and to extend the study to Enewetak. The investigations were carried out in 1948 and 1949. The investigations had practical consequences for Bikini. Enewetak had been seized for an indeterminate period, but Bikini Atoll was lent by the population for only a short time, with the

* General Advisory Committee (GAC).

† When talking about an atomic bomb 'of 50 ktons', you must not make the mistake of thinking that it weighs 50 000 tonnes. However, a trinitrotoluene bomb ('TNT' being the abbreviation for trinitrotoluene) would have needed to weigh 50 000 tonnes to create the same explosive effect.

population moving to Rongerik. Unfortunately, the situation at Rongerik was bad - those who moved there were starving and the American Navy wanted them to be moved. But Bikini Atoll was too radioactive.

Juda looked at different options. One option was Ujelang Atoll, 130 nautical miles south-west of Enewetak but the population of 120 from Enewetak had been moved there. In March 1948, Juda and his population were therefore temporarily moved to the larger Kwajalein Atoll instead, 400 nautical miles east-south-east of Enewetak, while a permanent place to live was sought. In autumn 1948, the Bikini population finally moved to Kili, a single coral island without a lagoon and with no possibility of fishing but with good farming options. But those who were moved there were not farmers and longed to return to Bikini - a bit of a social tragedy.

The AEC had a division for biology and medicine with Dr. *Shields Warren* (1898–1980) as its first Director. In a report to the Congress, Warren wrote the following in 1948:

> Over the many decades during which physicians have used X-rays and radium for the treatment of disease, they have become familiar with the harmful effects of overdoses of radiation. Biologists have assisted by studying how radiation affects plants and animals. […] By the time that atomic energy was developed, therefore, science was already familiar with the biological effects of most types of radiation.
>
> What was new to the biologist and the physician in the development of atomic energy was the massive quantity of radioactive materials created and the greater potentialities of these materials for both good and ill. The Atomic Energy Commission has the obligation to investigate these potentialities and to encourage and assist others to do so. It must explore the many benefits in prospect […] and it must learn how to forestall the dangers to human, plant, and animal life.

In 1949, the radiation studies on Bikini were extended, this time with better measurement instruments. Thus far, measurements had been taken only of the total beta radiation (i.e., the measurement of the beta radiation irrespective of which nuclides it came from) from material turned into ash, but it was now also possible to take measurements of alpha radiation and use a proportional counter and perform some chemical analyses. In June 1949, the AEC signed a contract with Holmes & Narver to extend the test station on Enewetak. The project meant that the atoll was completely set up for land-based operations. Both Crossroads (on Bikini) and Sandstone (on Enewetak) had been largely waterborne expeditions. Crossroads had certainly involved 42 000 people, but mainly on ships. Equipment was now planned on a much larger scale on the atoll itself. Hundreds of measurement stations and homes were erected on Parry Island, one of the islands of the atoll.

After the end of the war, the scientists in Los Alamos felt uncomfortable about the impending bomb research. Research had certainly been done on the hydrogen bomb in parallel with the work on the atomic bomb, but it was principally *Edward Teller* (1908–2003) who had devoted himself to this. After the war, Oppenheimer, who had previously supported the project, no longer wanted anything to do with the research. Just one small group under *Robert Richtmyer* continued with the assignment in Los Alamos. Teller had temporarily left Los Alamos for Chicago but often visited Richtmyer's group anyway.

On 23 September 1949 President Truman declared that the USA was no longer alone regarding the atomic bomb and that the Russians had just detonated a bomb (called 'Joe-1' by the Americans). Teller had then returned to Los Alamos and was convinced that the Russians would also develop a hydrogen bomb. However, his concern was not shared by the GAC, the Advisory Committee of which Oppenheimer was chair.

A couple of days after a GAC meeting on 29 October 1949, Teller went to Washington to meet Senator *Brien McMahon* (1903-1952), who was chair of the Congress' Joint Committee on Atomic Energy. Teller travelled by way of Chicago where he met Fermi, who participated in the GAC's meeting, but Fermi did not want to disclose what had been decided. While with Fermi, Teller had a telephone call from John Manley, Deputy Director of Los Alamos and Secretary of the GAC. Manley asked Teller not to meet McMahon and not to try to influence the politicians in Washington. He told Teller that it would be unfortunate if McMahon were to gain the impression that the scientists were divided in their views on the issue.

However, Teller refused to comply with Manley's request. He visited McMahon who had just read the GAC's report and said that it made him feel sick. Teller pleaded with McMahon to do what he could to make the hydrogen bomb a reality, which McMahon promised to do.

It turned out that the GAC unanimously declared itself to be against a hydrogen bomb programme. 'We all hope that by one means or another, the development of these weapons can be avoided. We are all reluctant to see the United States take the initiative in precipitating this development.' The Committee's majority wrote 'In determining not to proceed to develop the super bomb, we see a unique opportunity of providing by example some limitations on the totality of war and thus of limiting the fear and arousing the hopes of mankind', and the hydrogen bomb was designated a 'weapon of genocide'.

The Committee's minority, consisting of Fermi and Rabi, wrote: 'The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an evil thing considered in any light. For these reasons we believe it important for the President of the United States to tell the American public, and the world, that we think it wrong on fundamental ethical principles to initiate a program of development of such a weapon'. But Fermi and Rabi thought the decision not to develop the hydrogen bomb ought simply to be made on condition that the Soviet Union took the same step. They wrote: 'It would be appropriate to invite the nations of the world to join us in a solemn pledge not to proceed in the development or construction of weapons of this category'.

Teller thought that these statements and unanimity within the GAC would definitely put a stop to any thoughts of the hydrogen bomb. To his surprise, he found that the effect in Los Alamos was the opposite. The scientists had been irritated by the GAC's statement and were thus in favour of the hydrogen bomb rather than against it. Senator McMahon and other members of the Joint Congressional Committee for Atomic Energy asked the President to make a decision regarding the hydrogen bomb.

A decision came when the German-British physicist *Klaus Fuchs* (1911– 1988), who had worked in Los Alamos, on 27 January 1950 admitted that he had given information on the American hydrogen bomb research to the Russians. On 31 January, President Truman rode roughshod over the GAC's recommendation and asked the AEC to continue with the hydrogen bomb programme. In actual fact, the information that Fuchs had provided on the American hydrogen bomb work was not much use to the Russians because it concerned Teller's first construction concept which subsequently proved to be unusable (Fuchs had on the other hand shown the Russians how to construct an ordinary atomic bomb).

The AEC then said that a new section had been set up for hydrogen bomb research in Los Alamos and that resumption of the test detonations in the Pacific Ocean was anticipated. However, the Korean War, which broke out on 25 June 1950 and lasted until 1953, initially constituted an obstacle to new bomb tests because the Navy's resources were needed for the war. The Korean War shook the American population. A world war had recently ended with the use of atomic bombs and now a new war was beginning in which both the USA and the Soviet Union had nuclear weapons.

The Korean War meant that the work with the hydrogen bomb was accelerated. However, the constructions on Enewetak were not yet finished. The AEC therefore set up another test station which was located in Nevada, 100 km north-west of Las Vegas in the 13 000 km^2 Las Vegas Bombing and Gunning Range. The new station was called the Nevada Proving Ground. The first test detonations in Nevada started on 27 January 1951 with Operation Ranger under the leadership of Los Alamos.

Teller, who was about to take up a professorship at UCLA, decided to stay in Los Alamos to concentrate on the hydrogen bomb. Comprehensive mathematical calculations were needed to check the possibility of the bomb structure as imagined by Teller ('Teller's Super bomb'). The programming of the world's first electronic 'mathematics machine' or ENIAC (the Electronic Numerical Integrator and Calculator) had begun. It had been designed for the American army by the University of Pennsylvania and was put in place at the Aberdeen Proving Ground in Maryland in 1946. At the same time, mathematician *Stanislaw Ulam* (1909–1986) began equivalent manual calculations with *Cornelius Everett*. The calculation concerned what happened in the bomb during the very first moment, less than one millionth of a second. Ulam has described the way in which the calculations were done manually using slide rules for 4–6 hours per day. The first results were ready before anyone had even had time to programme the ENIAC. In February 1950, Ulam had already realised that the quantity of tritium that

Teller had thought would be needed for the bomb was not enough.* The calculation was re-done for more tritium, but Ulam found that the bomb would still not function. In April, Ulam travelled to Princeton to discuss the results with Oppenheimer, Fermi and the prominent mathematician *John von Neumann* (1903–1957). It is said that Oppenheimer looked almost relieved when it appeared in principle as though the hydrogen bomb was impossible.

Back in Los Alamos, Ulam told Teller about the disappointing result and according to Ulam, the latter turned 'pale with rage' and refused to believe the information. The calculation at ENIAC started in June 1950 and the result confirmed what Ulam and Everett had found. Teller was very disturbed, despite the fact that if a hydrogen bomb was not possible, this ought to have meant that the Russians could not produce one either. Teller was now starting to be criticised for having led the country into a costly development programme without initially having made sure that it was viable.

Nonetheless, Teller worked stubbornly on his old idea for the rest of 1950 and said that he would have achieved results if he had had more competent colleagues, which did not make him many friends. However, the solution came in February 1951 in the form of a completely new idea. It is not clear whether the idea was Teller's or Ulam's or someone else's; Teller has given completely different versions and Ulam has always maintained that the original idea was his. Contributions were probably made by both.

With the new idea, the fission charge is placed at a distance from the tritium and the fusion is initiated by x rays from the fission explosion before the shock wave gets there. The x rays heats up all fusion material at once similar to the way in which microwaves heat up the food in a microwave oven. However, this is not sufficient; the material also has to be compressed. According to Rhodes' book (Rhodes, 1995), this takes place by the fusion material (in the form of a cylinder) being surrounded by plastic foam. The x rays rapidly vaporise the plastic which expands and explodes, creating an incredible pressure. This pressure makes a plutonium rod that is placed inside the fusion material cylinder (tritium) supercritical so that the plutonium explodes. The whole device can be surrounded by a uranium tamper. All of the conditions for a continuous fusion reaction are thereby satisfied and the fusion material also detonates.

This was a completely new principle, the 'Teller–Ulam bomb'. The next series of tests at Enewetak was now being prepared. It was not about the detonation of bombs but uranium charges with a small amount of tritium to see whether fusion was actually possible and whether the neutrons from the fusion process could fortify the efficiency of the fission process. Bombs that utilise this principle are called boosted fission weapons as opposed to hydrogen bombs, where the main energy comes from the fusion process. The series of tests at Enewetak was called Operation Greenhouse and included 4 detonations between 7 April and 24 May 1951. The test detonation of 24 May concerned a fission process boosted with tritium.

Teller observed the first explosion with Ernest Lawrence, the designer of the first cyclotron, and they then swam in the lagoon. Teller thought that the experiment had failed, but Lawrence bet 5 dollars that it had succeeded. Teller lost his money. A thermonuclear reaction had been created for the first time anywhere in the world, although not a hydrogen bomb. Despite the pressure of the Korean War, 9 000 men participated in Operation Greenhouse.

Around one month after Greenhouse, the AEC convened a round-table conference to be held at the Institute for Advanced Study at Princeton on 19 and 20 June 1951. Members of both the GAC (including Oppenheimer) and the AEC participated along with scientists from Los Alamos. The subject was how best to construct a hydrogen bomb. One person after another spoke but no-one mentioned the Teller–Ulam idea. The impatient Teller then went to the blackboard and gave an account of the idea and the calculations that had been done and the experimental support provided by Greenhouse. According to Teller everyone, including Oppenheimer, was enthusiastic.

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* The principle for the hydrogen bomb was that tritiated hydrogen, tritium (H-3), from an explosion of a fission charge (i.e., a 'normal' atomic bomb) would be exposed to such high temperatures and pressures that its atomic nuclei would 'melt together' (undergo fusion) while releasing energy because mass would become lost (see 'The Sword of Damocles').

Plans were now underway to produce a bomb following the Teller–Ulam recipe to test at the end of 1952. However, after Greenhouse, a few 'ordinary' series of test detonations were firstly carried out in Nevada. The first of these (i.e., the second in Nevada) was carried out on 22 October – 29 November 1951. It was called Operation Buster-Jungle and included four small detonations. At the same time, it was said (in October 1951) that the Russians had detonated another two bombs, i.e., now a total of three.

In spring 1952, Teller succeeded in pushing through a new research laboratory for the hydrogen bomb. In the following year, he gained support from the AEC's new chair, *Lewis Strauss* (1896–1974) and from chemist *Willard Libby* (1908–1980) and Ernest Lawrence, and he also succeeded in convincing Secretary of Defence *Robert Lovett* of the necessity. Teller wanted the Laboratory to be in Chicago, but the Chicago scientists were tired of weapons research. Instead, it ended up in Livermore in California, administered by Ernest Lawrence at the University of California's radiation laboratory that had been so important in the Manhattan Project. The person in charge of the Livermore laboratory was *Herbert York*.

On 1 April – 5 June 1952, a third series of tests was carried out in Nevada, called Operation Tumbler-Snapper, with eight detonations. At the start of 1952, *John C. Bugher* succeeded Shields Warren as Director of the AEC's Division of Biology and Medicine. His closest aide and head of the biology section was *Paul B. Pearson*. The latter recommended a proposal from Donaldson regarding activity measurements following the next series of tests at Enewetak. Bugher found the proposal to be incomplete but Pearson's response was that this was because Donaldson had not been given any information about the test: 'I believe that if we give Donaldson information on the nature of the forthcoming tests, he will present more precise plans.'

The American authorities still wanted the population from Bikini returned from Kili but needed to ensure that it would not be dangerous. Bugher wrote to the Military Council stating that measurements had already been planned there for other reasons. However, in September 1952, without making the decision public, the AEC decided once again to give the Bikini atoll a role in the continued test activities, and building work for this purpose was started by Holmes & Narver in October.

In October 1952, the British detonated their first atomic bomb of around 20 kton at the Monte Bello islands off the northern section of Australia's west coast. The following year, the tests were continued at Emu and from 1956 at Maralinga, both areas in southern Australia on the borders of the Great Victoria Desert. In 1957–1958, the British nuclear test explosions also took place on Christmas Island (that being its former name – it is now called Kiritimati) in the island state that is now called Kiribati and which is the northernmost of the independent island states in the Pacific Ocean.* A few British test explosions were also carried out in 1957 over the sea next to the small island of Malden approximately seven hundred kilometres south of the then Christmas Island. The explosions at Maralinga in 1956–1957 attracted a great deal of attention although none of the tests had a blast strength of more than 25 kton of TNT. The alleged reason was lax security measures and lack of understanding of the local population's (the Aborigines') problems. Extensive reviews of the radiation risks, mainly from plutonium in the surroundings, were carried out during the 1980s and 1990s and measures were taken at the end of 1990s to reduce the risks for the local population.

The next imminent series of American tests in 1952 was called Operation Ivy and would include two detonations at Enewetak called Mike and King. Although Teller was now in Livermore, Mike, the first hydrogen bomb, was put together in Los Alamos. It weighed 65 tonnes and was too large to constitute a reasonable weapon, but it was still the first hydrogen bomb.

Teller did not go to Enewetak but stayed in Livermore. When Mike exploded on 1 November 1952, Teller was standing in front of a seismograph. Herbert York listened to a short-wave radio tuned to the frequency for the telemetry at Enewetak. After the bomb was initiated, the seismic wave would take approximately 15 minutes to reach California. The seismograph gave the predicted result. In Los Alamos they were still waiting for a report from Enewetak and it took time for the safety people to interpret the

* There is another island that is also called Christmas Island. It belongs to Australia and is in the Indian Ocean around 360 km south-west of Java. This may be confusing, but Kiritimati was the place where the nuclear weapon tests were carried out.

course of events and code a report. Teller therefore had the information before Los Alamos and asked York to send a telegram there from Livermore with the short message: 'It's a boy!'

The ball of fire had been ascertained as having a diameter of approximately 5 kilometres. The atoll island of Elugelab on which the bomb detonated had literally gone up in smoke. The world's first hydrogen bomb had exploded and was a success for its designers. The blast strength corresponded to a good 12 Mton (million tonnes) of TNT, i.e., around 500 Hiroshima bombs! It is not clear whether Teller had a bad conscience. He has written that he thinks everyone who worked with the hydrogen bomb was appalled at the conceivable consequences but that the scientist has a duty to research what humans can achieve and every citizen has a duty to consider the consequences. And Teller continued to do research; it was a matter of making the bomb so small that it could be transported by plane or missiles. This was difficult with (liquid) tritium. One alternative was lithium. Neutrons from the fission component of a lithium bomb will produce tritium in a very short time if they hit lithium-6, an isotope that constitutes 7.4 per cent of natural lithium (the dominant isotope is lithium-7). Because there is a considerable relative difference between the masses of the two isotopes, lithium-6 can quite easily be separated from lithium-7. If lithium in the bomb is in the form of lithium-6-deuteride ($^{6}Li^{2}H$), the fusion will take place between the tritium and deuterium formed at the instant the explosion occurs.

In the Soviet Union in 1948, i.e., before the first Soviet fission bomb was detonated on 29 August 1949, *Andrej Sacharov* (1921– 1989) had already been engaged for the task of producing a Russian hydrogen bomb. The hydrogen bomb research in Russia used lithium deuteride at an early stage. Fuchs' information on the early American hydrogen bomb research had not been of much use to the Russians, but nor had it put them on the wrong track. The construction that was produced was called 'the layer model' (the Russian word used, 'sloj', means 'layer') and the Americans used the term 'layer cake'. This model uses layers of uranium and fusion material.

In 1952, General *Dwight Eisenhower* (1890–1969) succeeded Truman as President. On 5 March 1953, the Soviet Union's head of government Marshall *Joseph Stalin* (1879–1953) died and was succeeded by *Georgy Malenkov* (1902– 1988). However, the head of the Soviet Security Service the NKVD (a predecessor of the KGB), *Lavrentiy Beria* (1899–1953), continued to be responsible for nuclear weapon production until he was overthrown on 26 July of the same year.

On 17 March, the Americans started a new series of tests called Operation Upshot-Knothole in Nevada, which continued until June. The AEC did not announce the decision to re-incorporate Bikini into the test area until April 1953: 'In order to accommodate of developing and testing new and improved nuclear weapons the United States Atomic Energy Commission is enlarging the Pacific Proving Ground in the Marshall Islands to include Bikini as well as Eniwetok atoll'. This was because they were now starting to plan the testing of hydrogen bombs in 1954 and Enewetak atoll was not large enough for this.

In August 1953, the Russians detonated two nuclear charges, one of which, on 12 August and nicknamed 'Joe-4' by the Americans (although Stalin was now dead), contained a fusion charge with both lithium deuteride and tritium. The American government set up a committee under the German-born physicist *Hans Bethe* (1906–2005) to evaluate the information on the Soviet test. The conclusion was that Joe-4 had not been a super bomb, i.e., a proper fusion weapon, but a boosted fission bomb and that the explosion strength corresponded to around 500 kton. However, the boundary between a 'boosted fission weapon' and a 'fusion weapon' is not all that interesting. In the American detonation in Operation Greenhouse on 24 May 1951, they had used only a few grammes of tritium and the Russian test had used kilogrammes of the fusion charge, and 15–20 % of the energy is calculated to have come from the fusion process. However, the Russians were not pleased; the layer cake model was unable to achieve an explosion strength greater than 1 Mton and the Teller–Ulam bomb (Mike) was more than 10 Mtons. However, neither the USA nor the Soviet Union published any details of the Russian bomb's explosion strength or construction. *Pravda* and *Izvestia* simply said that the Soviet Union had tested 'one of the types of hydrogen bomb'. *N.A. Vlasov* has described his impression of the explosion site at Semipalatinsk following the detonation (Holl, 1994):

> The general impression of a terrible and huge destructive force took shape already at a distance. Yes, the explosion had indeed been far more powerful than the explosion of the atomic bomb. The impact of it apparently transcended some kind of psychological

> barrier. The effects of the first atomic bomb explosion had not inspired such flesh-creeping terror, although they had been incomparably more terrible than anything seen in the still recent war.

The corresponding explosion strength was still 'just' half a megatonne of TNT. *Igor Kurchatov* (1903–1960), who was the main person responsible for the Soviet nuclear weapon research, bowed in the direction of Sacharov who had awaited the detonation with audible palpitations (according to a colleague who was present). 'Thanks to you, the saviour of Russia!' he is meant to have said.

When taking over from Truman in 1952, President Eisenhower had inherited the cold war and an enormous production of nuclear weapons with a stock of more than one thousand American bombs. Stalin's death and Beria's fall on 26 June and summary execution at the end of December of the same year did, however, create conditions for a change in the relationship with the Soviet Union. On 8 December 1953, Eisenhower proposed to the UN general meeting an 'Atoms for Peace' programme. This proposal involved the big powers surrendering parts of their nuclear weapons arsenal and transferring the fissile materials to an international organisation which would monitor their use for peaceful purposes.

Eisenhower's apprehension about the development with the threat of nuclear weapons spreading is shown by the notes in his diary (according to Gims, 1985) in which he says

> the clear conviction that as of now the world is racing toward catastrophe - that something must be done to put a brake on this movement.

However, Eisenhower's proposal was not possible for the USA with the old Atomic Energy Act from 1946. Using skilful diplomacy, he succeeded in pushing through a proposal for a change to the law, and in 1954 the Congress adopted a new Atomic Energy Act which made it possible to implement Eisenhower's plans. The new Act also gave the private industry access to necessary technical information without participating in any nuclear weapons programme. During this development, an objection arose between the AEC's chair Strauss and the then chair of the Congress' Joint Committee on Atomic Energy, Senator *Clinton P. Anderson* (1895–1975). The AEC was prepared for cooperation with the industry; the Congress wanted to see a stronger role for the government.

On 19 March 1954, the USA gave the Soviet Union a proposal for an International Atomic Energy Agency. Following initial hesitation and criticism, Moscow agreed in August 1955 to participate in discussions with the USA and others to write statutes for the proposed IAEA (see also Chapter 14). At the same time, the Russian scientists were looking for new ideas for weapons production. In spring 1954, they came up with the equivalent of the Teller–Ulam construction, an idea which was worked out by Sacharov and *Jakov Zeldovich* (1914–1987) with the support of *Yuli Khariton* (1904–1996) and Igor Kurchatov, the 'Third Idea'.

The USA continued with the development of a bomb based on lithium deuteride. Both Los Alamos and Livermore now became active and both intended to assist with the big series of tests, Operation Castle, which would be started on the Bikini atoll on 1 March 1954. However, the fusion bomb which would then be detonated in a blast called Bravo was constructed in Los Alamos. The Livermore scientists found themselves still in an early experimental stage.

That which would detonate in the Bravo test was a very special fusion bomb containing lithium deuteride. The detonation took place on the morning of 1 March, local time. The cloud rose high up into the stratosphere. The fission products had been expected to travel northwards but the wind had changed direction. A radioactive cloud was carried in an easterly direction over the atoll islands of Rongelap and Rongerik and over the Navy's ship which had been situated on what was thought to be the windward side of Bikini for observations. When increased levels of radiation were observed on the ships an hour after the explosion, the crew were commanded to go below deck.

The atoll islands, where more than 200 Marshall-islanders and 28 American military personnel had been, were evacuated over the next two days. All ships were washed clean of the fallout, which was described as being similar to snowflakes. On 9 March, the AEC had organised a medical group to be flown out to Kwajalein, one of the southernmost Marshall atolls, to which the evacuees had been taken.

John Bugher and the head of his medical section *Charles Dunham* (1906–1975) also came to the Marshall Islands.

The upshot was that 'a significant amount of penetrating irradiation to the entire body had been received and that extensive contamination of the skin and possible internal deposition of radioactive materials had occurred'. However, no-one seems to have received life-threatening doses of radiation. It was not yet possible to estimate how long the contaminated Atolls would remain uninhabitable. The explosion strength of the bomb was estimated to correspond to 15 Mton of TNT. Herbert York wrote: 'The very first test in that series, the Bravo test, was of a device using LiD as its fuel and yielding 15 megatons. It was in a form readily adaptable for delivery by aircraft, and thus was the first large American hydrogen bomb'.

In May 1953, the eastern border of the blocked-off area had been placed just west of the Alinginae atoll west of Rongelap and Rongerik because it was thought that the radioactive cloud would move to the west or north and that there was no substantial risk to the east. However, the cloud that emanated from Bravo yielded considerable fallout far beyond this border.

No-one observed the Japanese trawler *Fukuryu Maru* (Lucky Dragon) which travelled with its long lines laid out in calm waters eighty-five nautical miles east-north-east of Bikini. The ship lay north of Naen, the north-westernmost island of Rongelap and was passed by the radioactive cloud from Bikini. The crew had seen the ball of fire from the explosion and had perceived it 'as though the sun had risen in the west'. They had also been exposed to the shock wave. Working on the time difference between the visual appearance and the shock wave (approx. seven minutes), the radiotelegraphist *Aikichi Kuboyama* located the explosion to Bikini.

The crew on the *Fukuryu Maru* guessed it was an atomic bomb explosion but were surprised that no warning had been issued. A few hours later, a bank of fog had formed and a light drizzle had begun to fall. The rain changed into small particles like white sand or ash which irritated the eyes. Some of the men collected the ash in bags as a souvenir. Some wondered whether it had any connection with the atomic bomb, but there had been no news of any ash raining down in Hiroshima or Nagasaki.

The ash fell for four hours. The men washed the deck and tried to wash the ash off themselves. At night, several felt sick and vomited and the 'grains of sand' caused their eyes pain. Blotches on their throats and heads began to burn. After around twelve days, some of the men had tufts of hair coming out.

On the Sunday morning of 14 March, the *Fukuryu Maru* entered its native port of Yaizu. They had had a bad journey, lost half of the fishing lines and managed only an insignificant catch. The owner of the boat *Kakuichi Nishikawa* was waiting on the pier. Fishing Master *Yoshio Misaki* rang the local hospital but the hospital could not get hold of anyone because it was Sunday. Misaki therefore approached the doctor, Doctor *Toshisuke Ooi*, at his home. Ooi promised to see the men at the hospital in the afternoon.

Ooi thought that radiation sickness was a possibility but was thinking of direct radiation from the bomb and could not understand why there could be any damage at such a long distance. Because the men were still alive after two weeks, he (wrongly) believed that the injuries were not life-threatening.

Misaki nonetheless insisted that some of the men be referred to a larger hospital in Tokyo. Ooi slightly resentfully wrote a referral for two of the men: 'The above-mentioned persons, while fishing at Bikini Lagoon area, among 23 crewmen aboard the boat, seemed to have been taken with radiation sickness(?) on March 1. They are supposed to be suffering from the atomic cloud of an H-bomb. I humbly beg your honourable consultation'. At the university hospital in Tokyo, Professor *Shimizu* decided to admit the patients for examination.

On Tuesday 16 March, the Tokyo newspaper *Yomiuru Shimbun*, who had caught the rumour of the news, single-handedly published a comprehensive report: 'JAPANESE FISHERMEN ENCOUNTERED ATOMIC BOMB TEST AT BIKINI. 23 Men Suffering from Atomic Disease. One Diagnosed Serious by Tokyo University Hospital. H-BOMB?'

The first scientist on site was Professor *Takanobu Shiokawa* from the closest city, Shizuoka, where he worked at the university's chemical laboratory. The County Administrative Board's healthcare department asked him to travel to Yaizu and measure the radiation. Shizuoka is on the coast of Suruga Bay around 150 km south-west of Tokyo and had half a million inhabitants. Shiokawa travelled to Yaizu on 16 March carrying a Geiger counter and visited Doctor Ooi at the hospital where five of the crew were held. Shiokawa found that they were contaminated with radioactive substances.

Shiokawa then went to the *Fukuryu Maru* and measured an exposure rate of 25 milliröntgen per hour at the gangway and 100 milliröntgen (approx. 1 millisievert) per hour at the roof of the aftcastle.* Shiokawa asked for the boat to be moved from its mooring and placed in quarantine. During the 20 minutes for which Shiokawa was examining the ship, his pen dosimeter had registered a total of 16 milliröntgen, corresponding to around 0.15 millisievert.

Professor of Biophysics *Yasushi Nishiwaki* and his American wife *Jane* were in Osaka. They read the report in *Yomiuru Shimbun* but found it incredible that a ship so far away from Bikini could be exposed to serious radioactive fallout. Because no other newspapers had written about the case, the report was thought to be based on a misunderstanding. However, another newspaper rang Nishiwaki for an interview, asking what he thought about the matter. Could the fish be contaminated?

Nishiwaki asked if any fish from Yaizu had come to Osaka. He then received an official request to examine the fish on Osaka's fish market. When Nishiwaki held his instrument in front of a tuna fish, the counter rat-tatted out 2 000 pulses per minute. Terrified surrounding people said 'The fish are crying!'

Nishiwaki was asked whether the fish were unusable, but because he did not know which radioactive substances were involved, he was unable to answer. He took samples to his laboratory and found that small pieces of the fish also led the counter to register 60 000 signals per minute. Nishiwaki asked himself how the trawler and its crew could be getting on. The following night, he and his wife travelled to Yaizu and visited the trawler the next morning.

Like Doctor Shiokawa, Nishiwaki found high levels of radiation on the trawler and measurable radioactive contamination of the crew at the hospital. Which doses of radiation had the men received? To estimate this, Nishiwaki needed to know what 'the ash' consisted of. He wrote an open letter to the American Atomic Energy Commission (AEC) and left it with an American news agency to forward the request. However, the head thereof thought Nishiwaki was an 'alarmist who was obviously seeking publicity' and did not forward the letter.

In the afternoon, the *Fukuryu Maru* was also visited by Professor *Masanori Nakaidzumi*, a well-known radiologist from the University of Tokyo. Nakaidzumi explained that the activity of the radioactive substances was too insignificant to constitute any danger and that he thought that the crew would soon become healthy. However, the Japanese population was now scared and millions of Japanese believed that they would die if they ate the 'nuclear-contaminated' fish. Information that fish had been removed from the Emperor's menu did little to improve the situation.

Attempts to state the limit values for the measurement results and thereby get the consumers to accept fish with lower values was unsuccessful. *Ralph Lapp* writes: 'They looked at the situation in an all-or-none light. Either the fish was radioactive (and therefore dangerous to health) or it was non-radioactive and safe to eat'.

In Tokyo, the two admitted crew members were examined by Doctor *Masao Tsuzuki* (later Japan's representative at UNSCEAR), who ascertained the following about the worst affected: 'The whole clinical picture of the fishermen was a typical form of an acute dermatitis (inflammation) which is usually observed in the case of acute superficial skin injury caused by an overdose of soft x rays'. No prognosis could be given at the time, but the doctors concluded that the other crew members needed medical treatment.

A group of doctors and nurses therefore went to Yaizu under the leadership of Doctor *Kazuo Miyoshi*, the Tokyo hospital's blood expert. Miyoshi decided that all of the crew members had to cut off their hair and also cut their nails, which were still radioactive. In Tokyo, chemist and Professor *Kenjiro Kimura* attempted to analyse the samples of the atomic ash he had got in order to produce a basis for estimating doses.

On 18 March, the head of the Atomic Bomb Casualty Commission (the ABCC), Dr. *John Morton*, came from Hiroshima to Tokyo and continued on to Yaizu. Morton offered help with flying the patients

* See the next Chapter for information on the quantity of *exposure*.

to Tokyo for better care. However, the American statements, although well-meaning, were naively worded. Ambassador Allison said on 19 March: ‘Our joint investigations of the incident are already under way and will, I believe, lead to findings which we can both accept. In advance of those findings, I am authorized to make clear that the U.S. is prepared to take such steps as may be necessary to insure fair and just compensation if the facts so warrant.’

The final reservation stirred up bad blood – were there doubts as to the injuries or as to the *Fukuryu Maru’s* trip? A statement by Senator *Sterling Cole* (later first Director General of the IAEA), who succeeded Senator McMahon as chair of the Congress’ Joint Committee on Atomic Energy was perceived as an accusation that the *Fukuryu Maru* had been tasked with spying in the Bikini area. The unwillingness of the Americans to provide clear information on what had happened and what the ash could consist of also aroused vexation. However, *Merril Eisenbud* (1915–1997), head of the AEC’s New York Operations Office, came to Tokyo and Yaizu and took his own measurements and helped the Japanese with information on which radioactive nuclides could be involved. When giving evidence on 22 March, Doctor Tsuzuki said that it was not possible to preclude the fact that a few of the crew members (‘ten per cent’) could die of their injuries; ‘acute radiation disease is never to be belittled’. Finally, the crew members were flown to Tokyo to be admitted to the university hospital (five of them) and the Daiichi hospital (sixteen men).

The importance attached to the event by the people in Japan and the devastating effect that the apprehension regarding radiation had on the fish sales was not reflected in American newspapers. On 17 March, the *New York Times* wrote that ‘a radioactivity of 7.5 millimetres (!) had been measured’. The AEC declared that there was no risk in eating fish from the Pacific Ocean, yet at the same time, American importers refused to accept fish that would have been allowed to be sold in Japan.

In April, the blood count of the patients had deteriorated and their general health was poor, but in mid-May there was an improvement. However, some of the crew members were infected by jaundice through blood transfusions. The jaundice killed radiotelegraphist Kuboyama on 23 September 1954. The Japanese thought that the death was actually due to the radiation that had reduced Kuboyama’s level of resistance while the Americans thought Kuboyama would not have died had he not been infected by jaundice at the hospital. The relations between the USA and Japan became tense. Senator McCarthy’s pursuit of Communist sympathisers in 1953–1954 meant that criticism of the USA was often seen as part of a Communist conspiracy. At the start of 1954, even Oppenheimer was accused of non-American activities (this was not redressed until 1963).

In this tense situation, it was not easy for the Japanese to obtain information from the Americans. What did the ‘atomic ash’ that had rained down over the *Fukuryu Maru* consist of? Merril Eisenbud, who would have happily shared his knowledge, did not dare to reveal any secret - he could do no more than refer to Professor Kimura in Tokyo.

In turn, Kimura was surprised at his analysis results; he had found radioactive isotopes that included tellurium, niobium and lanthanum as well as strontium-90, but to his surprise he found also uranium-237, which he interpreted as a product of the irradiation of uranium with fast neutrons. Because the bomb was assumed to be a hydrogen bomb that was initiated by a plutonium charge, the conclusion was that it must have been supplied with a tamper made of natural (or depleted) uranium. The powerful super bomb had been what would eventually be called a ‘dirty’ bomb. The fission of the uranium had given rise to large quantities of fission products that yielded substantial radioactive fallout. Rolf Sievert in Sweden also drew the same conclusion when he processed the recordings that he had received from the pressure ionisation chambers at his measurement stations.

The contamination of fish was a serious event in Japan and the Japanese, who did not trust what they heard from America, prepared to take their own measurements. This is where *Yoshio Hiyama*, who later went on to form part of the Japanese delegation for the UN’s Scientific Radiation Committee UNSCEAR, played an important role. On 15 April 1954, the research vessel *Shunkotsu Maru* departed from Tokyo under the captaincy of *Hiroshi Yabe* to take their own measurements. Donaldson arrived one week later, too late to be able to take part in the expedition. Donaldson had to make do with meeting Hiyama. The *Shunkotsu Maru* returned to Tokyo on 4 July. The examinations of the material that had been collected were led primarily by geochemist *Yasuo Miyake*.

The *Shunkotsu Maru* was not the only Japanese ship that was sent out to take measurements of the radioactive contamination in the Pacific Ocean. On 28 October, the *Keiten Maru* began a measurement expedition that lasted until 28 January 1955. And on 30 November, the *Daifuji Maru* departed for an expedition that lasted until 19 February 1955.

On 15–19 December 1954, a conference was held between Japanese and Americans in Tokyo to discuss the contamination problem. From the American side, the conference participants were the head of the biophysics section of the AEC's Division for Biology and Medicine, Dr. *Walter Claus*, Merril Eisenbud and Eisenbud's closest aide at the AEC's New York Operations Office, *John Harley* (1916–1993). The Japanese delegation of 15 men included Hiyama, Kimura, Miyake and Nakaidzumi. The main thing that worried the Japanese was the possibility of the contaminated water around Bikini travelling towards Japan.

Eisenbud reported this anxiety on returning to the USA. He consulted experts from the Woods Hole Oceanographic Institution at Cape Cod and from the Scripps Institute of Oceanography in La Jolla, California. He heard that there was a great risk of the radioactive contamination reaching Japan and that they ought to look at the extent to which the radioactive substances were taken up by plankton and plankton-eating fish along the way. Eisenbud's initiative resulted in the first American oceanographic survey of the extent of the problem. The expedition was named Operation Troll. The surveys were carried out using the cutter *Roger B. Taney* from the American coast guards under the command of Captain *Albert Carpenter*. The expedition was led by John Harley. It confirmed the suspicions of the Japanese that radioactive substances were being transported in a westerly direction towards Japan, but the dilution and the radioactive decay meant that the concentration was so low that it did not lead to any health risks. The *Roger B. Taney* returned to San Francisco on 3 May 1955.

Another four test blasts in the Castle series were carried out until 5 May 1954. In July, the inhabitants of Rongelap were moved from Kwajalein to the island of Ejit in the Majuru atoll around 500 km east-south-east of Kwajalein.

On 18 February 1955, a series of 14 test detonations was started in Nevada under the name of Teapot, and in May of the same year, an underwater detonation named Wigwam took place off the west coast of the USA. After that, the military took activity measurements in tuna fish in the ports. Nuclear charge tests were also done in Nevada in 1957 and 1958 (23 and 15 respectively). The most powerful charge (1957) corresponded to 74 ktons of TNT.

In the AEC, Charles Dunham had succeeded John Bugher as head of the Division for Biology and Medicine. The radioactive fallout now began to worry the public in several countries. At its 10th session in 1955, the United Nations' general meeting decided to set up the scientific committee that is known by the name of UNSCEAR (the United Nations Scientific Committee on the Effects of Atomic Radiation). The United Kingdoms' Medical Research Council published a report called *The Hazards to Man of Nuclear and Allied Radiations*. The American Academy of Sciences along with the Federal Research Council published a number of 'white papers' on the biological effects of what was figuratively known as 'atomic radiation'. The content was summarised in a report to the public. The introduction to this said:

> Behind any discussion of radiation must necessarily loom the spectre of full-scale atomic war. That a single thermonuclear weapon can cause severe radiation damage hundreds of [kilometres] beyond its area of immediate devastation is all too well known. That enough such weapons exploded in an all-out war might render the entire Earth, or large parts of it, uninhabitable, is at least conceivable. […] There has been comparatively little attempt in the study thus far to estimate the possible courses of atomic warfare or to assess the biological consequences. The present emphasis has been on peaceful development. It should be pointed out, however, that so far as radiation is concerned, the two aspects are not entirely unrelated.
>
> In the first place, when a world-wide atomic power industry becomes fully developed, its accumulated waste products might represent more radiation than would be released in an atomic war. Of course, this radiation will be imprisoned, not broadcast. But the point underscores the magnitude of the coming problem.

> Secondly, it becomes clear in this report that even very low levels of radiation can have serious biological effects. […] Thus, many of the disastrous consequences of atomic war are clearly implied in this investigation of peacetime problems.

An article in *Science* in summer 1956 caused a great sensation. Two Chicago scientists, Argentinian-born *Leonidas Marinelli* and *Charles ('Chuck') Miller*, showed that by using a scintillation counter it was possible to demonstrate caesium-137 from the nuclear weapon tests in the Pacific Ocean in the bodies of people who lived in Chicago. Such a long-distance spreading of radioactive substances underlined the risks of the nuclear weapon tests (and nuclear war). The radioactive fallout had turned into a global problem.

In 1956, Senator *Albert Gore* (father of the subsequent Vice President) and Congressman *Chet Holifield* proposed a bill which required the AEC to build six different demonstration reactors as soon as possible to show that they had not fallen behind the United Kingdom and the Soviet Union. The Gore–Holifield bill was never adopted and the chair of the AEC, Strauss, declared that the USA had a civilian programme that was ahead of what had been expected in 1954.

In May 1956, the Americans started a new series of tests under the name of Redwing involving the use of Bikini and Enewetak. This series would also include thermonuclear charges. It was now inconceivable to implement these experiments without taking accurate measurements of the radioactive contamination. On 28 March, Dunham asked Donaldson and the Applied Fisheries Laboratory to carry out two measurement expeditions, one while Operation Redwing was ongoing and one at a later date. For the first expedition, Donaldson would use destroyer escort *USS Walton* and measurement and sampling equipment provided by the AEC's New York Operations Office with experiences from Operation Troll during the cutter *Roger B. Taney's* journey.

In June, the *USS Walton* arrived in Enewetak to be equipped and then started its ten-day cruise to chart the initial contamination during the ongoing test detonations. In September, the second expedition began, now with the help of the escort destroyer *USS Marsh*. They followed the radioactive contamination westwards towards The Marianas, although the latter showed very low concentrations. As expected, the most sensitive indicator of the contamination was plankton.

The early radioactive fallout was even noticed in Sweden. Sievert, a person with foresight who had taken the initiative for a number of measurement stations in different parts of Sweden, was able to record elevated levels of radiation at an early stage (see Chapter 13). The measurement stations were equipped with pressure ionisation chambers that were very sensitive to gamma radiation. The people who were taken on to see to the stations were instructed to immediately call Sievert if the recording indicated increasing radiation. Radioactive fallout was registered when it rained, even before any tangible contaminations from the nuclear weapon tests reached Sweden. The rain cleaned the air which usually contained radioactive daughter product of the radon that had emanated up from the ground when it had been formed there when the naturally-occurring radium decayed. By observing how rapidly the radiation decayed, Sievert was able to see when the fallout had a natural origin. Temporary elevation of the gamma radiation from radioactive fallout for other reasons was something that Sievert already thought he could see in December 1950. The recording of the gamma radiation in autumn 1951 and measurements of beta radiation particles clearly showed artificial radioactive fallout which Sievert attributed to Soviet nuclear weapons testing.

Sievert tried to correlate his recordings with information on the nuclear weapons tests. On 29 February 1952, Lieutenant Colonel Torsten Schmidt of the Defence Staff gave him a summary of the times of the nuclear weapons tests carried out by the USA, around twenty in all, and started to compare the way in which they corresponded with the times of the disruptions he had observed. The disruptions were unwelcome because they made Sievert's measurements of the natural radiation from the human body difficult, but Sievert possessed the capacity of a true scientist to turn undesired observations into something useful.

The Soviet explosion of 12 August 1953, the one the Americans called 'Joe-4', was not just very strong, causing severe radioactive contamination. One of Sievert's measurement stations showed a thirty-five per cent elevation of the level of radiation. Sievert, who at the time had only just recovered after the fiasco with the high voltage hall, was not slow to invest further efforts into the measurement stations. In April 1954, he wrote to the Prime Minister, the Foreign Secretary, the Supreme Commander of the Armed Forces and the Atomic Committee and gave an account of the measurement stations and the radioactive fallout. With his boyish fascination which preceded anything that was exciting and secret, he wrote in the accompanying letter: ‘There are only 5 numbered originals of this document, distributed as follows’ (the fifth original was his own). He also wrote:

> No radiation owing to atomic explosions that is dangerous from the health point of view has been observed in Sweden so far. However, recordings made support the assumption that if nuclear weapons were to be used to a great extent in one of our neighbouring countries in a future war, radioactive particles in unfavourable weather conditions might lead to serious radiation risks for the population of our country.
>
> […]
>
> The hydrogen bomb explosion of 1 March has not yet left clear traces in the Swedish recordings that are referred to here.

Observations of the decay speed of the beta radiation from measurements between 1951 and 1952 had led Sievert to find signs that one of the radiating substances could be neptunium-239, which is formed by irradiating uranium-238 with neutrons. This would mean that the bombs had contained not only the fissile substances plutonium-239 and uranium-235 but also non-fissile natural uranium, uranium-238. Such an arrangement had been proposed by *Leo Szilard* (1898–1964) as early as 1934 (!) and the objective would be to reduce the critical mass of the fissile material if this were surrounded by ‘some cheap, heavy metal’ that would reflect back neutrons which would otherwise have been lost. Such a reflector is usually called a ‘tamper’. Sievert concluded that the bombs had been supplied with a uranium tamper and chuckled with jubilant satisfaction at having been able to reveal this before the scientists at the FOA.

In April 1953, the FOA had also begun to measure the beta radiation from the precipitation and in 1956, the Danish Atomic Energy Commission started to measure the radioactive contamination in the air. In the same year, the FOA began using an electronic multi-channel pulse analyser* on its premises on Drottning Kristinas väg next to the Royal University of Technology in Stockholm. In 1956, the FOA published a number of important reports on the characteristics of the fission products in radioactive fallout. The FOA had a proficient group of scientists in this field, including *Bo Aler* (1926–), *Rolf Björnerstedt* and *Kay Edvarson* (1925–2006), who started at the FOA in summer 1955, and the popular *Kerstin Löw*, who was missed by everyone when she suffered an untimely death at a young age.

In 1956, Sievert and radiation protection inspector *Carl Gösta Rylander* published a release in *Nature* regarding increasing gamma radiation from meat and dried milk during 1953–1956 and assumed that the radiation originated from caesium-137. This was the start of more comprehensive measurements on food which it was possible to take when Sievert’s institution immediately thereafter gained access to a gamma spectrometer. In 1956, the FOA also started to measure particle samples that were collected using aircraft.

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* The energy spectrum of gamma radiation was examined by analysing the size of the current pulses formed in a photocell when it is hit by the flashes of light (scintillations) that arise when the photons of the gamma rays are absorbed in a sodium iodide crystal. The analysis equipment that recorded the current pulses was able to sort them into a number of ‘channels’ and the number of pulses in each channel was shown on the screen of an oscilloscope or was registered by a number of counters. Because different nuclides emitting gamma radiation have different photon energies and the size of the pulses is determined by the photon energy, the pulse-height spectrum that is shown in a pulse analyser can provide information on which nuclides the source of radiation consists of. The multi-channel pulse analyser that was used the most in the mid-1950s was the Hutchinson-Scarrot.

A list of nuclear charge explosions of a strength exceeding 4 megatonnes of TNT carried out during 1946–1966, plus some early tests that were mentioned separately

The following information has been taken mainly from the Hine 1962 and UNSC 2000 references. Please note that the dates refer to American time in Washington. The big Bravo explosion in 1954 is therefore assigned to 28 February whereas it took place on 1 March local time.

Date	Country	Name, series	Test site	Blast strength (megatonnes of TNT)	
24/07/46	USA	Baker in Crossroads	Bikini	0.021	
29/08/49	Soviet no. 1	('Joe-1')	Semipalatinsk	0.022	
24/09/51	"	no. 2 ('Joe-2')	"	0.038	
18/10/51	"	no. 3 ('Joe-3')	"	0.042	
31/10/52	USA	Mike in Ivy	Enewetak	10.4 of which	5.2 fusion
12/08/53	Soviet no. 4	('Joe-4')	Semipalatinsk 0.4	0.07 (?)	
28/02/ 54	USA	Bravo in Castle	Bikini	15	7.5
26/03/54	"	Romeo "	"	11	5.5
25/04/54	"	Union "	"	6.9	3.45
04/05/54	"	Yankee "	"	13.5	6.75
10/07/ 56	"	Navaho in Redwing	"	4.5	2.25
20/07/ 56	"	Tewa "	"	5	2.5
28/06/58	"	Oak in Hardtack	Enewetak	8.9	4.45
12/07/58	"	Poplar "	Bikini	9.3	4.65
04/10/61	Soviet	no. 113	Novaya Zemlya	4	2
06/10/61	"	no. 114	"	4	2
23/10/61	"	no. 123	"	12.5	6.25
30/10/61	"	no. 130	"	50	48.5
31/10/61	"	no. 131	"	5	2.5
04/11/61	"	no. 141	"	6	3
27/ 06/62	USA	Bighorn	Christmas Island	7.6	3.8
05/08/62	Soviet	no. 147	Novaya Zemlya	21	10.5
25/08/ 62	"	no. 158	"	4	2
27/08/62	"	no. 160	"	4.2	2.1
19/09/62	"	no. 168	"	4	2
62-09-25	"	no. 173	"	19	9.5
27/09/62	"	no. 174	"	17.6	8.8
22/10/62	"	no. 183	"	8.2	4.1
30/10/62	USA	Housatomic	Johnston Island	8.3	4.15
24/12/62	Soviet	no. 219	Novaya Zemlya	24.2	12

6. THE RESURRECTION OF ICRP, 1950–1955

The first International Congress of Radiology (ICR) after the Second World War, making the sixth in total, was held in London in summer 1950. At the same time, ICRP was to meet for first time after the meeting that had been held in Chicago in 1938 in connection with the fifth ICR. The original idea had been to hold the sixth Congress in Hamburg with Professor *Hermann Holthusen* (1886–1971) as President, but the idea of holding a big international Congress in Germany was still inconceivable at the time.

During the war, Lauriston Taylor had been responsible for both ICRP and ICRU at the request of the Honorary Secretary of ICRP, *G.W.C. Kaye* (1880–1941). In 1947, he had notified the President of the Chicago Congress, *Dr. Arthur Christie* (1879–1956), of the situation after the war. The only members of ICRP who had survived the war were Taylor and Rolf Sievert. They were also the only survivors of ICRU's working committee.

After having received Taylor's report, Christie contacted British radiologist Ralston Paterson, who was to be President of the London Congress. Christie suggested that Paterson ask Taylor to re-establish the two Commissions. Paterson agreed to the suggestion and asked Professor Mayneord to assist Taylor.

Following an exchange of letters, Mayneord and Taylor met at the National Bureau of Standards where Taylor was head of the Department of Radiation Physics. At their suggestion and following a further exchange of letters with Christie and Paterson, they drew up a list of nine people who would be invited to become members of the revived ICRP. All of these nine accepted the invitation, so the re-established ICRP was made up of the following:

Sir Ernest Rock Carling, the United Kingdom, Chair
Lauriston Taylor, USA, Secretary
Walter Binks, the United Kingdom
E.L. Chérigié, France
A.J. Cipriani, Canada
Robert Jaeger, Germany
W. V. Mayneord, the United Kingdom
R.R. Newell, USA
Rolf Sievert, Sweden

At the time, the chairmanship concerned the actual meeting and, until then, ICRP had, as was now also the case in London, met on one day only in connection with Congress of Radiology. Even the task of the Secretary had been unclear. Lauriston Taylor writes the following about this in his extensive history (Taylor, 1979):

> The question of officers for ICRP and ICRU was one of the continued confusion. Initially ICRP, when it was established in 1928 in Stockholm, had two Honorary Secretaries. Later one of these withdrew and the other sort of became chairman. At the same time it was customary to have an Honorary Chairman chosen from the country in which the Congress was being held. On at least one occasion two such honorary chairmen were designated but neither seemed to understand that this was an honorary, one-time position and continued to believe that they were members of the Commission. A similar confusion existed within ICRU. Hence it was decided at the London meeting

> of both Commissions in 1950 that the secretary would be elected from among the membership and would be the one continuing officer, with the presumption that the chairman would be selected prior to each Congress and from the country in which the meeting was being held. At the London Conference Dr. Paterson, President of the Congress had appointed Sir Ernest Rock-Carling as chairman of ICRP and Dr. W. V. Mayneord as chairman of ICRU. In the meantime, L. S. Taylor was the carryover secretary for both commissions. This was recognized as undesirable because of the workload as well as having both secretaries from the same country, not to mention being the same person. Hence by arrangements with ICRU it was agreed that Taylor would continue as secretary of ICRU and that Walter Binks would become secretary of ICRP with the office becoming effective at the close of the London meeting. The matter of the Chairman or Honorary Chairman was to be taken care of between the time of the London meeting and the next meeting of the Commission which was then scheduled to be held in Copenhagen in 1953.

Sievert travelled to London by boat from Gothenburg, accompanied by Lars-Eric Larsson who would assist him with the practical arrangements. Sievert saw trips as hazardous undertakings. Ships could sink, aircraft fall and trains crash. Larsson has said that Sievert was afraid of going down into his cabin and therefore kept him in the ship's bar for most of the night. 'Well I'll be darned – that's the best excuse I've heard for staying in a bar!' commented Lauriston Taylor when I told him the story.

From Sievert's point of view, the most urgent item on the agenda was to agree a dose limit. What was referred to as a 'dose of radiation' at the time was actually the quantity *exposure*, which was expressed in röntgen. 1 röntgen was defined as

> that quantity of X- or gamma radiation such that the associated corpuscular emission per 0.001293 gram of air produces, in air, ions carrying one electrostatic unit of quantity of electricity of either sign. It is to be noted that 0.001293 g is the mass of 1 cm^3 of dry atmospheric air at 0° C and 760 mm mercury pressure.*

The meaning of a 'quantity of x-radiation or gamma radiation' was not clarified until 1962 when ICRU interpreted it as the energy fluence (joule/m^2) of the incident x or gamma radiation. The exposure was a measure of the ionisation caused by the incident radiation when it ionised one cubic centimetre of air. It made no difference whether the air really was there; it was an imaginary cubic centimetre. The exposure therefore stated what an irradiated body was exposed to rather than what the radiation brought about in the body.

However, during the Manhattan Project, people had begun to ask about the 'absorbed dose', i.e., the radiant energy that was actually taken up per unit of mass in an irradiated body. The unit for absorbed dose is now 1 joule per kilogramme but in the 1940s they preferred to have a unit that corresponded to the unit of exposure, i.e., 1 röntgen. *Herbert Parker* (1910–1984) proposed the unit rep (an acronym of röntgen equivalent physical), first defined as 83 erg/g but shortly thereafter as 93 erg/g. The former value corresponds to the energy absorption in air at an exposure of 1 röntgen and the latter to the absorption in soft tissue at the same exposure.

When *Sven Löfveberg* (1928–2009) was responsible for the training at the Radiation Protection Institute, he formulated the following simile to help explain the concept, which he attributes to Harold Gray (Löfveberg, 1986):

> A Professor lecturing to a group of students emits a flow of words. This flow of words, which enters one ear and exits the other ear of the student, can be likened to the amount of exposure in röntgen. The small amount that remains in the student's brain corresponds to the dose that is absorbed.

* In my series of books, dose quantities and dose units have been previously discussed in 'Pandora's Box' (Chapters 13 and 14) and in 'The Sword of Damocles' (Chap. 9).

The year before the Congress of Radiology in London, i.e., in 1949, Americans, Brits and Canadians had met for a tripartite conference in Chalk River. At Failla's suggestion, they had then agreed that the weekly dose limit for the external irradiation of workers should be 0.3 rep in the critical organ (which was assumed to be the bone marrow) in whole-body irradiation. It was recorded in the minutes that this limit corresponded to a weekly exposure of 0.3 röntgen free in air and 0.5 röntgen measured next to the irradiated body since secondary radiation from the body then increased the exposure.

The Chalk River agreement was discussed in London. Sievert and Jaeger appealed for a lower dose limit. In Sweden, Sievert had applied a limit of 0.1 röntgen per week. However, Sievert and Jaeger came off badly against the overwhelming Anglo-Saxon majority of countries that had already made their decision in Chalk River. Sievert returned home and reported that his old friend Mayneord had been 'difficult'.

An 8-page report from the 1950 meeting of ICRP in London (including Appendix) was published in the British Journal of Radiology in January 1951. This laid the foundations for what would lead to ICRP's first major report in 1955. By way of introduction, without mentioning the atomic bombs, reference was made to 'the development within nuclear physics' that had taken place since they had last met in 1938.

Much more was now also known about the harmful effects of radiation and there was talk of both hereditary injuries and cancer. They were in a quandary as regards finding a suitable quantity to limit, and wrote:

> While it is still Fundamental to express whole body exposure in terms of a single number it is not practicable, in general, in view of the complexity of circumstances now arising, to express the maximum permissible hazards in terms of a single parameter

The need to limit the concentration of radioactive substances in air and water was mentioned as an example, as well as the problem that arose in that the röntgen unit could not be used for all types of ionising radiation. When a dose limit for fast neutrons was recommended, reference was made to the absorbed dose for the first time and the following was written:

> […] The maximum permissible energy absorption per gramme of tissue exposed to fast neutrons should not be greater than one tenth of that permitted for high energy quantum radiation *[i.e., gamma radiation]*.

Limiting the absorbed dose from neutrons to one tenth of the dose permitted for gamma radiation involved applying the knowledge that was starting to be collected regarding the relative biological effectiveness (RBE) of different types of beam – the neutrons were considered to be ten times as effective as the reference radiation, gamma radiation. It was then said that RBE = 10.

An Appendix to the report stated Maximum Permissible Concentrations (MPC values) for some nuclides for the first time, but the emphasis was placed on estimates of the maximum permissible body content thereof. For bone-seeking nuclides, the values were based on comparisons with radium-226 for which the body burden was limited to 0.1 microcurie (= 0.1 microgramme). The nuclides for which the values were stated were (in the order in which they were discussed): radium-226, plutonium-239, strontium-89, strontium-90, polonium-210, tritium, carbon-14, sodium-24, phosphorus-32, cobalt-60 and iodine-131. For natural uranium, it was said that the activity is so low that the chemical risk dominates.

The Appendix also contained a table containing the recommended RBE values for different types of radiation and organ weights and the level of different elements in the body of what was called a 'Standard Man'.

What is less well known is that, despite having been published along with ICRP's own report, this Appendix did not actually originate from ICRP. In the introduction to the Appendix, the Commission says that it is 'not in a position to make firm recommendations regarding the maximum permissible amounts of radioactive isotopes that may be taken into or retained in the body'. The Commission then referred to 'the following data' which was 'presently used in the U.S., Canada and the UK'.

What had happened was that at the time of the Congress of Radiology in London, radiation protectors from the three Anglo-Saxon countries had gathered for a new 'Tri-partite Conference' along with a few ICRP members in Buckland House near Harwell. The material in the Appendix to the ICRP report consists of the result of this meeting. A more complete text was published in the NBS Handbook 47.

The exposure limit of 300 milliröntgen (free in air) for *every* week (i.e., not *per* week) led to the following comment, which is not that easy to read:

> Whilst the values proposed for maximum permissible exposures are such as to involve a risk which is small compared to the other hazards of life, nevertheless in view of the unsatisfactory nature of much of the evidence on which our judgments must be based, coupled with the knowledge that certain radiation effects are irreversible and cumulative, it is strongly recommended that every effort be made to reduce exposures to all types of ionizing radiations to the lowest possible level.

The unfortunate wording 'lowest possible level' is an example of thoughtless wording because most things are possible but not always reasonable. The wording led to some misinterpretations before it was eventually changed.

The meeting in London was the first time that ICRP appointed committees for different tasks. There were six in all (which would soon be reduced), namely:

I.	Permissible dose for external radiation.	Chair: G. Failla
II.	Permissible dose for internal radiation.	K.Z. Morgan
III.	Protection against x rays generated at potentials up to 2 million volts	R. Jaeger
IV.	Protection against x rays over 2 million volts, and gamma-rays and beta-rays	W. V. Mayneord
V.	Protection against heavy particles, including neutrons and protons	D. Cowie
VI.	Disposal of radioactive wastes; handling of isotopes	H.M. Parker

The irony of fate meant that Cowie was unable to fulfil the task because he was affected by a cataract owing to work with neutrons (!). At the ICRP meeting in Copenhagen in 1953, Professor Mayneord proposed that his own committee take over the protection against particle radiation. Committees IV and V then merged to form a new Committee IV, and Committee VI was thereafter known as Committee V.

The new Secretary of ICRP, Walter Binks, had been head of the newly-established Radiological Protection Service (RPS) since 1 January 1953, an organisation which had been set up jointly by the Ministry of Health and the Medical Research Council (MRC). The RPS had taken over some of the activities at the National Physical Laboratory (NPL) where Binks had been employed in 1926 and had been head of the radiology section since 1943. Binks was accompanied by *E.E. Smith* (1911–1998) who had been employed by the NPL before the war broke out in 1939 and who became his closest associate. This meant that Binks and Smith had long-term practical experience of radiation protection. During the war, they worked with one gramme of radium as the radiation source to radiograph fallen bombs that had not exploded. They estimated that the doses of radiation they had received corresponded to approximately 200 röntgen (i.e., around 2 sieverts) without obvious effects on their health. Binks certainly had tangible health problems but these may have been due to other causes. He was a nervous man who was often worried about his health. Eric Smith on the other hand was of a robust nature who liked swimming and playing hockey. He spoke with a cockney accent, and irreverent colleagues sometimes called him East End Smith with reference to his initials.

Over the next few years after the meeting in London there was an extensive exchange of letters between Walter Binks as the new Secretary of ICRP and a number of members. Binks proposed some new committee members. One of the things he mentioned was that Gray warmly recommended *E. E. Pochin* (1909–1990), a specialist in internal medicine who was head of the Department of Clinical Research at University College Hospital Medical School in London. He also suggested his colleague

Eric Smith. Taylor wondered whether the Smith to whom Binks was referring was the American Scott Smith, but Binks explained that he was referring to the British Eric Smith, who he explained was a British equivalent of the Bureau of Standards' *Harold Wyckoff* (1910–1999).

The Brits wanted to invite some French radiologist to some committee, and maybe then to the main Commission. Mayneord had written to *Lew Kowarski* (1907–1979) at the French Atomic Energy Commission and asked for advice. Kowarski strongly recommended Professor *Louis Bugnard* (1901-1978) at the *Institut d'Hygiene* in Paris. Binks supported the proposal and wrote to Taylor saying that Bugnard was a man whose advice was worth listening to. Binks also wrote that in turn, Bugnard had suggested a young radiologist by the name of *Henri Jammet* (1920–1996) who worked at both the Atomic Energy Commission and the Curie Hospital, but Kowarski had thought that Jammet was 'not sufficiently experienced as yet'.

When Chérigié had declared that he was unwilling to continue in ICRP's Main Commission, Bugnard suggested that he ought to be replaced by the young radiologist *Maurice Tubiana* and that physicist André Allisy would be a suitable candidate for ICRU.

Another matter which needed an early decision was who would act as Chair of the Commission at the meeting in 1953 during the Congress of Radiology in Copenhagen.

In a circular to the members on 9 June 1952, Binks reminded people of an agreement during the London meeting whereby the Chair of a meeting would be selected six months in advance and become an *ex officio* member of ICRP but only during the conference in question. Binks gave three options:

1. To re-elect the present Chairman (Sir Ernest Rock Carling);
2. To consult the Danish Committee [for the Congress] and ask if they would like to nominate a Dane as a Chairman;
3. To elect a new Chairman without consulting the Danish Committee.

Lauriston Taylor's response to this proposal was as follows:

> I feel very strongly in favor of the second suggestion, namely asking the Danish Committee to select a temporary chairman for that meeting. I am strongly opposed to the first suggestion. This casts no reflection whatever on Sir Ernest. As a matter of fact, I think he has made one of the best chairmen that we have had. On the other hand, I remember all too well the situation that existed prior to 1940 at which time the chairmanship was in Dr. Kaye's hands. Here was a situation where we had a man of long-standing reputation but who was no longer personally active in the field. Again there is nothing personal in my remarks in this regard. But it was the feeling of many members of the Commission in the earlier years that the work either lagged or was dominated by a single individual. This, I think, is a real mistake. The rules which were set up in 1937 Congress were designed with the idea of assuring a rotating chairmanship. The chairmanship was largely honorary and was not expected to carry over from one Congress to another. The main continuity of the [work of the] Commission was to be through the secretary who was elected by the committee members *[Taylor must have meant the Commission's members]*. The Secretary really did the leg work but at the same time he was supposed to be a person who was working very closely in the field and hence had intimate knowledge of the most recent developments.

It became apparent that the other members shared Taylor's view. The opportunity to discuss this further came later on in 1952 when several of them met in Stockholm. The reason was that Sievert, along with George de Hevesy, arranged an important radiation biology and radiation protection conference in autumn 1952. Those participating in the conference were members of ICRP, ICRU and UNESCO's* Radiation Biology Committee plus a few invited speakers.

* United Nations Educational, Scientific and Cultural Organisation, established in 1946. The interest in radiation effects increased more and more. The Radiation Research Society was formed in 1952. The first issue of the *Radiation Research* journal came out in February 1954. The first of the Society's Congresses was held in 1958 with de Hevesy as President.

Following a reception at Stockholm town hall, a unique group photo was taken from this meeting showing Gray and Sievert side by side, the two people to whom tribute would later be paid through their names being used as official SI unit names for absorbed dose and equivalent dose. The picture shows almost all of the most prominent radiobiologists and radiation protectors of the time (see p. 179). Just a few were missing, including Failla, Marinelli and Stone.

The meeting gave rise to hostilities within Sievert's institution. He had already told me a few years earlier that he wanted to see me take over from him and he had plans for me to become involved with the international radiation protection cooperation. He allowed me to participate in the meeting using the justification that he needed my help. This aroused displeasure among the radiation protection inspectors, primarily Thor Wahlberg who, not entirely without reason, thought that Sievert ought to seize the opportunity and offer the other personnel the option of education. But Sievert, who did not hold his radiation protection inspectors in particularly high regard when it came to anything other than practical supervisory experience, referred to the fact that the conference was a closed meeting and that the few people invited who were not members of the international committees were people of high standing in the scientific world, such as Nobel Prize winner Hermann Muller. However, Matts Helde was already invited owing to his blood count studies and Forssberg and Thoraeus were members of the organisation's committee.

The meeting was my first big exposure to international celebrities. There were older, imposing and renowned people such as Hermann Holthusen, Antoine Lacassagne and Sir Ernest. There were eccentric personalities like George de Hevesy, K.Z. Morgan, Hermann Muller, Val Mayneord and Boris Rajewsky. There were obliging, serious men such as Walter Binks, Harold Gray, Robert Jaeger and F. W. Spiers. There was the powerful Lauriston Taylor. And there were many more. But I was missing Failla.

Rajewsky showed respect in the face of authority as the Germans were wont to do. During our tour following a reception in the town hall he pulled me aside to express his disappointment at the facetious youth who was our guide. 'That young man is not showing your national monuments proper respect!' he said. Karl Morgan spoke a southern dialect that I had not yet become fully familiar with. He reminded me of Gary Cooper, but he surprised me by saying that he found Swedish beer to be the best in the world. Harold Gray emanated friendliness and interest in my movements. One day I gave him a lift from Skokloster in my rickety Opel Kapitän. He tactfully suggested that I might like to reduce my speed. Three years later I drove Sievert to Blekinge, this time in my little Volkswagen. He wondered whether I was able to drive a bit faster. What different reactions from the big men of the units!

Louis Harold Gray (1905–1965) is best known for the Bragg-Gray principle (so called because it was indicated by Sir Lawrence Bragg in 1912) which he published in 1936.* Gray, who was trained by Rutherford at the Cavendish Laboratory, had a future within nuclear physics, but his deeply religious persuasion – which was expressed in terms of gentle kindness rather than righteousness – led him to want to devote his life to something which helped people. He therefore started at Mount Vernon Hospital in 1933 as a medical physicist. While there, he drifted further away from physics towards biology. After the war, the Medical Research Council established a research department for radiation treatment at Hammersmith Hospital and Gray was invited to go there as a physicist. He was soon promoted to deputise for the boss who was radiologist Dr. *Constance Wood*. In 1949, they procured a 45-inch cyclotron for radiobiological research and the production of radioactive nuclides.

Gray did advanced cell research along with geneticist *Alma Howard*, although this was not appreciated by the MRC, who wanted research to be more clinically orientated. Constance Wood, who was perceptive to the wishes of the MRC, demanded that Gray prioritise neutron therapy. Gray perceived

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* An instrument that is normally used for measuring doses of radiation is an ionisation chamber where you measure the electric charge that is released when the air volume in the chamber is ionised. If the chamber is large, the ionisation of the air will be brought about by electrons that are released when the x rays or gamma radiation reacts with the molecules in the chamber air. If on the other hand the chamber volume is small, most of the electrons that pass it will originate from primary absorption processes in the chamber wall. The principle pointed out by Bragg and further developed by Gray gives a connection between the energy absorption in the chamber wall and the ionisation of the air in the chamber. By making the chamber wall similar to the material in which you are interested, e.g. body tissue, it is possible to calibrate the chamber so that the air ionisation gives a measurement of the energy absorption (the absorbed dose) in the material.

this as underhand behaviour because he considered the cyclotron to be unsuitable for the purpose. He wrote to the MRC and explained that he had lost confidence in his boss. The conflict led to Gray being dismissed, which was something that hit him pretty hard. Luckily he found support from many places and an anonymous donor enabled the establishment of a research laboratory which became the core of what would develop into the well-known Gray Laboratory.

Gray's colleagues within physics and biology held him in very high esteem. He was the Deputy Chair of ICRU from 1956–1962 and was the President of a major international Congress of Radiation Research in Harrogate 1962, a task which he was worried about and took extremely seriously. He was affected by a stroke in 1963 and never fully recovered before dying in his sleep in the summer of 1965.

Gray was a fascinating person. He was incredibly humble and always thought that other people's time was more important than his own. He was therefore deeply interested in what others were doing and, as do many people who do not overestimate themselves, had a natural sense of humour which often led to the infectious spreading of laughter. His colleagues thought he had fewer character flaws than anyone they had ever met.

The day before the conference in Stockholm, i.e., 15 September 1952, Sievert arranged a provisional meeting with ICRP members of the Institute of Radiophysics under the chairmanship of Sir Ernest Rock Carling. Binks was Secretary and the other people present were Jaeger, Mayneord, Sievert and Taylor. A decision was made to ask the Committees to submit their reports to the Secretary before 31 March 1953 and for both Committees and the Commission to meet in Copenhagen a week before Congress of Radiology in 1953.

Sievert wondered whether there might be time for ICRP to sever the ties with the Congresses of Radiology; after all, there were now radiation risks in many areas other than healthcare and medical treatment. One possibility would be to create an International Society for Radiation Protection. The group asked Sievert to come up with a proposal for the next meeting which was planned for 18 September.

Binks gave an account of how far the Committees had progressed. It transpired that only Failla's Committee I had actually engaged in any independent thinking; as for the rest, people wanted to base their reports on what the equivalent committees within the USA and the United Kingdom had already concluded. This meant that ICRP was strongly influenced by the USA, although this was no strange phenomenon bearing in mind the intensive activities there during the 1940s.

Sievert expressed doubt about the exposure limit of 0.3 röntgen for the weekly dose free in air recommended in 1950; he wanted 0.1 röntgen. However, the decision regarding any change was postponed until the meeting in Copenhagen the following year. In the end, the Secretary was asked to contact the Danish Congress Committee and request a proposal for an appropriate Dane to act as chair of this meeting.

In the evening, Sievert held a dinner for 50 or so people at the Rosenbad restaurant which no government offices had yet taken over. Of these, around 20 were outsiders whom Sievert had invited out of politeness (and maybe with future financing in mind), people such as the Chancellor of Stockholm University *Harald Cramér* (1893–1985), the 'consul general' *Axel Ax'son Johnson* (1876–1958), the Director General (of the Medical Board) *Arthur Engel* (1900–1996), head of *Atombolaget* Harry Brynielsson, head of Thule *Alvar Lindencrona* (1910–1981), Professor Manne Siegbahn, Chancellor of *Karolinska Institutet*, *Hilding Bergstrand* (1986–1967), bosses at *Radiumhemmet* Elis Berven and James Heyman, and Sievert's Norwegian colleague N. H. Moxnes.

On the following day, 16 September, the Stockholm Conference was opened in *Karolinska Sjukhuset's* assembly hall by University Chancellor *Arthur Thomson* (1891– 1977), who said that the subject area had been alien to him at first, but he then went on to say: 'I soon realised that the problems to be discussed at this conference are not just of interest to researchers within different branches of science – they're also of vital importance to all mankind.'

Thomson's opening address was followed by eight addresses by meeting participants. The most important were the first two. Hermann Muller proposed a limit for the 'genetic dose' for a population. Dr. *Katharine Williams* from Harwell reported the results of ongoing blood tests within the British Atomic Energy Research Establishment (AERE). Muller's proposal was to limit the average exposure of younger individuals in a population to 20 röntgen up to the age when they have children. He thought that this would limit the long-term increase of the mutation frequency to 25 %.

Those taking part in the conference arranged by Sievert and de Hevesy in Stockholm in 1952.
The photo was taken in front of the town hall. The back row directly in front of a column shows Harold Gray and Rolf Sievert standing side by side, the two 'unit men'.
The people who are in the front row are Elis Berven, Raymond Latarjet, Robert Jaeger, P. Bonet-Maury, George de Hevesy, Antoine Lacassagne, Boris Rajewsky, Sir Ernest Rock Carling, Hermann Muller and Walter Binks.
The rows in between show: Bertil Swedin, Robert Thoraeus, Cornelius Tobias, Frank Ellis, F.W. Spiers, Z. M. Bacq, Katherine Williams, Hermann Holthusen and Lauriston Taylor.
The back row shows: Karl Z. Morgan, Arne Forssberg (obstructed), Bo Lindell, Lars Melander, Matts Helde (obstructed), Carl-Gustaf Sundberg (partly obstructed), L. H. Gray, Rolf Sievert, Wybe Oosterkamp, W. V. Mayneord and Gösta Dahlberg

Katherine Williams referred to a recent article in *Radiology* by Robert Stone, who felt that the routine blood tests by personnel working with radiation were 'a terrific waste of money, time, manpower and effort'. Stone thought that the routine tests ought to be replaced with ones that were more research-orientated and executed on people whose doses of radiation could be reliably measured. Dr. Williams concluded her lecture by saying that that which was said by a person with Stone's experience was not easy to dismiss. Matts Helde, who had been deeply involved in the routine tests in Sweden, was disappointed. But from that day on, people all over the world began to realise the limited benefit of such examinations.

After the day's lectures, *Atombolaget* held a dinner at Solliden for around 40 people. The notabilities who had participated in the previous day's dinner at Rosenbad were now replaced by the participants' wives and the atmosphere was easier. It would be remiss of me to forget to mention an amusing incident at Solliden. When drinking coffee, I happened to sit next to Karl Morgan whereupon I then tried out an old trick. I wrote the numbers 1 2 3 4 on a slip of paper and asked Morgan to delete a number. He crossed out the 3 (as do most people if you do not make them suspicious). I asked him to turn over the slip of paper and read what I had written there: 'Why number 3?' Morgan was clearly sufficiently surprised for it to be obvious to the other person who was sitting next to him at the table, the small-of-stature yet greatminded geneticist Hermann Muller. Muller asked me to show him what was going on and I repeated the procedure with him. Muller also crossed out the 3 and could not understand how I could have predicted it. Morgan and Muller then spent a good while discussing my apparent clairvoyant capacity.

After this dinner, Sievert, in the best of moods, spontaneously hugged little Walter Binks, appreciatively thundering: ‘My dear friend William!’ We teased him about this little mistake for a long time.

On the following day, 17 September, the first thing to be discussed was the problem with radon in uranium mines. *Bonet-Maury* was after a limit value for inhaled radon and Boris Rajewsky spoke about the risks. The next thing to be discussed was the maximum permissible dose of radiation, which was still stated as a weekly dose. The discussion from the meeting in London in 1950 continued. However, it was agreed that the risk of injuries to the blood-forming organs was already insignificant at 0.3 röntgen per week. However, Sievert warned that the dose rate, i.e., the dose per time, could be of significance.

The limits that were discussed concerned people in radiation work, not the public. Hermann Muller’s concern for hereditary injuries would be justified even if the public received high doses of radiation. It was agreed that the limit 0.3 röntgen for the weekly dose would be too high for application to the public as well. Sievert reported that the natural radiation in Sweden gave annual doses of between 50 and 1 000 milliröntgen (between around 0.5 and 10 mSv). It was ascertained that, in spite of all x-ray examinations, natural radiation still gave the highest dose over a lifetime and that there was therefore no rush to state any limit for the genetic dose.

On 18 September, the six participants from ICRP met once again, this time in Uppsala. Having sought advice from Mayneord, Sievert produced a brief memorandum on an international radiation protection organisation. Its tasks would be:

a. to prepare international recommendations for radiation protection in all f ields in which radiation hazards occur;
b. to establish internationally agreed values for permissible doses and dosage rates;
c. to establish units for ionising radiations and to advise on methods of measuring such radiations;
d. to arrange conferences for discussion of scientific and practical problems regarding radiation protection;
e. to stimulate international co-operation;
f. to act as an information bureau, (e.g., to distribute information regarding legislation or recommendations adopted in various countries to safeguard radiation workers); and
g. to improve contacts between research workers dealing with practical protection problems.

Sievert also had ideas about the practical organisation and had imagined some form of academy based on his experience of work by the Academy of Sciences. His colleagues saw difficulties in financing the proposed activity which would require a permanent secretariat. Sievert believed he could obtain support from private sources. His colleagues thought that this could perhaps be arranged but had doubts as to the possibility of regular support continuing unless it came from major organisations such as WHO.

Sir Ernest wondered whether there might be conflicts between the organisation’s recommendations and the legislation in different countries. Sievert thought that the organisation ought not to get involved in legal matters but ought to adhere to the fundamental scientific principles.

Everyone thought that ICRP and ICRU were now so well established that they must form the core of the new organisation. Mayneord pointed out that the activity must also be broadened to include radiation protection not just within healthcare services but also within industry and maybe also health matters concerning the public. If ICRP did not take on the task of giving recommendations for industry as well, there was a risk of other less competent organisations within the area coming onto the scene. Binks pointed out that other organisations had already started discussing radiation protection instructions, among them the ILO. Even the television industry worked with rules for which they would rather have had advice from ICRP.

It was agreed that ICRP would continue to limit itself to ionising radiation and not look at problems concerning ultraviolet radiation.

The final agreement reached at the meeting was that ICRP would continue working in connection with the International Congresses of Radiology in the meantime but that it would endeavour not to extend its influence and also take up matters of radiation protection outside the healthcare services. The secretary was asked to write to the chairs of Committees III, IV and VI and request that they extend their activities to also cover radiation protection matters within industry Sievert was asked to summarise his organisation proposal in a more detailed memorandum to send out to the members.

After the meeting had ended the following day, Friday 19 September, Sievert had arranged a trip by boat across the archipelago to Vaxholm and to dine on the boat. At the dinner, Sievert told the old story that General Field Marshall *Helmuth von Moltke* (1800–1891) had only laughed twice in his life: first when his mother-in-law died and then when he saw Vaxholm's fortress. Lauriston Taylor asked him to correct this. Moltke had laughed a third time, i.e., when he heard that it *was* a fortress.

Throughout his visit to Stockholm, French biologist *Raymond Latarjet* had irritated Sievert by directly wrinkling his nose and rejecting the wine that was served at each meal and asking for mineral water instead. On the Vaxholm boat he initially reacted in the same way but then asked the waiter to come back and show the wine label. His sulky expression disappeared immediately and he shouted: 'Ah, an Aloxe-Corton from my old friend's vineyard! An Aloxe-Corton Latour! *S'il vous plaît*!' Sievert, who was no great wine connoisseur, was deeply influenced by Latarjet's delight and he adopted the habit of offering 'Al-Oxen' at mealtimes for many years afterwards.

At the end of 1952 in the USA, the seed was sown for the organisation that would be called the Health Physics Society. A radiophysicist by the name of *Saul Harris* wrote in the November issue of *Nucleonics* about the need to form an organisation for everyone working with radiation protection. He thought that the AEC (the Atomic Energy Commission) was responsible for a contact network for its employees and for those working on the contract for the AEC. However, thought Harris, if you ended up outside this group you became too isolated. In any case, it was possible the greatest radiation protection problems arose when using x-ray device and radium, sources of radiation for which the AEC had no responsibility. Harris wrote:

> … let us remember that the AEC has been entering the field of industrial safety on a new scale. Heretofore, the various state and local groups held themselves primarily responsible for problems of industrial hazards and public health hazards. This responsibility for the control of health hazards by the AEC extends only to reactor-produced materials and some accelerator products. The volume of X-ray equipment and naturally occurring radioactive material outside the responsibility of the AEC is probably presenting hazards to more persons than are currently being carefully supervised by the AEC. It is the equating of these types of exposure to radiation, AEC and non-AEC, that is strongly needed.

Harris' proposal already met with opposition from the editor of the journal, who said in an editor's note after Harris' text that what was needed was one single society for all nuclear issues rather than 'numerous professional groups which cover only portions of the nucleonics field'.

The year after that, Harris was criticised in the January edition of *Nucleonics* by the secretary of the American Industrial Hygiene Association, *Henry F. Smyth, Jr.* He wrote:

> I would like to point out that radiation protection is simply a new emphasized phase of the protection of industrial or occupational health. True, it has its own techniques for measuring hazard, but the medical and engineering phases of protection are not new. Neither is radiation hazard a new occupational exposure. It has been dealt with since application of the discoveries of Becquerel, the Curies, and Roentgen. […]
>
> The American Industrial Hygiene Association is one forum where all the specialists can meet on a common ground. Neither the physician, the engineer, the chemist, the

> nurse, nor the physicist predominates. At least fifty health physicists are members. More would be welcomed.
>
> We feel that it is appropriate for the Atomic Energy Commission to have its own health-physics association because of the classified information it deals with, but we see no need for those outside AEC to form their own association when they can have adequate voice in an existing association of people whose aim is the same as theirs and who are already using the tools they would specialize in.

The same edition of *Nucleonics* also contained a note by Dr. *Eugene Saenger*, representative of the isotope Laboratory at Cincinnati General Hospital. Saenger said that an organisation of the type sought by Harris had been formed in Cincinnati in April 1952. The organisation had 75 active members, 50 or so of whom were usually present at the meetings which were held each month.

Due to the interest that had been shown locally, Saenger thought that a national organisation such as the one proposed by Harris would be of value.

The 7th International Congress of Radiology was held in Copenhagen on 19–24 July 1953. Unlike previous occasions when ICRP had met on just one single day of each Congress of Radiology, they would now meet every day from 13–18 July, i.e., before the Congress. Because ICRU was expected to make a decision that would affect ICRP's work, it was agreed that ICRU meetings would be held on the first few days, like the meeting of ICRP's Committee I which, according to its Chair Professor Failla, would also discuss questions of general interest. Of ICRP's members, Failla, Sievert and Taylor were also members of ICRU. This meant that it was only possible to hold an informal meeting with the other members in the first few days.

ICRU made the important decision to introduce the new quantity *absorbed dose* and establish its unit the rad where 1 rad = 100 erg per gramme = 0.01 joule per kilogramme (i.e., 1 rad = 0.01 gray using the current name for the unit), thereby abandoning the former unit rep which had been introduced by Herbert Parker and which had been equal to 93 erg per gramme.

One problem arose in that the quantity, which consisted of the product between the 'dose' that was stated in rep and a weighting factor which stated the biological effectiveness of the type of radiation, had previously been stated using the unit rem. The equivalent quantity (which would eventually be called the dose equivalent and later the equivalent dose) now consisted of the product of the weighting factor and the absorbed dose and would also be stated in rem. However, since 1 rep = 0.93 rad, the old and the new unit 'rem' were different quantities. ICRP recommended that the old units be quickly abandoned in order to avoid confusion.

With the introduction of the absorbed dose, i.e., the energy that was taken up per unit mass in an irradiated body, we had a physically well-defined dose concept and avoided using the exposure measurement in röntgen as a dose measurement, the exposure that was inaccurately called 'quantity of radiation'.

In time for the meeting, the members had received the memorandum that Sievert had been asked to write at the meeting in Uppsala in September 1952 regarding his proposal for a new radiation protection organisation. He had given it the title 'Suggestions for an International Organisation for Radiation Protection'. The text is reproduced in its entirety in Lauriston Taylor's big book (Taylor, 1979). It is of interest to quote the first two paragraphs:

> Nuclear physics has placed new resources at the disposal of science, medicine and technology which require extensive precautionary measures if irremediable damage is to be avoided. It is the responsibility of our scientists to master the new forces they have set free. one of their objects is to provide protection against harmful radiation.
>
> In the utilization of atomic energy, and even during the preparatory steps to achieve this, ionizing radiations will be so common that there will arise protection problems on a scale never before conceivable and of importance not only for the individual but also for future generations. Radiation protection problems are therefore no longer the concern of individuals or nations alone, but are of widespread importance for the whole human race. Experience in radiogenetics, medical radiology and practical protection work, has shown the very complicated nature of these problems. Particularly strong grounds thus exist for international cooperation.

Sievert then referred to the valuable work which had been carried out by ICRP and ICRU, but then moved onto the shortcomings:

> The present international commissions do not cover all the fields of research necessary for dealing with the problems of radiation protection, are, in general, only active during or just before a radiological congress, and have not that form of organization which is necessary for efficient and progressive work. It is evident, therefore, from what has been said that a very great need exists for a body organized on a broader basis than that which exists at present.

Sievert proposed an organisational form of an academy of 60 members divided among four sections: radiophysics, radiochemistry, radiobiology and radiomedicine. He probably did not mean medical radiology with the latter-mentioned but radiation protection medicine instead.

On 15 July, ICRP's Main Commission met for the first time with all its new members. After ICRP's contacts with the Danish hosts, Bohr's colleague Professor of Physics *J. C. Jacobsen* had been appointed as Honorary Chair. However, Sir Ernest Rock Carling was still the active chair. The other members were:

Walter Binks, Secretary
André Cipriani
Gioacchini Failla, Chair of Committee I
Robert Jaeger, Chair of Committee III
W. V. Mayneord, Chair of Committees IV and V
Karl Z. Morgan, Chair of Committee II
Rolf Sievert
Robert Stone (succeeded Newell)
Lauriston Taylor
Maurice Tubiana (succeeded Chérigié)

Professor Mayneord's proposal to merge Committees IV and V into one new Committee IV and let Committee VI take over the designation V was followed. On 16 July, ICRP and ICRU met to discuss Sievert's proposal for a new organisation; Taylor was Chair of ICRU. They also discussed possible financing options. A group consisting of Rock Carling, Bugnard, Failla, Holthusen, Mayneord and Sievert was given the task of drawing up a short document that members could use in their respective countries when looking at the possibility of obtaining economic support for a new organisation. It subsequently turned out that, partly due to the time factor, the group had failed to reach agreement on any text. However, they did agree once more that ICRP would spend a few more years working hand in hand with the International Congresses of Radiology.

New byelaws were adopted for ICRP. The first two were worded as follows:

> 1(a) The International Commission on Radiological Protection (ICRP) shall be composed of a Chairman and not more than twelve other members. The selection of the members shall be made by ICRP from nominations submitted to it by the National Delegations to the International Congress of Radiology and by ICRP itself. The selections shall be subject to approval by the International Executive Committee (IEC) of the Congress. Members of ICRP shall be chosen on the basis of their recognized activity in the fields of medical radiology, radiation protection, physics, health physics, biology, genetics, biochemistry, and biophysics, with regard to an appropriate balance of expertise rather than to nationality.
>
> (b) The membership of ICRP shall be approved during each International Congress for service until the end of the succeeding Congress, or until new members are appointed. Not less than three but not more than five members shall be changed at anyone Congress. In the intervening period vacancies may be filled by ICRP.

The term ‘health physics’ was a concession to the Americans who had coined it during the Manhattan Project. The Congress of Radiology’s approval of the members elected by the Commission itself was a formality. ICRP became a self-perpetuating body, i.e., a group that continued to survive by virtue of its own power and on its own terms. This went on to arouse a great deal of criticism, but the Commission saw no better alternative. The Commission had to be small in order to be able to work efficiently. Its members had to be knowledgeable and competent so that the Commission’s recommendations could be accepted. Every election of members which was controlled by outsiders risked increasing the number of members and adding members who had been elected for some immaterial reason such as nationality or prestige, members who appeared to increase the Commission’s authority but who in reality reduced its competence. A safety valve lay in the fact that ICRP could exist only for as long as its recommendations were good enough to be accepted by the outside world.

The Commission met for the final time on 18 July. Sievert proposed that ICRP and ICRU merge into one single Commission in anticipation of a more efficient organisation. The final decision on this was postponed until a later date (which never arose). The Secretary, Walter Binks, was given the task of drawing up revised recommendations based on what had been decided at the meeting. The members for the next three-year period (those I have already listed) were confirmed.

One matter that was not discussed was the recommendations previously stated by ICRP regarding shorter working hours and extended holidays for people in radiation work. After the Congress, the Canadian Association of Radiologists wrote to Lauriston Taylor and asked why ICRP no longer spoke about the subject. Taylor’s response was that the question ought to be addressed to Walter Binks because Taylor was no longer Secretary of ICRP, but that he could give his own view of the matter (Taylor, 1979):

> […] the question of extra vacation for radiation workers has been the subject of numerous discussions over the past twenty years. It was initially felt that there was justification for extra time away from work. However, as we have come to know more about the problem of radiation protection it has become more and more evident that extra vacation or unusual working hours does not have any known beneficial effect, provided the basic installation in which the people work is in accordance with the general safety regulations. This has now been omitted from the International Recommendations without any discussion.
>
> And so ICRP entered into a new stage of activity, aware of the need to expand its activity in order to meet the increased requirements set for the Commission. The first thing needed for this was money. Money was needed for the large organisation that Sievert had in mind, but more substantial resources than the Commission had at its disposal were also needed for continued activity. After the meeting in Copenhagen, Sir Ernest wrote to Taylor and said that he had contacted British insurance companies and that Cipriani had taken the equivalent steps in Canada. He also mentioned the Ford Foundation as a possible source. However, these preparatory stages did not appear to lead anywhere. It was Rolf Sievert who would succeed in securing ICRP’s finances a few years later.

7. SWEDISH ATOMIC ENERGY, THE FIRST DECADE, 1945–1955

On 27 July 1945 – ten days before the atomic bomb was dropped over Hiroshima – the US Ambassador in Stockholm, *Hershel Johnson* (1894–1966), contacted Cabinet Secretary *Stig Sahlin* (1899– 1963) at the Foreign Office.* The case was so important that no-one had dared to give Johnson instructions by telegram which could be read by unauthorised people. Johnson had had to fly to London to get his instructions. His message to Sahlin was that the Americans and the British wanted an exclusive right to Swedish uranium production. In his report to the Foreign Office, Sahlin wrote that Johnson told of 'comprehensive experiments concerning the use of the metallic element uranium for military purposes' and added 'As far as I could understand [...] they were well on the way to producing "the atomic bomb"'. The Foreign Office, who knew very little about uranium and even less about its significance in terms of nuclear weapons production, contacted Manne Siegbahn. The following was written about the result of this contact in a report: 'He gave us all the technical details about the characteristics of the element uranium and, indeed, of its explosive nature'.

The coalition government that had existed during the war was succeeded on 31 July 1945 by a pure Social Democratic government under *Per Albin Hansson* (1885–1946). Johnson's message was already being discussed by a small group of the most closely-involved ministers on 2 August, but it was not until after the atomic bomb was dropped over Hiroshima on 6 August that people began to comprehend the significance of uranium and realise that Sweden had one of the largest uranium deposits in the world; low levels (200–300 grammes of uranium per tonne of shale) it was true to say, but the belief was that there were very substantial quantities in total. The government thought that Sweden was in a good position to have its own atomic energy programme, and many thought that

Sweden's neutrality called for the deterrent that nuclear weapons could constitute.

Immediately after the bombings in Japan, Supreme Commander *Helge Jung* (1886–1978) had given the FOA the task of obtaining information on the atomic bomb. At an FOA board meeting on 17 August 1945, the Supreme Commander's representative, Lieutenant Colonel Torsten Schmidt, suggested 'that the FOA might like to compile and submit to the Supreme Commander a report on what was currently known about the atomic bomb'. The FOA task was given to Torsten Magnusson, who had previously been Administrative Director (from 1941) and was then (1944) in charge of the Military Physics Institute (MFI) which Rolf Sievert had initiated. In April 1945, Magnusson had become head of department for the FOA 2 for physics at the newly-formed Defence Research Council which replaced the MFI and the Defence Chemical Establishment.

Magnusson, at that time having insufficient FOA expertise at his disposal, contacted university researchers who were given various assignments. *Lamek Hulthén* (1909–1995) in Lund gathered information on nuclear charge explosions, *Ivar Waller* (1898–1991) in Uppsala studied reactor structures, *Erik Hulthén* (1891–1972) in Stockholm looked at methods for enriching uranium-235 and Rolf Sievert examined the radiation protection problems. You must bear in mind that in 1945 there was barely one single nuclear measurement instrument available to buy; everything had to be built: GM counters, proportional counters, ion chambers, amplifiers, coincidence circuits, spectrometers, etc. Extensive and expensive work!

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* In order to gain an idea of context and a perspective on the development, this chapter starts with events before 1950, some of which have already been mentioned in 'The Sword of Damocles'.

Tage Erlander (1901–1985), who was then Minister of Education and Ecclesiastical Affairs, had received a letter from his friend from student times in Lund, Professor of Physics Torsten Gustafson, containing advice from Niels Bohr that the Swedish State ought to safeguard Sweden's uranium assets. Until that time, uranium had not been sufficiently valuable to warrant a mining concession, but belonged to the landowner. However, the Mining Act was now changed in favour of the State with regard to uranium.

On 11 September 1945, Foreign Minister *Östen Undén* (1886–1974) sent a letter in response to a proposal from the Allies to an arrangement between Sweden, the United Kingdom and the USA. The Swedish government promised to establish control of uranium production and of the export of uranium. On the other hand, it refused to give the United Kingdom and the USA a concession regarding the uranium assets and to accept any veto power on their part when it came to the export of uranium. The letter was returned by Ambassador Johnson with the advice that a formulation could 'hurt the American President's feelings'. A revised response letter was sent on 15 September. Since the American initiative through Ambassador Johnson was based on concern regarding the possibility of the Soviet Union gaining access to Swedish uranium, both of the great powers left it at that; they thought Undén's response gave adequate guarantees.

On 23 November, the committee that was named The Atomic Committee was set up to find methods for 'the assimilation of atomic power'. The Atomic Committee included Torsten Gustafson, Manne Siegbahn, Hannes Alfvén and ASEA's former Head of Engineering *Ragnar Liljeblad* (1885–1967). *The Svedberg* (1884–1971) would come later on. The Chair of the Committee was County Governor Malte Jacobsson. The Atomic Committee was initially meant to be an investigative and advisory committee, but it was soon given administrative tasks concerning the organisation of research and had access to research funds. At the time, people used the term 'atomic power' rather than the more correct 'nuclear power'.

The Atomic Committee had no formal responsibility for research into nuclear weapons but half of its members were also on the FOA's board which was chaired by the Director General of the Royal Telegraph Administration *Håkan Sterky* (1900–1992). This personal link to the FOA meant that it was natural to give some research assignments to the FOA, ones which were also in the FOA's interests such as research into uranium production and reactor construction. However, The Atomic Committee had sufficient power to avoid being controlled by the FOA; the Committee refused to accept demands from the Supreme Commander of the Armed Forces regarding the secrecy of many of the operations, for example. In the doctoral thesis called *Hela nationens tacksamhet* [The Entire Nation's Gratitude] (1991), Stefan Lindström writes that 'during this period, there was a limited group of leading scientists who succeeded in obtaining a considerable influence over both the civilian and the military research and […] used this influence to strengthen their own activities'.

Kerstin Lundqvist, who initially worked as a local government clerk and later as a registrar for The Atomic Committee, has described the way in which the ongoing work for both the Committee and the Natural Science Research Council (NFR), which was established in 1946, was taken care of by lecturer *Gösta W. Funke* (1906–1991). She recounts (Lund 1992):

> The person who pulled all the strings so that the council would be able to function in administrative terms was Ph.D. Gösta Funke, who had been appointed Secretary. He was previously a senior master in physics at Bromma Secondary Grammar School but had already partly left his teaching work the year before. When The Atomic Committee was established in 1945 at the initiative of the former Minister of Education and Ecclesiastical Affairs Tage Erlander with County Governor Malte Jacobsson as Chair, Funke had been appointed Secretary of the Committee. From now on, he would devote the whole of his remaining working life, almost three decades, to research.
>
> Funke was exceptional at organising. He was just about the only person to build up the administrative foundations for both of the Committees [i.e., the NFR and The Atomic Committee]. He did of course consult the newly-formed technical and medical research councils. […]
>
> Initially, Funke took charge of all the work at his private residence in an apartment at Besmansvägen 10 in Åkeslund in Bromma. It was not long before an assistant clerk

> was employed to do registration and archive work. A work room was rented for her in a nearby apartment. The most necessary office furniture, a calculator and a typewriter and mimeograph were purchased. The first assistant had been brought in. […]
>
> In September 1947, the NFR's first assistant clerk was replaced by Karin Linde who came from the Royal Telegraph Administration board where we were colleagues. In 1948, both the Secretariat and the administration had moved to Nockeby. Funke bought a villa on Thaliavägen 53. He now had the option of fitting out a larger work room with archives for all documents. The administration moved to another villa 10 minutes' walk from Funke's villa, i.e., to Orsavägen 9 where the Andersson family rented out their 25–30 m^2 dining room for the administration of the Committees. […]
>
> Karin recommended that I apply for the job of Assistant Clerk for The Atomic Committee. She was employed by the NFR. The pay was slightly higher than it was for the Royal Telegraph Administration board. It was only a temporary job, but the jobs on the Committees might well become permanent jobs. […] However, in the eyes of many people, The Atomic Committee was not the best bet. My boss on the board of the Royal Telegraph Administration at the time became so concerned when he found out what my plans were that he contacted his boss, Director General Håkan Sterky, who was a member of The Atomic Committee. He wanted to know whether 'this somewhat diffuse committee' really was something that was properly established. But Sterky was able to give a reassuring answer.
>
> The transfer to my new job bordered on the traumatic. In my previous job, where I had been secretary to a departmental manager who was incredibly friendly and considerate, the pace of work and work assignments were not that demanding. My new boss on the other hand, Dr. Funke, was both imposing and exacting. But he did give Karin and me substantial responsibility and we were able to look after ourselves at the office. We were two young girls aged 23 and 21 who were to do EVERYTHING. There was no buyer, watchman or cleaner [the financial matters were taken care of by an accountant at The Swedish National Audit Office as a spare-time job]. There were daily walks to and from Funke's home to collect post, which was always opened by him, for registration. On the following day, the documents had to go back to be entered into Funke's own card index and then be put into folders. Hand-written concepts for letters, minutes, agendas, contracts, etc. were collected every day. After typing them out using carbon paper for copying purposes, the documents were taken for signature the following day. We had to take stencil copies of all documents that were to be sent out to members. After collation, it was time to use the ink-clad mimeograph. Sufficient numbers of application documents were requested, i.e., nine copies, but there were still an incredible number to be copied. Every afternoon on the way home we handed in our bundles of outgoing post. We first had to lick and stamp all registered letters - The Atomic Committee had many classified letters - with a seal.

In 1945, Torsten Magnusson took on physicist *Sigvard Eklund* at the FOA, and in 1946 he became head of a special nuclear physics section. One of his early colleagues was Rolf Björnerstedt who had not yet finished his Bachelor of Science at the time of being employed. Eklund's first major assignment was to construct a pressure-isolated van de Graaff accelerator to be built on the FOA's premises on Drottning Kristinas väg near KTH in Stockholm. A simpler version was erected in a high voltage hall next to Professor *Gudmund Borelius'* (1889–1985) Physics Department at KTH. Eklund has written the following about his time at the FOA (Fröm, 1995):

> My work at the FOA concerned fundamental nuclear physics and nuclear technology to a much greater extent than the impact, protection and military issues. At the start of the 1950s I remember participating in a discussion on the role of Swedish nuclear weapons in the event of a war. When I asked what would be done if the adversaries also used nuclear weapons, the answer was that we would then have to increase our use of weapons, a conclusion that was enough to scare anyone in my eyes.

In March 1946, The Atomic Committee released its first report, written by the Secretary who was Dr Funke. The typewritten report covered 60 pages of loose-leaf format and was not published in any other way, possibly due to misgivings within the Defence Staff. In a secret memorandum of 27 March 1946, Commander in Chief of the Swedish Defence Staff *Carl August Ehrensvärd* (1892–1974) and Torsten Schmidt warned of the risk of outsiders obtaining too much information on the Swedish capacity to produce nuclear weapons and on persons involved in the work. However, the bill that was based on The Atomic Committee's proposal did contain a fairly in-depth examination of the report's content and proposals.

In April 1946, Supreme Commander Helge Jung stated the following in a letter to the King (quoted from Fröm, 1995):

> It is not currently possible to predict the way in which any use of atomic energy as an energy source, etc. may affect our defence. On the other hand, the special significance of atomic power in the production of weapons is already a reality which may lead to far-reaching consequences. Even if the use of such weapons were to be forbidden through international agreements or it were said that they ought not to be introduced into our Defence organisation for some other reason, we would need to seek to gain the maximum possible knowledge regarding the conditions for the production, potential use, effect, etc. thereof, particularly with regard to necessary protection measures and countermeasures.

In the same year, The Atomic Committee asked the FOA to examine the possibilities of purifying Swedish uranium and plutonium production. It was not thought possible to obtain uranium from external sources because the USA enforced a strict export control on fissile material. Swedish uranium exists mainly in large quantities but low concentrations in the shales in Närke, Billingen-Falbygden and Östergötland, which have a uranium content of 0.02–0.03 %. The shales also contain the carbonaceous rock called kolm which can contain 0.3 % and sometimes up to 0.5 % uranium. The kolm was therefore where the initial interest lay. The FOA's chemical department was given the task of researching methods for the purification of uranium and contact was therefore made with scientists at the Swedish Shale Oil Company and at KTH.

Late in the evening of 6 October 1946, Per Albin Hansson unexpectedly passed away on the way home. He was succeeded by Tage Erlander, who was already familiar with the matters concerning atomic energy. It was now evident that The Atomic Committee was not an appropriate organisation to handle the expansion of the facilities that were needed within an atomic energy programme. The formation of a new company for this purpose, AB Atomenergi, with the State as the principal owner, was discussed at a meeting with ministers in January 1947. On 26 April 1947, The Atomic Committee issued a second sub-report, now containing proposals regarding the company. The industry had been consulted and the industry preferred the State to be responsible for the initial, expensive development at this early stage. The State subscribed to 57 % of the equity (2 out of a total of 3.5 million Swedish kronor). The inaugural meeting for the new company was held on 8 November 1947.

The responsibility was now shared within the government. While the Department of Education and Ecclesiastical Affairs with Erlander as Minister had taken the initiative, it was now the Department of Commerce under *Axel Gjöres* (1889–1979) which proposed the bill regarding *AB Atomenergi*, popularly referred to as 'Atombolaget' (the Atomic Company).

The company was asked '*to research and extract necessary basic materials for atomic energy use, to build experimental piles* * *for use in atomic energy, to subsequently build piles for the use of atomic energy in research and trade and industry on a larger scale, and to continue research in connection with said operations as well as industrial and commercial activity*'.

24 companies from trade and industry were part-owners of Atombolaget and they agreed to exchange experiences and research results. There now appeared to be two parallel developments: a civilian

* The first nuclear reactors were called 'piles' (sometimes 'atomic piles'), in Swedish: *staplar* (piles, stacks); Fermi's reactor had after all contained piled-up blocks of uranium and graphite. When the idea of establishing *Atombolaget* was first discussed, it had been referred to as a '*stapelbolag*' (pile company).

development connected with trade and industry and a military development. However, there was still a strong personal union between The Atomic Committee and the boards of the FOA and Atombolaget. Håkan Sterky was on the company's board from 1947–1969 and Hannes Alfvén from 1956–1968. *Atombolaget's* first Managing Director was the former MD of AB Nitroglycerin, *Sigurd Nauckhoff* (1879–1954) who was succeeded by Harry Brynielsson, Master of Engineering, in 1951. Chemist *Erik Svenke* (1918–2014) was already employed by Atombolaget in 1947 as one of its first scientists. He developed methods for leaching uranium from kolm in Nitro Nobel's old factory in Vinterviken in southern Stockholm.

It should come as little surprise that at this time, both the State leaders and the military were intent on supplying Sweden with nuclear weapons ('atomic bombs' as the inappropriate name was to begin with; I am using the terminology that was typical for this period). The atomic bomb was an effective weapon. From the point of view of the military, the destruction it could cause was not unique – the conventional bombings of Dresden, Hamburg and Tokyo had been equally destructive. The unique thing was that this destruction could be caused by one single aircraft or one single rocket. According to his own information, Tage Erlander also held a positive view of the atomic bomb in the 1940s and for a short while into the 1950s. It was agreed that being neutral required the potential for a great force of impact.

And yet the bomb was not what was uppermost in the mind of the Swedish government. Thomas Jonter (Jonter, 1999) writes:

> The civilian use was what initially attracted Prime Minister Tage Erlander and other leading politicians in Sweden. The years of preparedness with energy rationing were behind them. The supply of oil had been strictly limited and various reports also stated that the world's oil supplies would probably run out within a few decades. On this basis, nuclear power was seen by many of the political elite as the dominant type of energy in the future. In the same way that oil had taken over from coal, the dream was now to let nuclear power take over from the uncertain oil supplies, which led to the vision of a Sweden that was self-sufficient in terms of energy.

This is probably correct, but it does not mean that the politicians distanced themselves from the possibility of nuclear weapons. On the contrary, substantial investment was put into reactor structures in which Swedish industry showed less and less interest as time went by but which would facilitate the production of nuclear weapons. As late as 1959, *Per Edvin Sköld* (1891–1972), the most influential Social Democrat politician after Erlander, said he was in favour of Swedish nuclear weapons.

In 1949, the FOA and Atombolaget entered into a secret cooperation agreement which was approved by the government in the following year. In this connection, Atombolaget took over the development of reactors and in summer 1950, part of the nuclear physics section and some of the equipment were transferred from the FOA to Atombolaget. The research into uranium extraction had already been transferred there. And so Sigvard Eklund left the FOA to become head of research at Atombolaget. It was implied that Atombolaget's results could also benefit military research, but Eklund has rejected the statements that Atombolaget worked directly for the FOA (Fröm, 1995):

> Bearing in mind the aberrations which still seem to be circulating in the Swedish mass media, I would like to emphasise that AE's task was to promote the development and use of nuclear power for energy purposes. It is extremely misleading to maintain that secret and silent agreements meant that the ring in AE's nose was pulled by either the FOA or the military.

The State still had full control of the development, something which worried part of trade and industry – although not ASEA (the General Swedish Electric Company), where Ragnar Liljeblad pointed out that the activity was still only costing them money and would not earn income for quite some time.

Sigvard Eklund's research activities illustrate the close link that would arise between civilian and military research purposes during the 1950s. Eklund researched the conditions for the production of Swedish atomic bombs on behalf of the FOA, and under the auspices of Atombolaget he led the construction of the first Swedish research reactor, called R1, which ended up being located in a rock shelter next to the IVA's test station on Drottning Kristinas väg near KTH in Stockholm.

Sigvard Eklund had already drawn up a 'memorandum regarding the type, size and location of a first reactor planned by AE' in 1949. There were several reasons for choosing heavy water rather than graphite as a moderator: it was thought to be difficult to produce sufficiently pure graphite, a graphite reactor would require considerably more uranium, and uranium was still in short supply. Eklund also pointed out that a graphite-moderated reactor could not be run at as high a temperature as a heavy water-moderated one and that it was therefore less suitable for a programme with reactors for the production of electricity in mind. Eklund had suggested that it be located in a rock shelter next to the IVA's test station because he and the head of radiation protection at Harwell, Greg Marley, had also looked at the possibility of homing the reactor at Gåshaga on Lidingö.

The reactor was built 25 metres down in the rock and would eventually be run with an output of 1 megawatt (MW). The primary coolant was the heavy water which functioned as a moderator, but this in turn was cooled by air which was blown through a heat exchanger and out through a tall chimney. One prerequisite for the success of the construction was the cooperation that had been established between Atombolaget and the French *Commissariat à l'Énergie Atomique* (CEA) in Paris. As early as 1950, physicist *Eric Hellstrand*, chemist *Eric Haeffner* and metallurgist *Hilding Mogard* had had the opportunity to visit the CEA and study fuel production methods. One particular factory for the production of aluminium-encapsulated metallic uranium fuel elements was set up on Lövholmsvägen (at Liljeholmen) in southern Stockholm, utilising experiences from the French heavy water reactor ZOE and the Canadian NRX.[*]

It is interesting to read what Karl-Erik Larsson writes about the arrival of the reactor (quoted from Larsson, 1987):

> It is important to note the spirit and the organisation which were involved in the construction era of R1, which the reactor in Stockholm ended up being called. The spirit was unique in the latter half of the 1940s and throughout the 1950s. Everyone was inspired by the will to achieve success: civil engineers and workshop mechanics cooperated in trust, with enthusiasm and with simple means. Sometimes no proper drawings were needed to develop a product. A sketch on a sheet of A4 was enough. Calculations and measurements, drawings and productions took place using youthful imagination. Around 1950, the average age of the employees was below the age 30.
>
> […]
>
> It is interesting to note that all those who were responsible for radiation experiments had to acquaint themselves with radiation protection physics and dosimetry. The activity was certainly monitored from the radiation protection point of view by the Institute of Radiophysics, but all experimenters became their own protection engineers to use modern nomenclature.
>
> The licence to run R1 was issued by the Medical Board based on a recommendation by the Institute of Radiophysics. And so the concession procedure was simple, but Sievert's institution still guaranteed that nothing unreasonable could occur. Nor was anyone injured by radiation throughout the first 10-year pioneering era of 1945–1955 when the risks ought to have been at their greatest (and nor did the work cause any cases of radiation injury later on).

The reactor would use natural uranium with heavy water from Rjukan as a moderator and was commissioned at 18.59 on 13 July 1954. This was what opened Sweden's door to the atomic age.

The heavy water delivery from Norway, five tonnes, was surrounded by great secrecy and the heavy water was initially stored under strict surveillance in one of the rock shelters by Henriksdal's treatment works in south-east Stockholm.

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[*] See 'The Sword of Damocles' for these reactors.

In 1950, Atombolaget's board had decided to erect a uranium extraction plant in Kvarntorp in Närke. In 1953, this plant was ready to produce 5 tonnes each year, although this would prove to be insufficient. They were therefore forced to borrow three tonnes of uranium from France for the R1 reactor, which Harry Brynielsson, equipped with diplomatic passport, moved by train to Stockholm.

The same rock shelter that contained the reactor also contained a plant for criticality experiments called ZEBRA (Zero Energy Bare Reactor Arrangement), a 'zero energy reactor'. R1 and ZEBRA were actually training objects and the facility on Drottning Kristinas väg was too small to be of any significance other than to provide experience and train personnel. The next stage needed a larger plant and also laboratories for the reprocessing of irradiated fuel and 'hot labs' for handling heavily radioactive material. Such a plant with a considerably larger reactor was not suitable to be located in densely-populated areas; a separate research station was needed. Such plans were mentioned in Atombolaget's estimates in August 1952.

In Sörmland there were suitable areas close to water, and in 1955, Atombolaget was able to purchase suitable land which had been parcelled out from the Baroness *von Plomgren's* Hånö estate just less than a mile south-west of Trosa. The area, like the plant that was built there, was named Studsvik. The plant is in a beautiful location next to the round bay of Tvären, which is thought to be of volcanic origin and has a maximum depth of 80 metres (the average depth of the Baltic Sea is 60 metres). This is where the experimental station would be erected, which would correspond to Harwell in England, Brookhaven and Idaho Falls in the USA and Risö in Denmark.

Eklund's report for the FOA showed that a Swedish atomic bomb programme would not be economically unviable – 5 bombs per year could be produced at an affordable cost. Both The Atomic Committee and Atombolaget were negative towards such a separate programme, however, because they feared that it would prevent the normal development of the peaceful programme.

However, the FOA continued to work towards the goal of nuclear weapons. If Atombolaget succeeded in building reactors, the FOA's primary interest would be to extract plutonium from the irradiated reactor fuel, i.e., to find a reprocessing method. The young chemist Jan Rydberg was employed for this purpose in 1947 and worked out a method for reprocessing the fuel using liquid extraction, a recreation of a method proposed by Glenn Seaborg which was still being kept secret at the time. It was not used until 1954 according to what Rydberg has described (Fröm, 1995):

> 1954 was when we produced the first quantities of plutonium. It just so happened that I had a conversation with Gunnar Randers* in Norway, who was head of the Norwegian nuclear research programme in Kjeller, and who promised us irradiated uranium pellets from the Norwegian reactor JEEP. Once we had them and they had cooled down, we sawed them up into a remote-controlled box and got the uranium metal out. It was *Birgit Olausson* who isolated the first microquantities of plutonium; it presented as small, fluffy precipitation right at the bottom of a test tube. It was very alpha-active and had all the characteristics to indicate that it was plutonium.

Plutonium research obviously also went on at Atombolaget. It was led by Eric Haeffner and was initially done in a small analysis laboratory at number 47 Drottning Kristinas väg. In the mid-1950s it was also possible to extract plutonium from uranium in this research programme, which in this case came from France. The cooperation between Haeffner and Rydberg was not the best. Rydberg recalls this (Fröm, 1995):

> The FOA and AE had an agreement whereby we would keep each another informed. But things became one-sided for us nuclear chemists; we at the FOA told the AE what we were doing but received very little information in return. You might say that the cooperation was such that we greeted one another with a friendly hello when we met,

* Gunnar Randers' background has been described in more detail in 'The Sword of Damocles'.

usually at major conferences, and that was all. It may have worked better between the physicists - I don't know.

The FOA's experiments were moved to Ursvik in Sundbyberg west of Stockholm. Rydberg tells us (Fröm, 1995):

> We built a small remote-controlled facility up in the forest in Ursvik (currently Bldg. 79). One wall of the building was made entirely of glass and we had flood lighting so that you could see everything inside the building from outside. 15 metres from the glass wall we had a 'cottage', a small hut, and there were long rods between this and glass building. So, we sat in the hut and turned the rods and watched the measurement instruments through binoculars. The vessels, which stood on the shelf, contained hexon and nitric acid, some of them mixed; if you put your hand on them you could feel that they were becoming slightly warmer day by day, and also darker. [There was a risk of explosion which had to be carefully controlled.]

In 1956, the FOA did a cost estimate for a larger plant in a rock shelter which came out at 56 million Swedish kronor, but no money was granted. Questions were now starting to be asked about the investment in Swedish nuclear weapons.

There was nothing strange about the fact that both the FOA and Atombolaget were doing plutonium studies. In a civilian nuclear power programme at that stage, the interest was in being able to extract the plutonium that was gradually formed in the fuel elements by running a reactor. It could in principle be possible to use the plutonium as new fuel and then preferably in oxide form. In the military nuclear weapons programme on the other hand, the core of the bombs would consist of metallic plutonium-239. However, in a reactor, more plutonium isotopes are formed, including plutonium-240, in different quantities depending on the method of operation of the reactor. In reactor plutonium, 40 % can consist of isotopes other than plutonium-239, and the level of plutonium-240 can be in excess of 20 %.

However, plutonium-240 emits neutrons from spontaneous fission. These neutrons could mean that the chain reaction in the bomb would be started too early so that only some of the plutonium would be split. The presence of a great deal of plutonium-240 would therefore make the power of the bomb unpredictable. From the military point of view, it was therefore desirable to have as pure plutonium-239 as possible. Weapon-grade plutonium (plutonium of a quality suitable for weapons) should have a purity of 90–95 % plutonium-239. When first starting to operate a reactor, weapon-quality plutonium is formed but because the operation continues, the share of plutonium-240 increases, and especially quickly if there is a high neutron flux. A large power-producing reactor in a civilian nuclear power programme is therefore not particularly suitable for producing weapon-quality plutonium. A State that wants to procure nuclear weapons therefore has a greater possibility of producing weapon-grade plutonium in a small, specially-built production reactor although it would not be impossible to insert special, easily-removable fuel elements into the outer edge of the core of a larger reactor. If a 'civilian' nuclear power reactor is built so that you can replace fuel elements during operation, there is reason to suspect military ambitions.

The extent to which this detailed knowledge of different plutonium isotopes existed among leading politicians and military personnel is unclear. However, what certainly was known was that it was possible to make atomic bombs from plutonium and that plutonium was formed in nuclear reactors.

The fact that the FOA scientists had the opportunity of travelling abroad was very important to them. Jan Rydberg writes about this (Fröm, 1995):

> However, our trips abroad were the most important; we obtained a great deal of information through these. Both my head of FOA 1, Gustaf Ljunggren[*], and my subsequent head of FOA 4, Torsten Magnusson, were incredibly generous in allowing us to travel and do basic research. These two things are very closely related: research

[*] *Gustaf Ljunggren* (1894–1966), chemist and head of department at FOA 1, former head of the National Defence's Chemical Establishment.

> and travel! By virtue of our basic research, publishing and the lectures we held abroad, we were very well received on our trips and had close contact with the premier foreign scientists, and this is also when we often obtained other valuable information. I was rendered speechless with amazement the other day when Anders Fröman* asked which consultants we had. We had no consultants - we needed no consultants. What we were doing was good enough for the scientists who had worked in the Manhattan Project and then moved out to universities to be eager enough to tell us 'Europeans' what they had been up to. So, one way or another we did actually have 100s of consultants, all unpaid and none of them formal.

When President Truman's successor Dwight Eisenhower surprisingly offered other countries technical information and access to uranium fuel in his speech at the UN's general assembly in 1953, the conditions for Swedish trade and industry's interest in 'atomic power' changed drastically. It was now all starting to open up as never before so the private power industry could start making plans for nuclear power plants.

However, in the middle of this encouraging development, the need for the military use did make itself felt. On 1 March 1954 (local time), the big hydrogen bomb called Bravo which was boosted with uranium was detonated on Bikini and which, contrary to all plans, contaminated the Marshall Islands of Rongelap and Rongerik and the Japanese fishing boat the 'Lucky Dragon' with radioactive fission products. The event attracted a great deal of attention in the newspapers and caused an interpellation in the Swedish Parliament. Tage Erlander responded to the interpellation on Wednesday 18 May and said the following about the effects of the atomic bombs:†

> Unnecessary hush-hush and exaggerated false pretences to airbrush the picture of the risks would constitute very poor preparation. It could lull us into a false sense of security or we could fall victim to unfounded rumours. A democracy lives through the knowledge, insight and trust from its individual citizens and through their possibility of being able to openly discuss important questions on the basis of freely-available facts. My intention is therefore to give as clear as possible an account of the situation that our country currently finds itself in as regards the atomic field, and to highlight the measures that have been taken and which are planned should a nuclear war affect us.

Some of these measures were prepared by the FOA (Defence Research Council), and this is what Erlander said in that connection:

> The measures will depend on what we know from continuous research about the essence of the nuclear weapons, their effects, the structure of the different weapons, the likely energy content thereof, medical protection measures, etc. […] The FOA is doing multi-disciplinary research to ascertain the development of the nuclear weapons and protection on the other hand […]

The question of a Swedish 'nuclear weapon' was not taken up in the debate, which concerned mainly the consequences of a war involving nuclear weapons. The former Foreign Minister Rickard Sandler did make the following cryptic statement, however:

> […] spare us the thought that people in this country are playing with the idea of Swedish offensive atomic bombs.

The content of the statement has been discussed. Did Sandler completely distance himself from Swedish atomic bombs? If so, why did he speak of 'offensive' weapons? Was it simply the big strategic

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* *Anders Fröman*, Assistant Professor at the FOA, project manager of the meeting at which Jan Rydberg held an address.

† Many of the quotes in this chapter have been taken from Stefan Lindström's doctoral thesis (Lindström, 1991) although the primary author is stated. This secondary source is not mentioned in each individual case. so as not to make the text unnecessarily cumbersome to read.

hydrogen bombs used for terror bombing that he wanted to get away from? Did he accept the small, tactical nuclear weapons of the size used on Hiroshima?

However, it was not just the powerful explosion on Bikini that started a nuclear weapons debate in the mid-1950s. The Supreme Commander's defence report (SC 54) recommended that the Swedish Defence be equipped with nuclear weapons to be able to defend Sweden's neutrality.

In November 1954, *Howard Robinson* from the American Embassy in Paris visited Torsten Magnusson at the FOA; the two were well acquainted from before. Magnusson referred to SC 54, which said that, in the event of a Soviet attack, Sweden must have access to tactical nuclear weapons within a few hours to prevent an invasion. It would be a while before Sweden would be able to produce atomic bombs. Would it be possible to buy some nuclear weapons from the western powers?

Later that same month, the USA's *chargé d'affaires* reported home to the State Department that the SC 54 had recommended nuclear weapons. Thomas Jonter writes (Jonter, 1999):

> It was said that Swedish authorities had been extremely frank in off the record talks with the Embassy's personnel and with national and international press regarding the plans to procure nuclear weapons from the western powers. For example, Cabinet Secretary Arne Lundberg* had told Joe From of U.S. News and World Report that they were aware that current American legislation did not permit the sale of nuclear weapons to Sweden. Lundberg added that perhaps these laws and regulations could be changed in the future.

On 25 January 1955, *Georg Branting* (1887–1961) and *Oskar Åkerström* proposed in their identical bills in the First and Second chambers of the Swedish Parliament that a Commission ought to be established to investigate and provide information on the effects of the nuclear weapons.

In February 1955, Stockholm's branch of the Social Democratic Party held a meeting in *Konserthuset* on the subject of 'Atomic power – destruction or prosperity'. Erlander, *Torsten Nilsson* (1905–1997), Sandler and The Svedberg gave addresses there. However, the latter, whose presence emphasised the scientific connection of the subject, spoke also of energy policies.† Or, as Stefan Lindström has put it (Lindström, 1991):

> Natural Science was an obvious gateway to the atomic energy issues and Svedberg outlined the scientific background to the breakthrough of atomic energy and stressed the importance of fundamental research. According to Svedberg, the material culture rested principally on technical applications of the results of fundamental research, and atomic power was described as a prime example of such an application.
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> Svedberg did not limit himself strictly to his own area of competence in his capacity of Professor of Physical Chemistry, but also made energy policy statements. He predicted a very strong increase in the need for energy and, owing to this, a future energy shortfall with atomic energy being the only way out. So, the picture of the future was one of praise and blame. An almost systematic development – the increase in the need for energy – would infallibly lead to a fundamental crisis for the progress and persistence of Swedish society, not to mention that of other States. However, it would be possible to face the crisis with the help of atomic energy.

Svedberg said that the radiation from a reactor certainly made it impossible to drive cars using atomic power, but that heavier means of transport such as ships, trains and aircraft could perhaps be run on atomic power. He confirmed that even if the plutonium formed in the reactor of a nuclear power plant were of no use to the military, the reactor could be 'adapted to produce mainly bomb material'. Svedberg

* Envoy *Arne S. Lundberg* (1911–2008) was Cabinet Secretary of the Ministry of Foreign Affairs from 1951–1956.

† When I wrote 'Pandora's Box', I was not aware that in 1908, in cooperation with *Daniel Strömholm* (1871–1961), The Svedberg had already practically speaking discovered that the elements occur in the form of different isotopes. Professor Jan Rydberg has drawn my attention to Svedberg's book, *Arbetets dekadens* ('The Decadence of Work') from 1915 in which this is discussed.

thought it was necessary for people to learn how to handle the technology responsibly. He said that 'The nature and causes of our emotions must be clarified to enable us to master them more easily'.

Sandler also expressed concern about the development but said that he sooner believed that the effect of the peaceful use of atomic power would lead to good results rather than the formation of a disarmament convention, however welcome it may be. He said:

> We must hope for enough of a reprieve for common sense to tame the demon of destruction that inhabits the atomic nucleus and transform it into a powerful, submissive servant of mankind.

Both ASEA and Vattenfall now started to see the development in a different light, while Tage Erlander started to doubt whether it was reasonable for Sweden to have nuclear weapons. Four of the power industry's top men met on 7 March 1955, i.e., the MD of ASEA *Åke Vrethem* (1912–1984), the head of the ASEA's nuclear power department in Västerås, *Uno Lamm* (1904–1989), the Director General of Vattenfall *Åke Rusck* (1912– 1978) and the Deputy Director General thereof, *Bo Rathsman* (1915–1970). These powerful men dined together following a cocktail party that Vattenfall had arranged for a Soviet study delegation. It was ASEA's Vrethem who had asked about the meeting since he had heard that Vattenfall had started to cooperate with Atombolaget.

Rusck said that Vattenfall and Atombolaget had agreed a work distribution where Atombolaget would be responsible for research and development work while Vattenfall would build and run nuclear power plants using Atombolaget as a consultant. ASEA's representatives pointed out that ASEA ought to be able to participate as a party with equal rights. A preliminary agreement was reached which, for ASEA's part, was formulated by Vrethem:

> We also appeared to have agreed that the aim of the development work would be to obtain a basis for Vattenfall's board to order and for us [ASEA] to construct and deliver a real nuclear power reactor of at least 100 000 kW to be commissioned as soon as possible and in any case within ten years.

The conviction that unanimity had been reached proved to be too optimistic, however. The main disagreement was between Atombolaget and ASEA and concerned Atombolaget's reactor construction inputs. ASEA's former technical boss Ragnar Liljeblad who, following his retirement, was still active as a consultant technical director and, owing to his powerful nature, had substantial influence, summarised ASEA's criticism:

> The idea of AB Atomenergi acting as an architectural and construction bureau for a nuclear power station and ordering the sections from different workshops is also unrealistic in my opinion. You do not order a reactor from a sheet metal worker in the same way as you can order a conventional boiler from him according to submitted drawings.

Lindström describes the situation as follows (Lindström, 1991):

> That which Atomenergi, Vattenfall and ASEA were relatively easily able to agree on was the choice of heavy water technology and that both thermal and nuclear power reactors should be developed. But, as already shown, unanimity regarding the two projects appeared to be a partial illusion. Unlike Vattenfall, Atomenergi saw a thermal plant solely as a stage of development towards the nuclear power plant.
>
> Essentially, the conflicts of opinion ended up concerning the distribution of the work, and then primarily who would have the option of developing resources to construct reactors. The main objectors were Atomenergi and ASEA since the capacity to construct reactors was a condition for both to have the possibility of making a name for themselves in the atomic energy field. Without this competence, Atomenergi would, broadly speaking, become a control or research institute and ASEA – in the atomic energy field – an engineering company which manufactured according to drawings, a 'sheet metal worker'.

The nuclear weapons issue was discussed in the foreign debate in the First Chamber of the Swedish Parliament on 9 March 1955. *Sven Ohlon* referred to Georg Branting's bill on the establishment of a Swedish Commission to investigate the effects of the nuclear weapon tests. Branting had had little faith in the UN's capacity to carry out such studies, but Ohlon thought the problems were so severe that only an 'international body at the highest scientific level' would be able to overcome them. He thought that in addition to studying the effects of the nuclear weapon tests, such a body ought also to study the civilian problem of atomic energy waste.

Ohlon worried about the indifference generally showed by people regarding the atomic energy problems:

> […] when the big scientists peek into nature's most intimate secrets and in so doing place weapons in the hands of the military and the politicians, when these scientists who do actually know what is involved state opinions, one simply listens absent-mindedly as if it were a question of an anecdote.

Ohlon was backed by Tage Erlander on this point, who said:

> […] people have become used to relying on what the politicians and statesmen – or those they think are statesmen – say when it comes to public affairs, and this means that the politicians have great responsibility for informing the people, and we have a brilliant example of this here.

The foreign debate was also taking place on the same day, 9 March, in the Second Chamber of the Swedish Parliament. The People's Party leader *Bertil Ohlin* (1899–1979) criticised the government for not giving clear information on the issue of Swedish nuclear weapons. He was optimistic, however. The peaceful use of atomic energy would perhaps help to 'solve humanity's peacetime problems, a means of putting an end to darkness, poverty and starvation.' And the terrible capacity of the atomic weapon to destroy might end up meaning that 'apprehension and fear of the phases of the hydrogen and atomic bombs act as an ally for peace,' he added.

The Communist leader *Hilding Hagberg* (1899–1993) criticised the scientists whom the others appeared to trust. 'These experts are the ones who are constructing those terrifying weapons,' he said, but appeared to be the only one levying criticism. He denounced the Swedish experts 'for being so full of prejudices that they did not dare to talk about the fact that the world's first and only nuclear power plant had been in operation in the Soviet Union for nine months.'[*]

On 19 March 1955 head of Atombolaget Harry Brynielsson met Rusck and Vrethem at Rusck's office. Despite Brynielsson's reluctance to give ASEA a unique position, the agreement was to set up a technical cooperation committee whose task it was to investigate the possibilities of creating a 'commercial power-producing nuclear reactor belonging to Vattenfall's board within the shortest possible time'.

On 23 March there was a defence debate in Swedish Parliament. The leader of the Rightist Party (now known as the Moderate Party) *Jarl Hjalmarson* (1904–1993) pleaded in favour of Swedish tactical nuclear weapons and proposed that the FOA be reorganised into a State-owned company to be able to pay adequate salaries to attract competent scientists. The government was subjected to strong pressure to make a decision regarding the nuclear weapons issue.

However, Minister for Defence Torsten Nilsson maintained that it was still too early to make a definite decision. It would not hurt to wait because in another few years, [the military] research and development work would fully coincide with that which was needed to utilise nuclear energy as a power source.

[*] The reactor to which Hagberg referred was erected in 1954 in Obninsk 100 km southwest of Moscow. It was an RBMK graphite reactor, i.e., the same type of reactor as later in Chernobyl.

And he added that in:

> […] our current opinion, the production of nuclear weapons cannot start over the next few years under any circumstances.

Branting's and Åkerström's motions had been handed over to the general preparatory committee which sent them for referral. The referral bodies were surprisingly positive and agreed with the proposers that the information was dissatisfactory. But, unlike Branting and Åkerström, they did not want to see a special commission but preferred the information tasks to be given to The Atomic Committee. The Atomic Committee also thought that more information was needed, particularly since the information that did exist was often conflicting and difficult to comprehend. The Committee added:

> […] however, it ought to be pointed out here that many of the questions arising in this context cannot even be answered with great precision by the principal expertise available because this is looking at the very frontlines of research and technology and is within a field that is surrounded by secrecy, hush-hush and attempts to lead outsiders up the garden path.

The preparatory committee agreed with the proposers that there was not enough satisfactory information for members of the Swedish Parliament or the public and that an information service was needed. However, they did agree with the referral bodies that a special commission would not be needed since The Atomic Committee could take on the task. The Committee therefore determined that the motions should not lead to any action.

The technical cooperation committee between ASEA, Atombolaget and Vattenfall met for the first time on 23 April, consisting at the time of Lamm, Brynielsson and Rathsman. They decided to ask Atombolaget to inform them of the company's development work at the next meeting. A decision was also made that in future, the committee would consist of Sigvard Eklund, Lamm and Rathsman.

The Swedish Parliament's atomic energy debate continued on 4 May. In the First Chamber, Branting once again criticised the Swedish Parliament for its passivity. Sandler made a mental note of what was said about the questions being difficult to deal with and that international cooperation should therefore be sought. Foreign Minister Östen Undén said there were plans for an international study of the effects of the atomic weapon and that he himself wanted the government to take the initiative in the UN.* In the Second Chamber, Åkerström followed the same line as Branting. Here, it was *Rolf Edberg* (1912–1997) who took over Sandler's role.

The May issue of *Tiden* carried an article by the chair of The Atomic Committee, Malte Jacobsson, with the heading 'The peaceful and the military use of atomic energy'. The article reflected objections within the Committee. Jacobsson was concerned that a military nuclear weapons programme would prevent the expansion of atomic energy for peaceful purposes. Not only would there be insufficient personnel resources, a Swedish military programme would also make it impossible to import uranium and heavy water. Jacobsson thought that if Sweden needed atomic bombs, they ought to be bought from abroad. If it was what people wanted, they could be stored abroad to then be rapidly transported to Sweden in the event of a crisis.

The forms of cooperation were discussed by Atombolaget's board with Vattenfall and ASEA on 26 May, with Liljeblad representing ASEA. In his presentation, Brynielsson described Atombolaget as 'a central body for uranium production, reactor development, plutonium separation and fission products and isotopes'. The reactor development was the sensitive point. It was now clear that nuclear power

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* The initiative with which Sweden assisted and where Undén had had contact with Rolf Sievert led to the UN's general assembly taking the unusual step of setting up the Scientific Radiation Committee in December 1955 which is known by the acronym of UNSCEAR (the United Nations Scientific Committee on the Effects of Atomic Radiation).

reactors would soon become commercially available, but Atombolaget's interest was firmly in domestically-constructed reactors. Brynielsson said:

> Prototypes and commercial reactors will be constructed by an individual industry or government industry or in some cases through imports. When it comes to the construction of reactors, AB Atomenergi will function as a consultant.

The meaning of the last phrase is unclear, but Brynielsson probably did not want to release the plans that Atombolaget would be doing construction work.

The cooperation committee met for the second time on 17 June. The conditions had now changed in that some insight had been gained into what the big nuclear powers were planning prior to the Geneva 'Atoms for Peace' meeting proposed by President Eisenhower in 1953. Different cooperation and business invitations had started arriving from the USA, the United Kingdom and the Soviet Union. It looked as though heavy water reactors were no longer the only alternative available. Light water reactors which required enriched uranium had not previously been worthwhile considering because Sweden had no explicit plans for any enrichment plant (but had put aside a great deal of money for research into this). The position had now changed because the expectation was that enriched uranium would soon become available, at least from the USA.

Previously there had been doubts regarding such a development since it would mean that Sweden would be dependent on the continuous importation of uranium. However, there was now a possibility of starting the reactor with enriched uranium and then adding natural thorium (which consists 100 % of the isotope thorium-232) which can be transformed into fissile uranium-233. There were thus several alternatives to choose from.

The planned Atoms for Peace conference was held in Geneva in August 1955, with more than 1 000 scientific lectures being presented and 73 States represented along with several thousand participants. The Soviet Union participated, offering competent contributions and a willingness to cooperate. Professor The Svedberg reported the following on the conference in *Tiden*:

> The conference was held in Geneva for two weeks, 8–20 August. The UN made the *Palais des Nations* available as a meeting premises – the wonderfully-located building that was erected in the 1930s for the League of Nations and was then taken over by the United Nations. It was built in what was a moderate architectural style for the time with wide marble stairs leading to a vast park down towards *Lac Léman*. Colourful terraces of flowers bathed in brilliant sun and shadowy walks beneath trees that were hundreds of years old offered those taking part in the conference opportunities to have a little time to themselves to think and relax or chances to have discussions with colleagues in restful surroundings. The actual *Palais* with its big assembly hall and its numerous session chambers was filled to the brim by a gathering of energetic delegates from 73 nations, around 3 000 in all, who were eager to learn. There must have been a total of around 1 000 in an accredited audience plus men from the press. Our Swedish delegation, which included around 20 members, was led by the well-known UN specialist in atomic energy matters, County Governor Rickard Sandler. Physics, chemistry, biology, technology and industry were represented.

As well as Rickard Sandler, who was Chair of the Committee on Foreign Affairs and adviser to the Ministry for Foreign Affairs on atomic energy matters, the participating delegates were The Svedberg, George de Hevesy, Director of Atombolaget Harry Brynielsson and Secretary of The Atomic Committee Gösta W. Funke. As well as Rolf Sievert, the fifteen experts noted included Atombolaget's head of research Sigvard Eklund and the FOA's Torsten Magnusson, plus a number of other scientists who would end up playing important roles within the forthcoming Swedish atomic energy programme, including Erik Svenke, Jan Rydberg and *Roland Kiessling* (1921–2009). ASEA's energetic 'consultant technical director' Ragnar Liljeblad and physicist *Guy von Dardel* (1919–2009, Raoul Wallenberg's half-brother), radiation biologist Lars Ehrenberg and geneticist Åke Gustafsson were also present. Sievert showed off his mobile measurement laboratory, a specially-equipped Volkswagen bus.

The Geneva Conference enabled the civilian nuclear power industry to achieve its definitive breakthrough. Regarding the cooperation between Atombolaget, ASEA and Vattenfall, this meant that the cooperation committee's work ceased and direct negotiations between the parties began. This meant that the objections became clearer. ASEA emphasised the importance of not separating construction and production. Vattenfall, which saw itself as the main proprietor, was impeded because the government had not given its opinion on the board's plans. In the end, Atombolaget saw itself as the only party which had the necessary scientific competence for construction work.

On 2 September 1955, direct negotiations took place between ASEA and Vattenfall. The people who met were Liljeblad, Lamm, Rusck, Rathsman and Vattenfall's *Dag Jungnell*. They discussed the various possible alternatives, i.e., light water reactors versus heavy water reactors and nuclear power reactors versus reactors for the production of thermal power and plutonium. Rathsman thought it was possible to 'assume without further ado that the production of plutonium would be greeted with considerable enthusiasm by the military authorities'. In the end, they agreed to go with the heavy water idea and to develop a nuclear power reactor and also a reactor for the production of plutonium and thermal power. Rathsman said (according to Lindström, 1991) that they would 'see how ASEA reacted to a fairly normal invitation to tender in the autumn, and we now agreed that this appeared to be the best way of quickly getting the work started along normal lines'. Lindström writes about this (Lindström, 1991):

> So, it looked as though Atomenergi was losing the battle for Vattenfall to ASEA. However, it is important to emphasise that this did not mean that Atomenergi was completely disregarded. There was a limit to how hard Vattenfall and ASEA could press Atomenergi; they were dependent on the company's know-how and resources, and nor was it possible for political reasons to give Atomenergi too minor a role. If they adopted a collision course with the company, this would stir up bad blood, not just within the Swedish Parliament and the government but also within trade and industry. Industrial companies that specialised in subcontracts would, like the power companies, dislike the fact that ASEA had appropriated too powerful a position.

On 28 September, Vattenfall, Atombolaget's original cooperation partner, was held accountable before the company at a meeting with Sigvard Eklund, *Gunnar Holte* (1920–1985) and *Peter Margen* from Atombolaget and Rathsman, Jungnell and *Sture Ekefalk* (1909–1977) from Vattenfall. Ekefalk would soon succeed Rathsman at Vattenfall, who would become Deputy MD of *Karlstads Mekaniska Werkstad* (or Karlstad's Mechanical Works). Atombolaget's representatives were not pleased that Vattenfall had approached ASEA. Eklund thought that international contact would be impeded if the construction work were carried out by a commercial company.

The contact that was taking place between ASEA and Vattenfall also started to worry a number of private and municipal power companies, which feared that a monopoly situation would arise. At the end of September 1955, a Congress of the *Union des Producteurs et Distributeurs d'Energie Electrique* (UNIPEDE) was held in London. The general subject of conversation was what had happened in Geneva. A group of Swedish participants from the non-governmental power industry gathered to discuss how to approach the surprising American initiative which suddenly made enriched uranium and light water reactors commercially available. The group consisted of *Carl Kleman* (1887-1975), Chair of The Swedish Hydropower Association* and also of Krångede AB and the City of Stockholm's power company Svarthålsforsen, *Olof Berg* (born in 1901), head of Stockholms Elverk, *Ulf Glimstedt* (1915-2001), MD of Skandinaviska Elverk, *Tore Hedin* (1900–1981), MD of Gullspångs Kraftaktiebolag, and *Erik Ternström* (1905–1985), MD of Krångede AB.

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* The Swedish Hydropower Association was formed in 1909 when the Swedish rivers constituted the only significant source of power. In 1967 it changed its name to *Svenska Kraftverksföreningen* (The Swedish Power Association). It was initially a special interest association for the privately and municipally-owned power companies, with the State and Vattenfall as opposing parties. Then, The Swedish Hydropower Association became an industry organisation, and the now incorporated Vattenfall AB was a member. The Association's annual meeting in the year 2000 made a decision to close it down at the end of the year in order to merge with the newly-formed industry organisation *Svensk Energi*.

The group agreed that some form of merger was needed in order to work out how atomic power could be used in the new situation. It was left to *Olle Gimstedt* (1914–2008) to examine the issue under Kleman's leadership. Gimstedt had just taken up his post as Secretary and Executive Member of The Swedish Hydropower Association and ended up being a key person in the development.

The first step was that The Swedish Hydropower Association called its working committee to an information meeting on 25 October. An invitation to the meeting had been extended to the MD of Atombolaget Harry Brynielsson, who gave an account of the situation after the Geneva Conference and said that Vattenfall was now planning a 100 MW nuclear power station with Atombolaget and ASEA.

The working committee then discussed measures and decided that there was a need to form a joint power company to follow the development and to examine the conditions on behalf of the participants for building a joint nuclear power plant.

The partners in the new company would be the non-governmental CDL companies.* The company capital would be allocated in proportion to the annual fees to The Swedish Hydropower Association in 1955 so that the percentage share would be:

Krångede	21 %
Sydkraft	21
Stockholms Elverk	15
Stora Kopparberg	11
Uddeholm	9
Skandinaviska Elverk	8
Hammarforsen	8
Gullspång	7

In autumn 1955 until 15 November, the contacts and preliminary agreements between Atombolaget, ASEA and Vattenfall were highly confusing. Various drafts of agreements featured which had different ways of allocating responsibilities among the parties. The role distribution between Atombolaget and the industry was still not clear.

On Tuesday 15 November 1955, a two-day conference was started at the Rigoletto cinema on Kungsgatan in Stockholm. The conference was very ambitious and was a continuation of a research meeting that Tage Erlander had arranged in Harpsund in 1954. The intention was to bring together decision-makers and scientists. The conference was paid some attention by the media, with *Morgon-Tidningen* carrying a week-long series of articles in which the speakers were presented. The lectures and contributions to the debate were published in the book called *Tekniken och morgondagens samhälle* (Technology and the Society of Tomorrow) by the *Tiden* publishing company.

On the first day of the Rigoletto conference, Åke Rusck surprised the participants by saying that Vattenfall had plans to build two reactor stations called Adam and Eve. Adam was intended to be a thermal power station and Eve a nuclear power plant. According to Lamm's notes from the meeting, Rusck had said that he 'had fully intended to publicise this proposal before he put it to the Ministry and that he was anxious for Vattenfall to take the initiative rather than the politicians'.

Rusck's disclosure of Vattenfall's plans was all over the media and constituted *Morgon-Tidningen's* main news with big headlines on the front page the following day. Stefan Lindström writes (Lindström, 1991):

> At the conference, it also became apparent that Vattenfall and Atomenergi were not in agreement regarding the value of thermal plants and that Vattenfall had no faith in smaller plants – they supported 'very large nuclear power plants'. In the eyes of Atomenergi, it was really a matter of developing a type of reactor that could be introduced onto the market. For Vattenfall on the other hand, the thermal power reactor

* CDL stands for *Centrala Driftledningen* (the central operations management), a cooperation body between Vattenfall and the largest non-governmental power companies. The current equivalent of CDL is called KRAFTSAM.

> was a part of a development strategy and, to quote Rusck, they would 'take a rib from Adam to create Eve'.

The Rigoletto conference had a positive view of nuclear power and expressed great optimism for the future. Both Prime Minister Tage Erlander and Minister of Education and Ecclesiastical Affairs *Ivar Persson* in Skabersjö (1901–1979) gave addresses. Erlander's optimism led Minister *Ulla Lindström* (1909–1999) to make critical notes in her diary:

> At this moment, the circle of ministers gives me the impression of being a collection of eager boys who have a new mechanical toy to try out. Everyone is fascinated by the technical visions for the future which were conjured up at the conference on 'Technology and Society' at the start of November. Automation and atomic energy dominate the conversations. They require enormous investments that we cannot afford to make unless we reduce the rate of the consumption increase. Tage Erlander, the most speculative of us all, is giving his colleagues, the editors of the Social Democratic press and the Swedish Parliament group thrilling speeches on the long-term perspectives that the party must now prepare itself for.

After the conference, ASEA's take was that Atombolaget had had to start giving up its ambitions to do the construction work itself. Lamm comments on a new draft of a cooperation agreement between Vattenfall and ASEA:

> As regards the relations to AE [i.e., Atombolaget], the draft of this agreement simply mentioned that AE should assist with Vattenfall's implementation of the projects. ASEA's undertakings have instead been extended to include the preparation of necessary drawings, so the condition that was particularly difficult for us to accept, i.e., that AE would devise the main construction drawings for Vattenfall, no longer applies. Even the statements that Rusck was now making as regards the allocation of roles between AE and ASEA indicated a substantial change of attitude.

National Press Club, the Swedish Union of Journalists and the Swedish Association of Newspaper Publishers. The lectures were published in a book entitled S*verige inför atomåldern: 14 Svenska experter om ett aktuellt ämne* ('Sweden with the prospect of the atomic age: 14 Swedish experts on a current subject'). The book had been edited by Gösta Funke. During the course, Vattenfall as well as ASEA and Atombolaget gave accounts of their immediate plans. Stefan Lindström has summarised the situation (Lindström, 1991):

> It was […] not easy for an outsider to detect any differences of opinion. It was easier to note that there was significant unanimity regarding important points, including when it came to the fundamental energy policy analysis. Everyone was in agreement that atomic energy was necessary. Not initially because it was radically better or cheaper than some conventional energy sources but because it was the only alternative. The world's future energy needs would increase heavily. Brynielsson showed calculations which indicated an increase of anything between three and eight times until the year 2000. And the conventional energy sources could not fulfil the need. There was thus a further physical limit or, as Liljeblad put it:
>
> The saying goes that mankind knows for certain that he will die but he does not believe it will actually happen. Something similar seems to occupy the minds of those who are optimistic about fossil fuels.
>
> Finally, it can also be identified that the military aspects were catered for to a relatively considerable extent. There was evidently a technical connection between civilian and military nuclear power and the fact that reactors, and primarily heavy water reactors at that, produced plutonium which could be used for atomic bombs. It was also evident that this plutonium could be used as nuclear fuel.

The action of Vattenfall and ASEA concerned the government, which had now lost the initiative. The commercial interest appeared to determine the choice of reactors while, as Lindström puts it, 'There were other aspects – such as trade, preparedness and defence policy motives – to apply to the atomic energy policy'. Vattenfall's declaration of independence, even vis-à-vis the government, at the Rigoletto conference had disquieted the government and led to a number of internal ministerial memoranda. There are several undated versions of one of these, the 'Memorandum on Atoms', but it was probably formulated after the Rigoletto conference.

One of the drafts was written by hand by *Olof Palme* (1927–1986), who was Erlander's secretary at the time. This 'Memorandum on Atoms' stated the following:

> An organisational chaos prevails at this moment in time as demonstrated by the punches thrown in the columns between Vattenfall and Atombolaget with private industry fortifying its position in the meantime. There is an imminent risk of the government rapidly being faced with *faits accomplis* and fixed, face-saving points of view which will be very hard to change. What is required is a clear expression of will on the part of the government to establish an allocation of work between the bodies involved.

On Wednesday 23 November 1955 in the Cabinet Office, the government held what could have been its first discussion about its attitude towards a Swedish nuclear weapon. Torsten Magnusson gave an account of a report on the possibility of producing Swedish nuclear weapons. They rejected the alternative which involved building special reactors for the production of weapon-grade plutonium primarily on the basis of lack of competent personnel. Direct investment in weapon-grade plutonium would also involve competition that would be harmful as regards the development of civilian nuclear power. The discussions were described by Tage Erlander and Ulla Lindström. Erlander writes (according to Sydo, 1978 with parentheses and ellipses being Erlander's own):

> Undén thought that it (Swedish nuclear weapons) would mean that we would be unavoidably dragged into a big war. Sträng … thought that our investment options would not suffice and that our personnel resources would also be inadequate. Torsten (Nilsson) made a brilliant statement which put the boot on the foot where it belonged. It all ended with Sven Andersson and Gunnar Lange* being given the task of rapidly putting together a joint plan for the organisation of the 'atomic works'.

Ulla Lindström wrote in her diary (Lindström, 1969):

> There was an air of fate over the Cabinet Office during this discussion. Like me, Sträng and Undén were advocates of delaying a decision, not to mention generally averse to such a decision. But both of the government's patrons of atomic energy – Lange (to whom AB Atomenergi is signed), and Sven Andersson (in command of Vattenfall's board) – were at ease with the idea of the Adam and Eve reactors and thought from the start that it was rational right to build them so that plutonium could later be split for the production of nuclear weapons if required. However, I found so much as an unprejudiced and preparatory step dangerous and protested.

In her memoirs, Ulla Lindström writes that after the meeting she 'hinted to Inga Thorsson that something was afoot'. This meant that at a meeting of the National Federation of Social Democratic Women's board two months later, *Inga Thorsson* (1915–1994) stated that the Federation's executive committee had approached the Prime Minister and conveyed 'its strong protestations against nuclear weapons in the Swedish Defence'. It was also decided that Chair of the Stockholm section *Nancy Eriksson* (1907–1984) would write a motion (no. 9) against nuclear weapons to the Federation's

* Sven Andersson (1910–1987) was then Minister of Communications and *Gunnar Lange* (1909–1976) Minister of Trade and Industry.

Congress. The acrimonious wording of the motion led to strong antagonisms between the Federation and the party's leadership.

The discussions in the Swedish Parliament highlighted the dilemma in which the government had ended up. Stefan Lindström (Lindström, 1991) describes the situation with reference to a Communist interpellation on the acceleration of the civilian development work:

> […] the interpellation shows that the government had got itself into a paradoxical situation. The Communist Party of Sweden (SKP), the only party to have repudiated Swedish nuclear weapons, was also the party that was the most anxious for hard-line State control of the development work – a request which was completely in line with the coordination of civilian and military requirements which the FOA wanted. On the other hand, the parties that were calling for nuclear weapons were also those who were pushing the demand for freedom of trade, i.e., a demand that could lower both the political and the technical conditions for such a decision.

On 7 December 1955, the decision was made to form the company which had been discussed by The Swedish Hydropower Association in October and for which Gimstedt had drawn up a proposal. What Palme had referred to as 'the tranquil private industry' had taken action. The proposals concerned a consortium agreement and articles of association for the general partnership Krångede AB & Co. ('the Nuclear Power Consortium', abbreviated to AKK) and articles of association for a future AB Atomkraftverk when the time came.

The DG of Vattenfall Åke Rusck was informed on 8 December and on 10 December a press release concerning the new company was sent out. Once the boards of the partner companies had approved the agreement, it was signed on 13 December, Lucia Day. Carl Kleman was appointed as Chair at a statutory general meeting on 19 December. Olle Gimstedt provided the cohesion for the investigative work within the AKK. The new company had been formed at the last minute before the State ended up intervening.

Atombolaget held a board meeting on 14 December. The day before this was when the Prime Minister had declared in the Swedish Parliament that the government intended to investigate the civilian atomic energy development work. Brynielsson had been given power of attorney to lead the negotiations with Vattenfall on the basis that Atombolaget would handle the construction work. It was no longer definite that Vattenfall would be responsible for atomic energy. ASEA's plans were under threat. Maybe the State would take charge of all of the atomic energy activities.

8. THE NUCLEAR POWER PLANS BECOME REALITY

THE NOTIFIED ATOMIC ENERGY COMMITTEE was set up on Wednesday 21 December 1955. The Chair appointed was Secretary of State for the Ministry of Transport *Erik Grafström* (1914–1991) who would succeed Åke Rusck as DG of Vattenfall a few years later. The other members were MPs Sven Ohlon and Per Edvin Sköld. The Committee had three secretaries: Bo Aler, *Hans Håkansson* (1922–2019) and *Gösta Wiedesheim-Paul* (1906–1992). Bo Aler had been Assistant Professor at the FOA since 1952 but transferred to Atombolaget in 1957, where he firstly became head of the management staff and then became its MD from 1970–1978 when Atombolaget had withdrawn from its role as reactor developer. In the meantime, between the years 1964 and 1969, he had worked with nuclear power issues at the Ministry of Finance before the Ministry of Industry was set up in 1969. Hans Håkansson eventually became Assistant Under-Secretary and ended up playing an important role in a few governmental committees in the early 1970s; he was an Executive Member of the State Delegation for Space Activity from 1972–1986. Gösta Wiedesheim-Paul was an expert within the Department of Trade and Industry and finally became Deputy DG of the National Price and Cartel Office.

The investigation directions from Minister of Trade Gunnar Lange were based on the fact that it was important for atomic power to be put to use quickly. The rivers would soon be developed to capacity and Sweden, which relied heavily on hydropower, had to search for new energy sources to make the country independent of imported fuels.

The State's options to work efficiently towards this objective had to be improved. The Atomic Committee could not be used if it was now a matter of industrial construction rather than research and application. Vattenfall had shown an eagerness to get started but was dependent on the specialist knowledge of outsiders. Atomenergi had the specialist knowledge and the government therefore considered it to be

> […] natural for the company for the foreseeable future to have the main responsibility for the production of the reactor fuels and the construction of reactors in our country, for the handling of spent fuel elements and for associated activities in the field that might be covered by the planned concession legislation.

The instructions were clear where the operation of the future reactors was concerned. The responsibility had to lie with a governmental body, and for the time being with Vattenfall. In this respect, the desirability to keep open the option to be able to use the reactors for military plutonium production must have made a contribution.

The bourgeois press reacted strongly. *Svenska Dagbladet* carried the heading 'Socialised atomic power?' and *Dagens Nyheter* wrote 'The State intervenes in the atomic plans'. The disregarded party, ASEA, who had been surprised by the Committee's instructions, prepared countermeasures. On 4 January 1956, Deputy MD of ASEA *Halvard Liander* (1902–1990) wrote to Gunnar Lange saying that ASEA had concrete reactor proposals, cooperated with Vattenfall and negotiated with prominent atomic energy companies in the USA. He therefore considered that

> According to the record, the conclusion drawn by you, Minister, that it would be natural for AB Atomenergi to have the main responsibility for the construction of reactors in our country for the time being in our opinion could not have been drawn if

> the abovementioned circumstances had been known to the Government when the case was notified.

The letter aroused sympathy in the bourgeois press but was criticised in *Morgon-Tidningen,* which saw the size of ASEA as a threat to the desired development. The newspaper wrote (according to Lindström, 1991) that it was thanks to Atomenergi's research, which was financed by the taxpayers, that ASEA had been able to do draft for a reactor:

> It can also be said that ASEA has to some extent done a double deal since its representative on Atombolaget's board has taken care to obtain the information available at the company and is now making desperate efforts to make himself look like the country's most prominent atomic energy expertise.

This accusation, which personally affected board member Ragnar Liljeblad, aroused Liljeblad's wrath. He demanded rectification of 'the perfidious insinuations concerning my integrity' and indignantly wrote:

> Not one stroke of my constructions has been influenced by any experience that I have gained through being a member of AB Atomenergi's board or through incentives from them for that matter.

Morgon-Tidningen backed down to some extent, explaining that the debate had concerned essential points of view and that plagiarism had never been insinuated.

Despite the level of power demonstrated by the instructions of the investigation, the government allowed the negotiations between ASEA, Vattenfall and Atombolaget to continue during the investigation. The Committee simultaneously examined the conditions for an arrangement with trade and industry.

At the start of February 1956, the three negotiating parties appeared to be unanimous about the agreements that would be reached. Rusck wrote to Vattenfall's board:

> Following four months of negotiations, Ekefalk and I are happy to have now finally progressed far enough to be close to settling matters. Although we must make a few formal concessions to Atomenergi in the agreements, I am nonetheless convinced that the agreements will work well and that we can achieve good, open cooperation.

The proposal was to be handled by Atombolaget's board on 16 February and by Vattenfall's board on 23 February. But the government intervened prior to this. Rusck was called to see Minister of Communications Sven Andersson on 14 February and found out that he had to wait for the Atomic Energy Committee to submit its report – only then could the agreements be signed. Two days later, Atombolaget's board also postponed the matter of the agreement, also awaiting the result of the investigation. The government had stopped the negotiations. Stefan Lindström discusses why this had happened (Lindström, 1991):

> It is not particularly easy to answer the question and there are several different interpretations. To start with, it is a fact that the oppositions regarding the atomic weapons issue between the Social Democratic ministers and the split in the party management and the National Federation of Social Democratic Women's protests had placed the government – and the Prime Minister in particular – in a difficult situation. Erlander now had to lead a divided party, a divided coalition government* whilst countering the attacks from the opposition and the demands from the Swedish Armed Forces. He was forced to take a stand on the point at issue regarding both the civilian and the military programme and also to somehow adapt the civilian programme to what

* From 1951–1957, Tage Erlander led a coalition government with the Farmers' League (now the Centre Party).

the military wanted. It was not possible to be fully open in the latter case owing to the risk of a party split.

The apparent solution was to continue coordinating the civilian and military programmes for the time being but to delay a decision on the production of weapon-grade plutonium.

The atomic bomb was discussed again in greater detail at the meeting of the Social Democratic Party's board of 16 February 1956. The most important question was for how long the definitive decision-making could be delayed. The then Minister for Defence Torsten Nilsson did not question a possible link between a military and a civilian nuclear weapons programme. According to the minutes from the meeting (quoted in Lindström, 2000b), he said:

> If we were forced to procure nuclear weapons, we would be facing the question of whether we would be able to produce them ourselves. The possibility of utilising atomic energy for peaceful purposes also increases our military options.

He continued (according to Lindström, 1991):

> But if we look at the matter in practical terms, our first nuclear heating plant will not produce plutonium until around 1961. Were we to attempt to construct atomic bombs […], this could not take place until we had access to plutonium. The earliest this will happen is 1961. If specific preparatory work is to be undertaken in the purely scientific field, we do not need to make a decision on whether we are going to allow nuclear weapons to form part of our arsenal until 1958. On this basis, I conclude that since this is not a comfortable political decision, and since we ought to cling to the hope that an international agreement will be reached, we can say that we will not be obliged to make a decision until 1958. In my opinion, we can then wait and see how things develop politically. In the best case scenario, we may be released from having to make any such decision. However, there is much to indicate that this will not be the case. But we ought not to tie ourselves to a specific opinion at the moment.

Per Edvin Sköld, who had now left the government but was part of the Atomic Energy Committee, also expressed an opinion at the meeting:

> […] the production of atomic bombs will not involve any major expenses. Nor will substantial personnel resources be used if it is combined with peaceful use. There is not really that much to discuss. On the other hand, there is no doubt that developing atomic energy for peaceful purposes is the quickest way to obtain weapon-grade plutonium.

At the same meeting, Inga Thorsson declared that the National Federation of Social Democratic Women intended to campaign against nuclear weapons:

> We have used various ways to clearly dissociate ourselves from the idea of procuring nuclear weapons. We have motions to our Congress and will adhere to that view even if the party is going to delay matters. […] This is the first time that we have been given the power to wage war against a forthcoming generation. That is when we no longer want to be involved.

Inga Thorsson's contribution led her to be exposed to great pressure, which Östen Undén found uncomfortable:

> Eventually Erlander spoke and, strangely enough, he uttered what amounted to pathetic rhetoric, turning to face Inga Thorsson and admonishing her to change her position and accept that the matter was being postponed.
>
> […] I polemised against Erlander and could not understand the attacks against Inga T. I said I was satisfied that Torsten Nilsson had explained that the matter was not relevant until 1958. But it was of course completely natural for the women to have taken a stand. This did not necessarily mean that they had started a conflict within the party.

Ulla Lindström describes the same meeting and criticism against Inga Thorsson as follows:

> Everyone bombarded her with threats and the most pathetic exhortations, that is when they were not making contemptuous observations *à la* Lassinanttis*. In sing-song Norrland dialect he said: 'Ultimately, the women always follow their men, so let's hope they do in this case as well.' However, the most distasteful was Sköld. He promised to establish within the party a countermeasure of propaganda for nuclear weapons against the women's 'pacifism'.

On 20 March 1956, Torsten Nilsson gave an introductory address at a meeting with the group of Social Democratic Swedish MPs, a more open circle than the party's board, and then said (according to Lindström, 2000b):

> We have great options for the peaceful use of atomic power. However plutonium is produced as a by-product in the reactors and we then have material for atomic weapons. The scientists say that plutonium could be used to produce an atomic bomb relatively cheaply.

So, the possible and, according to some, undesirable link between a civilian and a military atomic energy programme was not exactly a great secret in the mid-1950s although in practice, there was still no link other than the fact that the short-term goal (a functioning reactor) required similar preparations, irrespective of the long-term goal. Stefan Lindström writes (Lindström, 1991):

> One disadvantage of this strategy was that it could not be openly reported in order to calm down the supporters of nuclear weapons. Outwardly, it seemed as though the government was dragging its feet making a decision, which enabled the right and the liberals to accuse the government of agonising over a decision. The strategy also meant that the government was attacked by those who opposed nuclear weapons. In other words, it was a strategy whose manifestos pleased neither supporters nor opponents.

Although the government and people who were close to the government openly declared that there was a link between the civilian and military research, in winter 1956 the political discussion continued in Swedish Parliament as though the opposition – including the Communist Party of Sweden – had not understood or realised that such a link existed.

In March 1956, Olle Gimstedt had drawn up a memorandum for the board of the Nuclear Power Consortium containing various alternative actions, one of which involved buying a nuclear power plant from outside, e.g., from Westinghouse. It was deemed that all alternatives would incur expenses of approximately 30 million Swedish kronor.

An invitation was extended to Curt Mileikowsky to attend the Nuclear Power Consortium's board meeting, a nuclear physicist who had previously worked at the Nobel Institute for Physics but who had been employed by ASEA since 1954 and would become head of its nuclear power department in 1958. Mileikowsky was asked to talk about the reactor situation in the USA. He spoke about the American Power Reactor Demonstration Project which involved the Yankee pressurised water reactor in Massachusetts and the Indian Point one near New York, and the Dresden boiling water reactor near Chicago. It was primarily Sydkraft's representative on the board of the Nuclear Power Consortium who was anxious for quick action. However, Carl Kleman concluded that it was still too early to assess which type of reactor was the most suitable for a nuclear power plant. The decision was therefore to wait for the outcome of the continuing investigations.

The Atomic Energy Committee produced its report on 12 March 1956. The Committee's proposal was not as far-reaching as had been stated in the instructions. Civilian atomic energy was considered

* The vivacious *Ragnar Lassinantti* (1915–1985) was a member of the Social Democratic Party's board and became a member of Swedish Parliament in 1957 and was County Governor for the County of Norrbotten from 1966–1982.

necessary bearing in mind the rapid increase in the need for energy and the desire to reduce the dependency on importation where fossil fuels were concerned. Atomic energy could also save a few

Swedish rivers that had not been developed. Nor was industry's option of developing products for export inessential.

The proposed investigation put Atombolaget in a leading position for the foreseeable future; the company would 'be responsible for the practical activities during the development stage'. Atombolaget would, as Brynielsson had always wanted, be responsible for the construction work to the extent that the company considered appropriate. However, the proposal did not preclude agreements of the type previously spoken of, but the State now had a stronger grasp of the development. However, this required the equivalent of other countries' atomic energy commissions. It was therefore proposed that a special delegation for atomic energy matters be set up under the Department of Trade and Industry. Reorganising The Atomic Committee was not considered to be suitable for the purpose; instead, the Committee ought to be turned into a Research Council (its activity was transferred in 1959 to the newly-formed National Council for Nuclear Research).

Most of the referral bodies criticised the concentration of construction activities for Atombolaget. The marked exceptions were the FOA and the Defence Staff, which welcomed the concentration. The FOA thought 'this means that work carried out in various places can be subsumed into a common objective and planning, thereby avoiding unnecessary duplicate work'. The Swedish Trade Union Confederation was late in stating its opinion. Stefan Lindström speculates that this was a consequence of a reminder from the Chancellery once it was shown that so many referral bodies were negative. The Swedish Trade Union Confederation said what neither the FOA nor the Defence Staff had said in straightforward language:

> The military importance of atomic energy and the great demands that must be set regarding protection devices in connection with atomic energy activities need to be controlled by the public.
>
> Rickard Sandler was even clearer in a statement of opinion from the Ministry for Foreign Affairs:
>
> It is generally known that peaceful atomic propulsion can technically be adapted to the production of nuclear weapons. The question of arms control cannot therefore be separated from questions regarding the operation of nuclear reactors.

Atombolaget's statement of opinion was slightly more accommodating towards the industry than the Atomic Energy Committee had been:

> The company assumes that when constructing the prototypes, the industry also has the option of making its own efforts. This does not apply purely in the cases where different constructions can be carried out by the company and suitable industries in cooperation. Likewise, it appears that Swedish industries and power producers which maintain contact with foreign companies that have progressed some way within atomic energy activities, without forgoing the concentration of the Swedish resources proposed by the preparation work, are drawing up their own proposals for reactors or reactor parts. […] The industry's endeavour to also develop reactors for future export should be supported.

With reference to Atombolaget's statement of opinion, the company's individual shareholders decided to send the following message to the Minister of Trade:

> The individual shareholders would like full freedom for the industry to do its own development and construction work. If one may interpret the report's proposal in accordance with Atomenergi's statement, there clearly are possibilities of achieving such freedom. If the bill clearly states that such freedom may exist, all prospects of an agreement lie with the individual shareholders.

On 13 April 1956, a government cabinet meeting made a decision on bill 1956:176 regarding Swedish atomic energy. The bill principally followed the Atomic Energy Committee's report and Atombolaget's statement of opinion. It justified the increase in the governmental influence in that atomic energy required special treatment due to 'political and military problems and security issues'. Minister of Trade Lange, who was behind the bill, was less optimistic about the development than Erlander had been. The set-up of atomic energy would require 'enormous' efforts and he thought it would be 10–20 years until nuclear power plants would be able to compete with conventional power plants.

Industry's concern that Atombolaget would attain a monopoly position was countered by Lange quoting Atombolaget's statement of opinion, according to which there was room for the industry to make its own efforts. Lange finally approved the report's proposal to establish a special delegation for atomic energy matters.

Before the bill was dealt with in Swedish Parliament, the National Federation of Social Democratic Women's Congress was held in the Grand Royal Hotel's winter garden in Stockholm. This is where the Stockholm section's motion no. 9 would be discussed with the risk of the conflicts between the party and the National Federation of Social Democratic Women increasing. However, following a strong contribution against nuclear weapons, Inga Thorsson requested support for a less controversial statement. It is interesting to note that motion no. 9 had stated that the possibility of purchasing nuclear weapons from the big powers was grounds for a statement, but that Thorsson was now indicating the real reason:

> The idea has now been raised and discussed that, as part of its endeavours to procure the most effective possible defence at the cheapest possible price, Sweden shall plan the use of atomic energy so that this could be used in our country to produce tactical nuclear weapons at some stage. The Stockholm section has put forward motion no. 9 with reference to this.

Bill 176 was handled for Swedish Parliament by the Third Judiciary Committee, which had had no strong objections. The idea of heavy water was accepted but it was emphasised that other alternatives should not be disregarded. Swedish Parliament's decision was made on Tuesday 29 May 1956 when Professor *Hugo Osvald* (1892– 1970), Chair of the Third Judiciary Committee, called for support for the Committee's petition for bill no. 176. Osvald had introduced his high-flown address with the words 'We are on the threshold of a new age'. Having made its decision regarding substantial investment in atomic energy, Sweden had thus entered the atomic age with the programme that would end up being called 'the Swedish line', and left the door open for a later decision on nuclear weapons. The characteristic thing about the programme was that they wanted to use natural uranium from Sweden with heavy water (from Norway) as a moderator. This would make them independent of access to enriched uranium, but they had not denied themselves the option of producing weapon-grade plutonium.

The choice was well prepared for. Stefan Lindström (Lindström, 1991) writes:

> The basis presented by the Atomic Energy Committee – as with Liljeblad's early forecasts – and primarily the Fuel Committee's reports* clearly show that the choice was preceded by relatively advanced analysis work. Long-term forecasts regarding the development of the energy field – in both global and national terms – and an assessment of the world's political development formed the external frameworks for an inventory of various alternative actions which not only covered energy technologies and energy systems (established for the future) but also attempted to define the problems from an organisational and political perspective.
>
> No attempt will be made here to assess the quality or sustainability of these forecasts. […] But, to give examples of simple and mechanical trend projections and of fantastic hopes for the future that were occasionally given free rein, this must also be weighed up

* SOU 1956:46 Fuel supply in the atomic age, part 1. Report on the 1951 fuel investigation, and SOU 1951:32 Fuel and power. Report published by the Fuel Committee.

> against the cautiousness that still prevailed in spite of everything, maybe primarily among the technicians, when it came to the future possibilities for atomic energy. It can thus hardly be said that the notion which is sometimes encountered, i.e., that the technicians were the intoxicated lead singers of the new technology, is correct.

On 1 June 1956, Swedish Parliament also adopted the Atomic Energy Act (SFS 1956:306). The introductory section of the Act read:

> Section 1. Without the permission of the King or an authority specified by the King, no-one may acquire, possess, transfer, process or otherwise have dealings with uranium, plutonium or any other substance that is used as fuel (nuclear fuel) in a plant for the extraction of atomic energy (nuclear reactor) or a compound of which such a substance is a part. […]
>
> Section 2. Without the permission of the King or an authority specified by the King, no-one may erect, possess, or run a nuclear reactor or plant for the processing of substances or compounds referred to in Section 1.

In connection with the introduction of the Atomic Energy Act, the Delegation for Atomic Energy Matters (Dfa) was set up as a separate advisory body to the government, which at this time meant the Department of Trade and Industry under Gunnar Lange. The Delegation was also asked to take responsibility for examining and supervising the reactor safety in accordance with the Atomic Energy Act. It was active in this manner until 1970 when it was transformed into an independent authority, although with the same name for the time being.

The Atomic Energy Delegation was too closely linked to the Ministry to be operational. It was therefore complemented by a sub-committee called the Reactor Siting Committee (Rfk) as an operational body while the Dfa functioned almost as a board. The Secretary of State for the Department of Trade and Industry acted as Chair of the Dfa while the FOA's head of department Torsten Magnusson was chair of the Reactor Siting Committee until 1 July 1968 when he became Director General of the FOA and was succeeded by Arne Hedgran from the Radiation Protection Institute.

At this point, it may be of interest to follow the development of the Dfa until 1974 when the State's Nuclear Power Inspectorate (SKI) came into being. In spring 1967, the Dfa was transferred from the Department of Trade and Industry to the Ministry of Finance. This meant that the operational part of the activity, i.e., the Reactor Siting Committee and its Secretariat, ended up in a carousel of moves, according to *Tore Nilsson* 'first to *Räntmästarhuset* (the Treasurer's House) at Slussen, to Storkyrkobrinken high up above the Cattelin restaurant shortly thereafter, and eventually to Liljeholmen'.

On 1 January 1969, the Dfa was transferred to what was then the new Ministry of Industry, from which the Delegation split off on 1 July 1970 to become an independent authority with its own budget, although under the same name. The Dfa then moved to Atombolaget's abandoned offices at Liljeholmen (the company's personnel had moved to Studsvik). Arne Hedgran left the SSI and became head of the new authority, which was reorganised on 1 July 1974 and was given greater resources, and in this connection was named the National Nuclear Power Inspectorate.

The oldest reactor inspectors at the SKI had been employed early on for the Dfa's activities: *Erik Jansson* in 1959, *Tore Nilsson* in 1962 and *Paul Ek* in 1963. Ek started with the safeguards activity, i.e., control of the use of fissile material, early on. This was appropriate when private ASEA also came onto the scene alongside Atombolaget.

An important research task for the FOA was what leads up to an explosion; to be more specific, that which occurs in an atomic bomb before the nuclear chain reaction starts. The intention is for a conventional explosive charge, appropriately positioned, to transform a subcritical configuration of plutonium to a critical mass which undergoes a chain reaction. The conventional chain of detonation events and its shockwaves lead to high pressure and temperatures which affect the material in the bomb. The research into this was moved to the experimental station by Grindsjön, which came about in 1941 at the initiative of Rolf Sievert and was made possible by a donation from Dr. *Olof Arrhenius* (1895–1977). The experimental station had been used by the Military Physics Institute (MFI) for research into explosion and rockets. This was where early experience was gained of the targeted explosive effect. The equipment was now supplemented with high-speed cameras and flash x-ray equipment. The person who ended up leading the activity was a fellow student of mine from KTH, *Lennart Lundberg* (1924–1969), who unfortunately died at the early age of 45. Lennart has been described as a Swedish Oppenheimer through his nuclear weapons research and his interest in mysticism and philosophy.

While Lennart Lundberg became head of institution 250 for detonics under FOA 2, under FOA 4 there was a section 471 under Ulf Ericsson for shockwave and the thermal effect. Ericsson prepared experimental explosions (which were never carried out with nuclear charges) at Nausta in Norrbotten. However, experiments were done using 50 tonnes of conventional explosives to simulate a nuclear weapon explosion, a powerful bang.

Despite what Torsten Nilsson and Per Edvin Sköld had said about the insignificant cost of a Swedish nuclear weapons programme, the concern was that it could still be a costly affair, and the Swedish government was not averse to finding alternative methods. It is no longer a secret that, despite neutrality, a certain amount of defence cooperation took place between Sweden and the USA during the cold war. The possibility of obtaining assistance with nuclear weapons from the Americans was investigated by *Erik Boheman* (1895–1979), Sweden's Ambassador in Washington. In 1956, he reported to the government that he had been in contact with people such as the then Foreign Minister *John Foster Dulles* (1888–1959) and Chief of Staff Admiral *Arthur W. Radford* (1896–1973) regarding the matter. The contacts did not concern the actual bombs but the equipment for the use thereof. Boheman wrote (according to Jont, 1999):

> We are convinced that we could produce nuclear weapons ourselves. However, it will require money, time and unnecessary experiments. No decision has yet been made in this respect. We are not disputing the possibility of purchasing nuclear weapons from the United States. We understand that this would be fraught with difficulties and it would also compromise our neutrality. But we would like to equip our new aircraft with the same atomic bombing devices that are or will be in the European NATO plan; we might like to purchase or find out about the construction of other such weapons that could be used for either atomic warheads or conventional charges. Finally, we would like to study atomic warfare.

At the end of June 1957, Boheman took up the matter of assistance with obtaining nuclear weapon carriers with the American Department of State once again. He was thinking of (dual purpose) robots that could carry either conventional explosives or nuclear charges. He said (according to Jont, 1999):

> […] dual-purpose weapons like *Nike* or *Honest John* which had recently been offered to Norway and Denmark relying, as Norway and Denmark had to, on the fact that nuclear warheads will be provided if necessary.

But the Americans were not unexpectedly negative towards the idea of facilitating matters for Sweden to develop nuclear weapons. Why would they do this when there were already more than enough nuclear weapons available to NATO in Europe? Sweden with its very own nuclear weapons without any American support certainly would have been an uncomfortable uncertainty factor which it would not have been possible to demand to control. But American experts did not think that Sweden would be able to develop its own nuclear weapons totally without support from the USA. Therefore, in 1960, the answer to continuing Swedish enquiries about nuclear weapons was no. By then, Sweden had already obtained from the USA nuclear fuel and components to run reactors through a cooperation agreement from 1956. In this connection, the USA was able to demand that compliance with the terms for the cooperation agreement – solely peaceful use of that which had been supplied – be controlled.

American inspections were carried out during 1960–1964, although initially only of Atombolaget's plants but then also of the FOA's plutonium laboratory in Ursvik and, once AB Asea-Atom was formed in 1968, also of its fuel factory in Västerås. The inspections were carried out a couple of times a year by one or two officials from the American Atomic Energy Commission (AEC). The Swedish Reactor Siting Committee had nothing to do with the inspections. However, Arne Hedgran, who participated in the

Dfa's meetings, remembers that 'really young diplomats sometimes gave incomprehensible information about negotiations between the USA and Sweden regarding atomic energy matters'. His impression was that the USA inspections went smoothly. Authorities and industry in Sweden were positive towards the USA. The American inspections were not taken over by IAEA, the UN's atomic energy body, until the 1970s.

In 1957, the Nuclear Power Consortium started to pre-plan a nuclear power plant with a 200 MW output. Olle Gimstedt writes the following about this (Gimstedt, 1985):

> Again we scouted around for a suitable place. We cooperated with Sydkraft, which made a big list of conceivable places within its own area. A unique requirement was that we considered the possibility of a mountain location. The result of this scouting around was that we had two main alternatives: an area by Ivösjön and one by Vättern. Of these, the Ivö alternative stood out as the most appealing.
>
> The work with searching for locations continued during 1958. We had now dropped the underground location requirement, which opened up options of locating the nuclear power plant on the coast.
>
> These surveys resulted in Simpevarp, north of Oskarshamn, looking particularly favourable.
>
> With reference to the continued project work, it was now appropriate to form the limited liability company *Atomkraftverk* (AKV) as presupposed in the consortium agreement.

The Swedish military's desire for nuclear weapons strengthened even further following Swedish Parliament's decision regarding 'the Swedish line'. Reports lent support for the usefulness of tactical nuclear weapons within the Defence. The tactical weapons would be used for defence purposes only and were seen at the time by the military as nothing more than a more effective weapon. Unlike the 'Hiroshima-strength' tactical weapons, the much more powerful strategic nuclear weapons were considered to come under the umbrella of the armed forces that wanted to attack and terror-bomb cities.

The FOA gave Atombolaget the task of investigating. On 23 October 1957, Sigvard Eklund wrote to Lieutenant Colonel Torsten Schmidt of the Defence Staff, saying that Assistant Professor Gunnar Holte at Atombolaget was responsible for investigations that were of significance to the military nuclear weapons programme. Of particular interest are the requirements that ended up being set by the military regarding the construction of Atombolaget's nuclear power reactor R4, the one that ended up at Marviken, possibility to facilitate refuelling during operation.

In 1957, Atombolaget proposed the erection of a factory for the production of uranium in Ranstad near Billingen. The capacity was calculated as being 120 tonnes uranium per year, which would be sufficient for the reactors that could be anticipated. However, the decision on Ranstad was postponed since it soon proved to be economically beneficial to import uranium.

During spring 1957, a solicitor who had been employed as deputy secretary of The Atomic Committee was revealed as being a spy for a foreign power. The revelation was pretty drastic in that Dr Funke and his colleagues were woken up in the middle of the night by police who had the task of ransacking houses in the hunt for evidence and any accomplices. The event hit the press headlines with stories about the 'atomic spy'.

The Social Democratic party was threatened by a split with regard to the nuclear weapons issue. The old politicians Östen Undén and *Ernst Wigforss* (1881–1977) were against the atomic bomb. The somewhat younger veteran Per Edvin Sköld spoke heavily in favour of the bomb. The National Federation of Social Democratic Women campaigned actively against the bomb. The conflicts culminated in 1958 when Supreme Commander *Nils Swedlund* (1898–1965) requested 2.8 million Swedish kronor in his estimate of expenditure for the FOA for research into the construction of nuclear weapons. A few statements made by well-known people in 1958 were quoted in *Tidens Kalender* in 1959. Some are sufficiently illuminating to be reproduced here:

> There are plenty of editors whose reasoning is as follows: the hydrogen bomb is so monstrous that it is a guarantee against war – but were it to be used, the injuries would

probably not be as great as people maintain! That logic dictates that it be accepted as life assurance! For me, it is like a person committing suicide because he is afraid of dying.

(*Journalist Barbro Alving* [1909–1987] *on the radio on* 14/3)

It is meaningless and dangerous for a small country to take on nuclear weapons. The talk is of tactical nuclear weapons. That is pulling the wool over people's eyes. The tactical weapons are atomic bombs; you cannot get away from that.

(Professor Åke Gustafsson in DN on 31/3)

Sweden ought not to procure evil weapons – and call them tactical! (Tactical is one of those mitigating words, touching on 'tactful' before you find out what its real meaning actually is. The bomb that destroyed Hiroshima had the power of what is now referred to as a tactical bomb …) But is it not the case nowadays that it has suddenly become so dangerous to live that we might as well say we can only afford to do the most important thing, to occupy ourselves by doing some good? We should not purchase or produce atomic bombs – it could be our last occupation. But if rather than procuring Hiroshima bombs we actually went in to alleviate a fraction of the tangible destitution in which two thirds of human life lives, now that would be a good and important occupation, whether or not it could not then guarantee us any security.

(Author Sara Lidman [1923–] in DN on 5/4)

Mrs. Lidman and her sympathisers forcefully emphasise what we are all agreed on, i.e., that war is hideous and that a war with nuclear weapons is even more hideous than other wars. Their conclusion is out with war, out with nuclear war, out with the idea of nuclear weapons in Sweden. However, in thus pursuing their reasoning, they fail to look at the realities that eventually not even indignation and fear can avoid. The hatefulness of war is one thing, the matter of Sweden's defence is quite another. The fact that war is hateful does not mean that Sweden should disarm. There are disconsolate people who are unable to make this distinction, but it is a duty for us all to seek to do so …

That we would defend ourselves but only with totally inferior weapons, that we should have cannons but only cannons with one thousandth of the power of defence weapons – surely that cannot be the opinion?

(Professor Herbert Tingsten in leader in DN on 6/4)

A Swedish tactical nuclear weapon is a threat to no-one. The fact that we have it is and remains a warning to every attacker. For way more than a hundred years we have shown that if we are to live in peace, we must leave others to live in peace. Anyone who mistrusts our defence efforts is not looking to understand but to misunderstand.

(Right-wing leader Jarl Hjalmarson in a speech in Borås on 12/4)

In 1958, twenty or so troubled people formed the Action Group against Swedish atomic bombs, AMSA. The group contained both pacifists and defence supporters. AMSA was most active during 1958–1959 and dissolved in the 1960s when it was clear that Sweden would not be procuring any nuclear weapons.

The first UN Atoms for Peace conference in Geneva in 1955 had been so successful that a second conference was held in Geneva in 1958. This time, Sigvard Eklund had been asked to be the Head Secretary of the conference (the Indian nuclear physicist *H.J. Bhabha* (1909–1966) was its President). This was Eklund's first major step into the international activity to which he would devote the rest of his life after 1961.

In 1958, the Supreme Commander's call for funds for the FOA's nuclear weapons research was not taken up by the Minister for Defence (Sven Andersson) in Swedish Parliament; it *was* put forward as a motion but was then rejected by a large majority. Erlander says in his memoirs that the government thought the FOA's protection research ought to continue as a high priority but that no research concerning the construction of atomic bombs could take place. Erlander found a source of comfort in that his advisers had said that the civilian and military research wishes would continue to coincide for another three to five years. No decision therefore needed to be made just yet, but it was important to keep their options open. ‘Freedom of action’ became a slogan for some time to come.

Nevertheless, in 1959 the Supreme Commander put forward a proposal for research into nuclear charges at the FOA. And this proposal was not taken up by the Minister for Defence in the bill for the 1960/61 budget year either. In 1961, a presentation was held by the FOA for the government on the situation within atomic research. The FOA emphasised that research over and above civilian research was now required for nuclear weapons; to construct a bomb, they primarily needed to study the initiation mechanism. However, since this would not be relevant over the next few years until weapon-grade plutonium became available from one reactor or another, Erlander thought that they could delay the bomb research and still preserve the freedom of action. This was the time when he ought to have abandoned the thought of Swedish atomic bombs.

Atombolaget, which was responsible for the reactor plans according to the Swedish Parliament’s decision of 1956, designated the planned reactors by using consecutive digits after an ‘R’ for reactor, i.e., R1, R2, R3, R4, R5 … Of these, R1 was the first reactor, the one that was built on Drottning Kristinas väg. R2 was the material testing reactor that would be stationed at Studsvik. R3 would be a thermal reactor. Only R4 would be the first real nuclear power plant.

Vattenfall’s reactor plans had initially covered the thermal plant Adam and the nuclear power plant Eve, of which Adam had originally been planned for Västerås.

So, the first of the new reactors was the one that would be in Studsvik, i.e., R2. It had firstly been intended as an enlargement of R1, i.e., a larger heavy water reactor, but after the Geneva Conference in 1955 it suddenly became possible and beneficial to buy ready-made reactors and enriched uranium from the USA. The American government also gave Atombolaget a contribution of 350 000 dollars, which therefore enabled R2 to become an imported light water reactor with heavily enriched uranium fuel. It was commissioned on 4 May 1960 at 03.54 with *Robert Skjöldebrand* at the control panel (in reality, but King Gustaf VI Adolf did not press the start button with all the pomp and circumstance until 1 June). It had a thermal output of 50 MW. R2 is still in operation and is used especially to produce radioactive isotopes for medical purposes.

A ‘zero energy reactor’ was also built the same as it was at R1. It was ordered from ASEA in March 1959 and ended up being called R2-0 (R2-zero). This was the first order for a full reactor that had gone to a Swedish company in the normal commercial way, i.e., at a fixed price and with normal guarantees. The reactor was originally dimensioned to be run with an output of 100 kilowatts (kW), but was sometimes run with outputs approaching 1 MW.

Reactors R2 and R2-0 were sunk into a pool approximately 3 metres in diameter and 18 metres in length. The usual depth of the water was 8.6 metres. This long pool was divided into three approximately equal-sized sections with the help of two vertical, watertight ports. Reactor R2I was in the one end and R2-0 was in the other end. The middle third was intended for the temporary storage of used fuel elements.

In the lower section of the R2-0 pool was a large aluminium plate ‘window’ that covered an equivalent-sized hole in the thick pool wall on the one long side and four smaller ‘windows’ divided down the length of the short side and the opposite long side. It was possible to move the reactor core between the windows. The same type of fuel element was used for both of the reactors with highly-enriched uranium. The thermal effect from R2-0 was cooled down by the pool water through natural thermal conductivity.

One important use of R2-0 was to test experimental devices which would later be used in the larger reactor through which there was no risk of losing expensive operational time in R2, which could have occurred if the devices had been inserted directly. R2-0 is still used following a number of protection fortifications and has more recently had greater relevance to irradiation with neutrons for medical purposes. The reactor was run up to criticality on 20 June 1960 and was commissioned for the planned experiments in autumn 1960.

The primary motive for Swedish investment in nuclear power was to find a new source of energy to meet the increasing demand for electricity and heating when hydroelectric power started to become developed to such an extent that continued development aroused protests. Conventional nuclear power plants based on oil were still economically competitive but the government wanted to make Sweden independent of fuel imports as far as possible. An upper limit for hydroelectric power was set in 1961 when the power companies and nature protection partners agreed on the future development of hydroelectric power (Peace in Sarek).

At the end of the 1950s, both Atombolaget and Vattenfall started to realise that their intended development programme would be more expensive and more knowledge-intensive than their initial optimism had imagined. Compromises were needed in order to be able to continue. In 1958, Atombolaget's thermal reactor R3 was merged with Vattenfall's Adam, which meant that the latter was never erected in Västerås. Instead, the merged R3-Adam reactor was the one that was built in a rock cavity at Ågesta in southern Stockholm.

For the 1957/58 budget year, Vattenfall requested funds to erect its planned nuclear power plant Eve at Lake Unden in north-western Tiveden in Västergötland. This request was denied. As in the Ågesta case, Vattenfall's and Atombolaget's plans were instead reduced and merged to become the nuclear power reactor R4, Eve, which would be built at Marviken on Vikbolandet by Bråviken east of Norrköping.

The thermal reactor at Ågesta became a hybrid, a thermal reactor with a thermal output of 65 MW, 10 MW of which were used for the production of electricity while 55 MW were used for district heating for the Stockholm area of Farsta. The Ågesta reactor used Swedish natural uranium (in the form of uranium dioxide) and Norwegian heavy water. It was a pressurised water reactor where the heavy water under high pressure functioned as both a moderator and the primary coolant. It was then conveyed through four steam generators where the water was usually heated to boiling point so the steam could drive a back pressure turbine* which in turn drove a generator to produce electricity. The waste steam from the turbine was finally conveyed through a heat exchanger where it was condensed and the heat was transferred to a hot water pipe to heat Farsta.

ASEA became the main supplier of the reactor for Atombolaget and Vattenfall, which took over operations in 1965. The City of Stockholm was responsible for the actual turbine and the district heating system. The reactor became critical on 17 July 1963 and was in commercial operation from March 1964 until it was closed down in 1974. The plant was a prototype and, like R1, a practice object from which to obtain experience. It would, had this been the decision, have been able to contribute plutonium that was of interest to the military but the structure of the reactor did not allow refuelling during operation. The plutonium produced by the reactor was not weapon-grade.

The major project was a combination of Atombolaget's R4 and Vattenfall's Eve, the culmination of 'the Swedish line'. Atombolaget - primarily its MD Harry Brynielsson - was the driving force; the development turned into a matter of prestige. Vattenfall hesitated with regard to the substantial investment in a large, untested project. Atombolaget's technical director and head of research Sigvard Eklund shared the doubt - he did not think the time was right. This meant that there were conflicts within the company, which may have contributed to Eklund leaving Sweden in 1961 to become Director General of IAEA in Vienna. A powerful man who had substantial influence within Atombolaget during the 1960s was Master of Engineering *Ragnar Nilsson* (1918–1993) who became Construction Manager in 1960, Project Manager in 1963, Technical Director in 1967 and Deputy MD in 1969.

At the end of the 1950s, a commercial development of light water reactors started in the USA, a development which the Swedish power industry followed with interest. In 1954, the world's first nuclear-powered submarine, *Nautilus*, had been launched in the USA and in 1958 it demonstrated its reliability by reaching the North Pole beneath the ice.† *Nautilus* was powered by a pressurised water reactor, which was later developed into the first land-based pressurised water reactor in Shippingport in Pennsylvania, which was commissioned in 1957. It was primarily the major companies General Electric

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* Back pressure turbines are used when residual heat (as in Ågesta) is used for heating; the steam then leaves the turbine at a certain pressure.

† See 'The Sword of Damocles' regarding the advent of *Nautilus*.

and Westinghouse which had participated in the development work that led to ‘nuclear submarines’. General Electric had specialised in boiling water reactors (BWR) while Westinghouse produced pressurised water reactors (PWR).

Several countries have laid claim to the first nuclear power plant. We have seen Hilding Hagberg proudly pointing out that the RBMK reactor in Obninsk was already in operation in 1954. However, the American experimental reactor EBR-1 had already turned critical in Idaho in 1951 and is said to have conveyed power to a light bulb, although it can scarcely be counted as a nuclear power plant. However, on 17 July 1955, the experimental Borax-III boiling water reactor in Idaho supplied the small town of Arco with all of its electrical energy for two hours. In 1956, an electricity-producing graphite reactor was started in Calder Hall in the United Kingdom. And after the advent of Shippingport, the development progressed quickly. Both General Electric and Westinghouse were soon able to show that they had built functioning light water reactors to produce electrical power. At the end of the 1950s, the USA had both a BWR (Dresden) and PWRs (Indian Point and Yankee).

The Swedish power industry started to become more and more impatient. The fully Swedish project worked up by Atombolaget with the government’s support was uncertain. If you wanted to construct your own nuclear power plant, the easiest thing to do would be to purchase light water reactors from the USA since access to enriched uranium was no longer a problem. However, unlike the power industry, Swedish producer ASEA was not enthusiastic about the development. ASEA wanted to produce reactors itself - importation would constitute unwelcome competition. Not only that, the work with Ågesta and R4-Eve led to useful experience which could be beneficial in the future.

In 1959, the Nuclear Power Consortium made a decision in principle to locate a boiling water reactor in the town of Simpevarp, 25 km north-east of Oskarshamn. The reactor was originally intended to have an electrical output of 60 MW. The application for a concession was submitted on 9 May by AB Atomkraft and was based on a tender from General Electric.

The Nuclear Power Consortium’s decision aroused astonishment and criticism from many directions since it involved a departure from the official ‘Swedish line’ pushed by the government and Atombolaget. But Vattenfall now also had its doubts, which were shown by a CDL letter to the Atomic Energy Delegation in May in which the power industry proposed the importation of two reactors, i.e., another reactor in addition to that which the Nuclear Power Consortium had planned.

At the delegation’s meeting in August 1959, the head of Vattenfall’s Atomic Energy Agency, *Ingvar Wivstad* (1924–1999), criticised Atombolaget’s R4-Eve project. No type of reactor had yet been selected and in his opinion, the project was still purely a drawing board product. It was unrealistic to conceive that R4-Eve would be ready before 1969. However, the MD of Atombolaget Harry Brynielsson had no doubt that the project would be implemented by 1965.

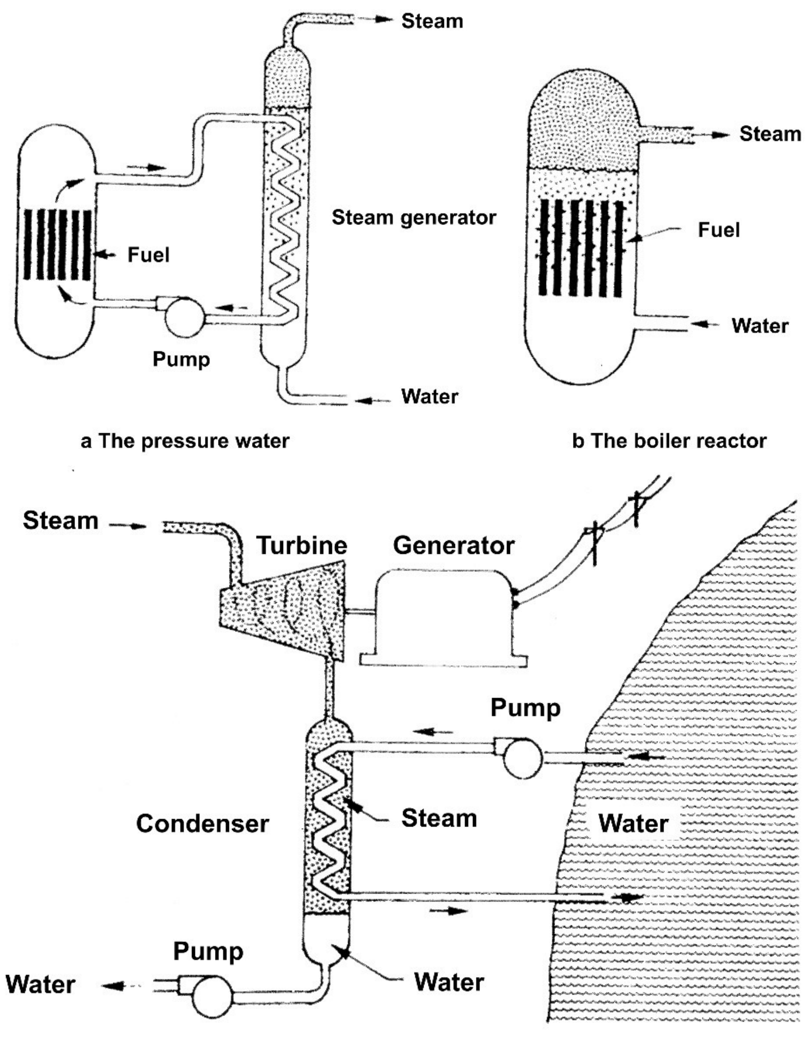
Vattenfall now took up discussions with the Nuclear Power Consortium regarding cooperation when it came to a possible reactor import. A Nuclear Power Consortium meeting in September 1959 discussed the matter of cooperation. Present at the time were *Marcus Wallenberg* (1899–1982), who was then Chair of the board of Skandinaviska Elverk, and *Tore Browaldh* (1917–2007), who was chair of Hammarforsen. Browaldh recommended cooperation and thought that independently importing a reactor would be aiming too high. Wallenberg on the other hand thought that an exclusive Nuclear Power Consortium project would be preferable. The competition between the Nuclear Power Consortium and Vattenfall which had been very beneficial would be lost if they were to merge. Vattenfall was also dependent on the government and on Atombolaget to some extent.

LIGHT WATER REACTORS

In light water reactors, ordinary water is used as a coolant and a moderator (to slow down the neutrons that are released at the time of nuclear fission so that they can be captured by the fuel and bring about new nuclear fissions). For this to be possible, the uranium must be slightly enriched in fissile uranium-235 so that this constitutes 2–4 % of the uranium instead of the natural 0.7 %. The most common types of light water reactor are boiling water reactors and pressurised water reactors.

In the pressurised water reactor, the water is heated in a primary circuit at high pressure and then passes the steam generators in which the water is evaporated from the overheated water in a secondary circuit. The steam then drives a turbine which in turn drives a generator to produce electricity.

In the boiling water reactor, the steam is used in the primary circuit to drive the turbine. In both cases, the steam is condensed in a heat exchanger where it is cooled by an outer water coolant circuit:



From Lindell & Löfveberg (Lindström, 1972), pp. 50–51.

In October 1959, Director General of Vattenfall Erik Grafström called a press conference and surprised the audience by saying that Vattenfall intended to depart from the Swedish line and erect a foreign light water reactor along with the private power industry.

The Nuclear Power Consortium decided to send the partner companies an enquiry as to whether they approved cooperation with Vattenfall. At the same time, the negotiations continued with Vattenfall, who accepted Simpevarp as the most suitable place for the reactor. However, it did not go as Grafström had intended; Vattenfall did not get to continue the cooperation. In October 1959, the Atomic Energy Delegation rejected Vattenfall's proposal for a merger with the Nuclear Power Consortium for reactor imports, and in November the government stated that no funds would be granted for Vattenfall to purchase a reactor. The Nuclear Power Consortium had to continue with its plans for Simpevarp under its own steam.

However, the Nuclear Power Consortium also negotiated with the City of Stockholm regarding a contribution towards the running of the Ågesta plant by purchasing electrical energy at overprice (corresponding to a contribution of 1 million Swedish kronor per year). This gave the Nuclear Power Consortium the option of instating personnel in the project organisation and assisting with the operation. This proved to be tempestuous. One of the technicians who was involved in the Ågesta project and who later became important to the Simpevarp project (i.e., the Oskarshamn reactors) was technical physicist *Nils Rydell*, who led the commissioning of Ågesta and later became project manager of the first Simpevarp reactor (Oskarshamn 1), and the very knowledgeable radiation protectionist *Bertil Mandahl*.

However, the Simpevarp project became cumbersome. The MD of the Nuclear Power Consortium Olle Gimstedt recalls (Gimsedt, 1985):

> The four first years of my time as MD at the start of the 1960s were a long, weary journey in the wilderness where a definitive decision on the Nuclear Power Consortium's first plant proved to be an illusion that did not materialise. The reasons were primarily at international level where, economically speaking, low fuel prices meant that atomic power was shooting a moving target. At the domestic level, this had the repercussion that it was not considered to be commercially justified until into the 1970s.

Meanwhile, Atombolaget was anxious about the development. Vattenfall was in no hurry since the oil price trends were falling and there was no urgent reason to introduce nuclear power. It had now been decided that R4-Eve would be erected at Marviken next to Bråviken. Marviken would also be the name of the project. There had been a delay in deciding on the type of reactor. Karl-Erik Larsson writes (Larsson, 1987):

> The situation as regards selecting the type for Marviken was a little chaotic. The first choice was a pressurised water reactor in a pressure tank. The decision was also to design the reactor so that refuelling could take place during operation. This design originates from a report carried out by ASEA and inspired by Ragnar Liljeblad. It is not clear whether this difficult complication was inspired by Liljeblad's substantial and genuine interest in Sweden's defence and his contacts with the Supreme Commander. Refuelling during operation is a typical design in plutonium-producing reactors. The output was initially stated as 105 MWe.*

The Atomic Energy Delegation, which reviewed the proposal, recommended in November 1961 that the nuclear power plant be erected as had been proposed. In 1962, Swedish Parliament decided to give a grant for the reactor but did not give an opinion on the type of reactor. The military believed that if a Swedish atomic bomb did actually become relevant, plutonium could be obtained from the Marviken reactor. However, weapons-grade plutonium required low burnup of the uranium fuel, i.e., frequent refuelling and substantial uranium consumption. However, for the military, the importation of uranium could lead to foreign control. It was therefore preferable to resume the Ranstad project. However, a report requested by Swedish Parliament within Atombolaget showed that Ranstad uranium would be 70 % more expensive than imported uranium. So, without laying the cards on the table, activation of the Ranstad project would appear to be inconceivable. To the surprise of many, the Dfa still recommended an extension with preparedness given as the reason. In 1960, Swedish Parliament decided that Ranstad would be fulfilled. The plant was ready for a test operation in 1965 and was run until 1969 with a total production of 213 tonnes of uranium. Following additional experiments, the decommissioning started in 1981. The production of uranium was never economically viable.

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* Many authors write 100 MWe when referring to an electrical output of 100 MW. However, quantities ought not to be described using special unit designations; instead, you define the quantity (in this case, the relevant output).

Extracting the plutonium formed from the spent reactor fuel requires a reprocessing plant. Reprocessing was possible for both the FOA in Ursvik and Atombolaget on a laboratory scale. However, a larger plant was needed for a greater scope. Where reprocessing is concerned, the military requirements are obviously the strictest. If you want plutonium for an atomic bomb, reprocessing has to take place. The civilian reprocessing requirements are less strict. The plutonium can certainly be used as reactor fuel, but the reprocessing is expensive and economic terms become crucial.

In 1960, the FOA's plutonium laboratory in Ursvik was commissioned until 1972. Around 50 people worked at the laboratory where there were 70 oxygen-free alpha boxes for plutonium handling, and Glenn Seaborg considered it to be the premier one in Europe.

In 1963, with a mixture of civilian and military motives, Atombolaget procured land for a reprocessing plant in Sannäs just west of Tanumshede in Bohuslän. An initial study of the plant (Hörm, 1962) had been carried out in 1962 by *Olof Hörmander* (1923–2010) and *Alf Larsson* (1922–). They reckoned the annual cost would be 21 million Swedish kronor. Based on the plutonium content that could be expected in the fuel from Marviken, they assumed a production of 270 kg of plutonium per year at a price of 78 kronor per gramme. However, it soon proved that the planned plant was too small to be profitable and the military motive was now not strong enough to get the plant carried through. Atombolaget defended its land purchase by stating that the land might be needed in the future.

In September 1960, the Nuclear Power Consortium received a concession for a nuclear power plant at Simpevarp. Of the tenders that had been received, the one submitted by the *Allis-Chalmers/Kockums/ Tekniska Byggnadsbyrån* (Technical Building Agency) was deemed to be the most beneficial. In June 1961, an order for investigation and consultation work was placed by Allis-Chalmers while the question of who would be awarded the order for the nuclear power plant was still open. In September 1962, the Nuclear Power Consortium was able to submit a detailed technical description to the Reactor Siting Committee for review.

Allis-Chalmers disappeared from the picture, however, when the company decided to discontinue its production of steam turbines. Instead, the Nuclear Power Consortium requested a tender from ASEA in 1963; in April, the latter submitted tenders for two alternatives, 20 MW and 60 MW. ASEA's production would be based on a licence agreement with General Electric. The Nuclear Power Consortium's board decided to recommend that the partner companies approve an order for the larger reactor.

ASEA was extremely interested. *Lars Leine* (1925–2014), who was later the technical head of Asea-Atom, wrote (according to Berg, 1985):

> The Simpevarp project is a very pressing assignment that would essentially increase our options of continuing our business in the atomic field and in particular protect us against the not improbable situation of the heavy water reactors not being able to contend with the competition.

It took some time within the Nuclear Power Consortium to convince enough partners to join the placement of an order. Some companies were concerned about getting themselves into hot water with Vattenfall. But there were also other objections. Mayor *Gösta Agrenius* (1914–2000) in Stockholm thought that 60 MW was insufficient and that they ought to invest in a larger plant instead. Assessing the economy of a nuclear power plant was also precarious.

However, in December 1963 all estimations regarding the economy were suddenly changed. The American power company Jersey Central (part of the General Public Utilities group) then ordered a 600-MW nuclear power plant called Oyster Creek from General Electric. The total cost of a 'ready-to-use' nuclear power plant was no more than USD 68 million, a surprisingly low amount. It now looked as though light water reactors could become economically viable as early as 1970 and not, as previously thought, until five years later.

Olle Gimstedt immediately informed the Nuclear Power Consortium's board and sent them a memorandum in February 1964 on the 'examination of possible alternatives'.

Around the end of 1962 and start of 1963, continued investigations within Atombolaget, ASEA and Vattenfall had led to the idea of a pressurised water reactor for Marviken being abandoned and they instead decided to go for a boiling water reactor with a higher electrical output of 200–400 MW. At

ASEA's proposal, the reactor would also be supplied with overheating of the saturated steam to achieve a higher degree of efficiency and a more competitive energy price. The Swedish freedom of action was considered to have been retained with regard to nuclear weapons, although this was something that the FOA questioned. They no longer thought that Marviken would be suitable as a reactor for producing plutonium. Another slight departure from the Swedish line was that the use of slightly enriched uranium was expected.

In spring 1964, Vattenfall started to want to withdraw from the project, the practical viability of which was in doubt. The Reactor Siting Committee, of which the painstaking Torsten Magnusson was Chair, disliked the overheating technology for safety reasons. The reactor would achieve a positive void coefficient, which is an expression for steam bubbles that have occurred in the cooling water increasing the output of the reactor. The opposite effect is what you want, i.e., a negative void coefficient which means that the reactor output is reduced if the cooling water boils.

In March 1964, the Nuclear Power Consortium's board decided to request a tender from ASEA for a nuclear power plant at Simpevarp with a light water boiler of at least 300 MW. The prerequisite for continuing to consider the tender was that 'it confirmed the supposition that it was capable of producing atomic power that was the economic equivalent of conventional condensate power'.

In June 1964, the board decided to write off the 60 MW project and concentrate fully on a nuclear power plant of at least 300 MW. They now started to call this the 'Oskarshamn Nuclear Power Plant' and continued to apply the name 'Simpevarp' to the now irrelevant 60-MW reactor.

Another UN Atoms for Peace conference was held in Geneva in early September 1964 alongside the World Power Conference in Lausanne. Important people from the companies who were interested in nuclear power had gathered there of course. While there, Olle Gimstedt and MD of Sydkraft *Sune Wetterlundh* (born in 1904) had a conversation with the MD of ASEA *Curt Nicolin* (1921–2006) in which Nicolin suggested for the first time that it could be possible for ASEA to produce a large boiling water reactor using its own construction, i.e., without a licence agreement with General Electric as per the previous intention.

Gimstedt and Wetterlundh were positive towards the idea but said that the crucial factors would be which costs would be incurred and what guarantees there would be. On 30 November 1964, ASEA submitted a preliminary tender for a ready-to-use delivery of a 400 MW nuclear power plant at a cost of 300 million Swedish kronor for the plant and 50 million kronor for the production of the fuel. The tender was considered by the Nuclear Power Consortium's board on 8 December. The board decided to request that all the consortia give Gimstedt power of attorney to negotiate with ASEA on the basis of the preliminary tender and also state how large a share of the power they were prepared to pay for.

Sven Bergquist (Berg, 1985) writes about the negotiations:

> They negotiated for days and nights. Charterplan transported the negotiation participants to and from the meetings. Curt Mileikowsky set an endurance record. To obtain a basis for negotiation he once went on a return flight between Sweden and Australia without staying overnight! At a night-time meeting, an ASEA lawyer had to remain on stand-by outside a telephone kiosk on Gotland to be there to answer any crucial legal questions.

On 17 March 1965, Swedish Parliament discussed the Marviken project, and the right motioned to wind it up. Liberal *Manne Ståhl* (1901–1976) was one of the leading critics and he made no secret of the fact that one of the main motives for the project was the production of military plutonium. He said (quoted from Gimstedt, 1985):

> There would be two reasons for now going with the Swedish line at Marviken as well. One reason was that we would use our natural uranium since we do not have the option of building uranium enrichment plants here in Sweden. That motive has presumably been weakened because the intention was for Marviken to go ahead with enriched uranium as fuel. The second motive would be to procure our own nuclear weapon within the foreseeable future. […]

> Under such circumstances, we would be able to procure our own heavy water and obtain natural uranium as a source of energy over which we had control and for which we would not be dependent on another world power. But, given the current political situation and also the technical conditions that we currently have, there is probably no-one in this chamber or in the Chancellery who would be of a mind to actually look at producing a Swedish atomic bomb. So, neither of these circumstances that are logical and conceivable to fulfil the Swedish line in Marviken actually exist. The minimum request could be the initiation of a general investigation into the whole of the Swedish nuclear power programme before pledging any more money to Marviken.

But the government's argument won and the Marviken project continued to receive funds. On 5 April 1965, ASEA submitted the definitive tender to the Nuclear Power Consortium. Hard negotiations were still ongoing. Bergquist writes:

> The final negotiation meeting before the board's decision in Stockholm was dramatic. The negotiation delegation from OKG [see below!] and ASEA sat throughout the night refining the agreement. Both parties wanted to make changes to the very last and not until the morning were the final arrangements made. Curt Mileikowsky of ASEA then attempted to squeeze additional improvements into the agreement, which was when the head of OKG Olle Gimstedt's patience snapped. He grew absolutely furious and ended the meeting. Mileikowsky went out onto the street with him to try and convince him to give way. Olle Gimstedt, usually a calm person, was fuming and threatened to call off the board meeting in the morning and the press conference that had already been announced. Five minutes to midnight, as it were, Mileikowsky rang home to Gimstedt and explained the last of the problems on the telephone and the board was able to approve the agreement.

So, on 14 July 1965, a new consortium agreement was ready and the articles of association of AB Atomkraftverk (AKV) were changed and the new name of the nuclear power plant company became Oskarshamnsverkets Kraftverk AB, abbreviated to OKAB. It was soon discovered that the abbreviation OKAB had already been registered by another company, so it was changed to the new letter combination OKG. Sune Wetterlundh was appointed as chair of the board and was asked to sign and hand over the request letter. With the new co-owners, the reactor's 400 MW of electrical power was distributed as follows:*

Sydkraft	100 MW
Voxnan (Skandinaviska Elverk)	100
Krångede	50
Stora Kopparberg	40
Svarthålsforsen (City of Stockholm)	30
Uddeholm	30
Gullspång	25
Bergslagens Gemensamma Kraftförvaltning (BGK)	25

The request letter was handed over to Curt Nicolin on the same day that OKAB's board had made its decision regarding the order. In the afternoon, Gimstedt and Wetterlundh along with Nicolin and Curt Mileikowsky, who had now advanced to being Deputy MD of ASEA, visited the Department of Trade and Industry to inform the Secretary of State *Gunnar von Sydow* (1911–1990) of the order. Olle Gimstedt says (Gimstedt, 1985):

> We waited with some anticipation as to how Gunnar von Sydow would react. The Department of Trade and Industry was after all the body which dealt with the State's

* The output actually ended up being 440 MW.

> atomic power matters. Although he obviously did realise the consequences that our decision would involve for the political debate on the government's nuclear programme, his response was an austere yet sincere 'Then I must congratulate you gentlemen.'
>
> On the way back from the Chancellery, Curt Nicolin invited everyone for a cup of coffee at an open-air café in Kungsträdgården. It was probably the best cup of coffee that we four had ever drunk.

In the evening, Mileikowsky invited us to dinner at Stallmästaregården. Everyone had reason to be pleased, particularly Mileikowsky and Curt Nicolin. Olle Gimstedt continues:

> It was ASEA's great technicians who were given the chance - people such as Lars Leine, Cnut Sundquist, Kåre Hannerz and Erland Tenerz to mention just a few. However, the crucial input was that of Curt Nicolin [...] who, through clever use of the company's resources and having the courage to take calculated risks at the crucial moment, won what was ASEA's biggest order over the years, Oskarshamn 1.
>
> [...]
>
> It had been a long, winding road to the order for Oskar 1 but the general direction had never changed. When battling against the odds, you have to zig-zag to reach the established objective, even if it would be more comfortable to edge away and go with the flow. Such a manoeuvre would have been involved in merging with Vattenfall in the R4-Eve project. It would simultaneously have removed the motive for ASEA to concentrate on the development of the BWR reactor. Had we not spent five years being obstinate with the smaller 60 MW project, OKG would probably not have become an independent power company and nor would ASEA have ended up being an independent reactor supplier.

On 17 March 1966, the Atomic Energy Delegation approved OKG's concession application and on 1 April, the government gave OKG permission to erect and run Oskarshamn 1. On 7 June 1966, County Governor Ivar Persson squeezed the trigger for the first volley of explosions for the building. The first phasing-in on the network from the completed station took place on 19 August 1971, but the delivery date for the station was 6 February 1972. Oskarshamn 1 was inaugurated on 18 May by King Gustaf VI Adolf.

When the work on Oskarshamn 1 had begun, OKG had acquired the village of Simpevarp on the Småland coast in the area known as the province of Tjust in northern Kalmar. The break-up of the town had already begun; two of the farms had been sold to the County Agricultural Board in a move from the countryside. OKG has commendably retained the old town area next to the nuclear power plant.

In March 1966, Erik Grafström reported new plans to Vattenfall; the idea was now definitely to concentrate on a light water reactor since Marviken was considered to have no future. The location for the reactor would be Värö on the west coast. However, the name was thought to be too difficult to use in international contexts and the plant was therefore already named Ringhals from the start. *Tage Nytén* (born in 1922) was appointed leader and chair of the project group. The first two reactors for this nuclear power plant were ordered in 1968, a boiling water reactor from ASEA and a pressurised water reactor from Westinghouse.

The decision made by OKG, ASEA and Vattenfall to concentrate on light water reactors was frowned upon by those who were advocates of the Swedish line and its lingering potential link to Swedish nuclear weapons. MD of Atombolaget Harry Brynielsson thought that Swedish industry's ability to independently pull off big light water projects was out of the question. He adhered to the energy policy decision that had been made in 1956 and also believed that Swedish heavy water reactors could become a profitable export product. Atombolaget had been in contact with India, Pakistan and Egypt. However, thought the critics, these countries were also interested in nuclear weapons.

The criticism of the plans at Marviken was growing. In 1965, the total cost of the project was calculated to be at least 400 million Swedish kronor. The safety was questioned. When the project was discussed at the 1965 Swedish parliamentary session, it was motioned for closure. However, the inherent inertia of the project in the form of prestige and money invested had now made it difficult to slow down the development. The government forced through a continuation in Swedish Parliament. ASEA was now the main supplier of the reactor. Despite the insight that it was the light water technology that would dominate, this was not a disadvantage to the company. ASEA gained valuable experience of boiling water reactors through the Marviken project.

In 1968, the decision was made for Sweden to affiliate itself to the UN's Non-Proliferation Treaty. Any thoughts of Swedish nuclear weapons were now definitely out of the window and thereby maybe also the primary motive for Marviken. On 27 May 1970, the King in Council accepted Atombolaget's proposal that the project be discontinued. By then, it had cost almost half a billion Swedish kronor. And yet it was still not money that had been completely thrown away. The industry was well prepared for the light water technology and had gained valuable experience. But it was hard-earned experience. And Atombolaget's time as central reactor developer had passed. Harry Brynielsson stepped down as MD (it is said that he did not want to have to move to Studsvik) and Atombolaget became fully owned by the State in 1969. Bo Aler took up the post of MD the following year. The production of fuel and the construction activities were transferred to the newly-formed company Asea-Atom, of which the State and ASEA each owned half. In 1982, Asea-Atom was bought out by ASEA.

When the reactor development had been left behind and another activity was in focus, Atombolaget's name was changed to Studsvik Energiteknik AB in 1978 and later to Studsvik AB. The remaining useable nuclear technology in Studsvik consists of reactors R2 and R2-0.

A comprehensive article in *Ny Teknik* (New Technology) in 1985 among others has said that the FOA's nuclear weapons research was 'unknown to the Swedish population and to the Swedish Parliament' and that the activities contravened express prohibitions, and that the military had an undue influence on the civilian nuclear power programme. However, my recollections in this chapter show that even if, as one might expect, *the way in which* the FOA's nuclear weapons research was run was kept secret, it was no secret that it *was* taking place. The numerous and intensive debates that were held, even in Swedish Parliament, on the nuclear weapons issue, bear witness to this.

The form of government that was in power before 1975 did not permit Swedish Parliament to use bans or orders to influence activities of administrative authorities. Swedish Parliament's only means of controlling such activity was legislation and terms for the use of allocated funds. The other State authority was the King and the government (the King in Council), which was the King's adviser. So, as far as the FOA was concerned, it was the government's instructions and appropriation directions which were to be followed once any funds had been allocated.

The fact that the Supreme Commander and FOA worked to make the civilian atomic energy programme as useful as possible for the military purposes was of course to be expected, and not reprehensible as long as both government and Swedish Parliament spoke about freedom of action with regard to Swedish nuclear weapons. Their possibility of controlling the development proved to be very limited, however. The final design of neither Ågesta nor Marviken had the option of refuelling during operation or tools to assist frequent refuelling.

'The ban' on construction research which Swedish Parliament is said to have issued in 1958 actually assisted in getting the government's bill regarding funds for protection research approved. The Minister of Defence said (according to Fors, 1985):

> Research that is directly aimed at the construction of nuclear weapons is obviously not what this is about. Such work will not take place without a decision having been made by Swedish Parliament. No proposal for this purpose is currently being put forward.

In the appropriation direction for the 1958/59 budget year, the government drew the FOA's attention to Swedish Parliament's terms for the use of the research grant. In this connection, these terms were changed into orders from the government.

The 1960 budget proposition proposed greater protection research for the FOA. The Minister for Defence emphasised the difficulty in maintaining a sharp boundary between protection research and construction research. Swedish Parliament approved the bill, whereupon the government issued directions for the protection research in the appropriation direction for the 1960/61 budget year and for the 1961/62 budget year. Directions in the appropriation direction concern only the budget year in question. The directions were not repeated for protection research after 1962, which formally meant that the ban on ‘research with a view to producing technical and economic bases for the production and testing of nuclear weapons’ ceased to apply. The FOA nonetheless still felt that its own research was bound by the previous directions. On the other hand, the Defence management, with *Torsten Rapp* (1905–1993) as the Supreme Commander from 1961, did not abandon the idea of Swedish nuclear weapons until the government’s 1966 budget proposition clearly said no to the question. The 1968 Defence bill then said that it was definitely not in Sweden’s interests to procure nuclear weapons.

9. THE RADIATION PROTECTION COMMITTEE

IN 1951, WHEN ROLF SIEVERT took on Bengt Hultqvist for research into ground radiation, it was for the purpose of finding out more about the doses of radiation to which people are exposed from natural sources of radiation. He had already begun his research into the dose of radiation from radium and potassium-40 in our bodies together with Bengt Håkansson. The resurrected International Radiation Protection Commission (ICRP) discussed new recommendations for dose limits. At the time, Sievert thought it was important to find out which doses of radiation the human body had been exposed to over the millennia and may have adapted itself to. The simultaneous international discussions regarding the risks to forthcoming generations owing to hereditary injuries from an anticipated general increase in the level of radiation made this knowledge even more important. This was made even clearer by Hermann Muller's warnings at the meeting arranged by Sievert and de Hevesy in Stockholm in 1952.

As early as 1951, Sievert and Hultqvist had ascertained that the level of radiation in a number of buildings made of lightweight concrete was far higher than it was in buildings made of other construction materials. This observation was published in *Acta radiologica* in 1952 and was noted by the mass media, which led Sievert to take the initiative for a larger investigation. In September 1953 he wrote:

> […] Since this circumstance has also been discovered elsewhere and has thus led to some publicity through some more and some less sensationalist articles in the daily press and in some weekly periodicals, it has been thought necessary to take up the issue of the impact of the radiation environment on the state of health for a comprehensive and objective review. The circumstance that in the not too distant future the use of atomic energy may conceivably increase the general level of radiation also prompts the question as to whether or not currently existing variations are of any significance from the health point of view.
>
> For the abovementioned reasons, the representatives of the Institute of Radiophysics have privately applied to the Building Committee, the Medical Board, the National Committee for Building Research and Stockholm University asking for instructions regarding suitable experts within construction technology, medicine and hygiene and statistics as well as requesting that the representative of the Institute of Radiophysics be on a committee which would look at the abovementioned questions. This committee has called itself 'the Committee for Investigating the Radiation Conditions in Homes'. The investigation will be financed through contributions from the construction materials industry and funds for scientific research.

As Secretary of the Committee, Sievert took on the then newly-employed Assistant Professor Arne Hedgran who had taken over Sven Benner's work assignments as head of 'control department C'. Hedgran remembers this (Hedgran, 2002):

> The study was set up as a broad technical medical investigation that would answer the question regarding whether the use of Ytong in homes could be approved from the health point of view. I was asked to be Secretary of the Committee as a complete newbie at the Institute of Radiophysics. This involved neither responsibility nor a great deal of work, but it was my first contact with the world of committees and I therefore have very distinct recollections, primarily from the start of the activity. The project was sanctioned

by the Medical Board and Medical Health Officer Rolf Bergman* was extremely dedicated. Experts involved were Åke Swensson† from Occupational Medicine and statistician Segerdahl‡. Bengt Hultqvist was employed full time in the project. Selected people from interesting home environments were asked to provide blood samples and were interviewed about tiredness, etc. A laboratory nurse was employed to calculate the ‘diffs’ in all blood samples so that comparable results could be obtained.

The Committee soon became known to everyone as ‘the Ytong Committee’ since the most radioactive material was the shale-based aerated concrete that was sold under the brand name Ytong® and was the subject of many a discussion. The pugnacious Professor *Hans Pettersson* (1888–1966) was interviewed on 23 May 1953 in *Göteborgs handels och sjöfartstidning* (Gothenburg’s Trade and Maritime News, GHT) and thought Sievert’s initiative was ‘completely superfluous’. Hans Pettersson was the person who had warned about the atomic bomb in GHT in summer 1939 before the idea had even crossed the mind of any government. He had worked with Marie Curie in Paris and for ten years at the Radium Institute in Vienna and had little respect for low doses of radiation. Sven Benner got a reply in on 30 May and criticised Pettersson’s exaggerated belief in the harmlessness of radiation. Pettersson came back with his own article on 15 June and asked what justified Benner’s contribution. He wrote the following about Benner: ‘He is evidently no great fan of the investigation in question. Does he not think that the easiest thing is to – well, write off the whole thing and let the large-scale project rest in peace?’

Benner responded to this in GHT on 2 July 1953. He explained that the discussion concerned two issues: the extent to which the homes investigation had a purpose and Hans Pettersson’s experiences of his work in Vienna. Regarding the first question he agreed with Pettersson that it would be best to write off the project. However, he wrote:

> Not so with the second point, the occupational risk involved with radium work. In his first article, Prof. Pettersson shows no indication of any understanding of this risk. He simply talks as though the large doses of radiation that he and others received without noticing while working at Vienna’s Radium Institute. I must react to such rash thinking and clearly warn everyone against taking the risks that Prof. Pettersson says he has taken.

Plant ecologist *Lars-Gunnar Romell* (1891–1981) was more far-seeing and awarded the title of Professor) at the Swedish Forest Research Institute in GHT on 16 July, who warned about the radon in indoor air. This was at a time when Sievert still saw hereditary injuries from gamma radiation from the construction material as the main risk.

Hjalmar Granholm (1900–1972), Professor of Building Technology at Chalmers University of Technology, was invited onto the Committee but withdrew. He thought, not without reason, that the health investigation would be difficult to interpret since timber houses have a better living environment than traditional houses.

The whole project was particularly sensitive since it was partly financed by the owners of Yxhult’s stonemasonry, which wanted to ensure that its product (Ytong), for which substantial use was predicted, would not harm anyone. Arne Hedgran has said that, looking back, he was concerned that generally speaking, the Committee did not seem to have considered that the result could give cause to ban use of the radioactive aerated concrete for homes. It had not been an unreasonably expensive alternative, but we should remember that where this matter was concerned, the Institute of Radiophysics and the Medical Board had no support from the 1941 Radiation Protection Act, which was purely an Act to protect workers.

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* *Rolf Bergman* (1897–1982), Medical Health Officer and head of the Medical Board’s healthcare office.

† *Åke Swensson* (1914–2005), Assistant Chief Physician at the Occupational Medicine Clinic at Karolinska Sjukhuset.

‡ *Carl-Otto Segerdahl* (1912–1972), Assistant Professor in insurance mathematics at Stockholm University.

And so the homes investigation continued with measurements of gamma radiation and the levels of radon in indoor air in homes within buildings made of different construction materials, and with blood samples from the residents and surveys on their state of health. On 13 October 1954, *Expressen* wrote:

> Material from extensive medical examinations of approx. 2 200 housewives in Central Sweden is currently being statistically processed at *Karolinska Sjukhuset's* Institute of Radiophysics. It is the start of the major investigation into the radioactivity in our homes and any impact it may have on our health which was started last spring. […]
>
> Most of the field work has […] done, says *fil.mag.* (MSc) Bengt Hultqvist at the Institute of Radiophysics and who is leading the investigation. Last spring, a special medical patrol travelled around Eskilstuna, Karlskoga, Tranås, Nässjö, Kalmar, Eksjö, Linköping, Norrköping, Kumla, Örebro, Katrineholm, Fagersta, Nyköping and some areas in Stockholm, namely Traneberg, Alvik and Ulvsunda, Johanneshov and southern Hammarby and Mälarhöjden.
>
> The most important examinations concerned the blood. They wanted to see the 'blood composition'. All housewives had lived in the same home for at least eight years so in theory, the radiation should have had time to have an effect. A note was also made of the number of days off sick per year for each housewife according to reports from the benefit societies. […]
>
> A trial study of approx. 150 housewives on Lidingö had already been completed in spring 1953. The reason for choosing housewives is that they are the ones who spend the most time in our homes. […]
>
> Thus far, very little is known about how large a dose a human can tolerate. The limit for harmful and harmless radiation is also very difficult to determine.

As expected, it was found that statistics could prove nothing. However, an interesting by-product was the connection between feelings of tiredness and the information on the length of time for which the housewives slept. Most of them felt tired irrespective of how long they slept (tiredness seemed to be a normal feeling), but those who said that they slept for eight hours a day felt the least tired. The share of tired people increased with both shorter and longer periods of sleep, in the former case presumably a result of a sleep deficit and in the latter case maybe because extreme tiredness requires more sleep. It is remarkable to read that there were still plenty of housewives in 1953.

The Nordic Society for Medical Radiology held its 17th Congress in Århus in 1951 with Carl Krebs as President. Chemotherapy for cancer was now starting to be mentioned more and more often. The Society's 18th Congress was held in Helsingfors on 1952 with *Sakari Mustakallio* (1899–1989) as President.

At the Society's 19th Congress, which was held in Oslo in 1954, physical analyses were reported regarding the 31 MeV betatron from Brown Boveri which had been installed at *Radiumhospitalet* in Norway in 1953. Johan Baarli described the results of an analysis of neutron radiation that had spread in the betatron treatment room. From Stockholm came Agnar Egmark's report about his scintigraph. Before 1950, scintillation detectors had been used to study the uptake of radioactive iodine in the thyroid gland, but when someone wanted to see the way in which the iodine was distributed, he was forced to take repeated measurements to analyse different parts of the organ using a collimator. This was a lengthy, manual procedure and they soon endeavoured to replace it with an automatic mobile detector to scan the organ. This led to the *scintigraph*, an instrument that was first constructed in the USA in 1950. Egmark's scintigraph did not move over the organ in a 'linear' manner but used a swaying motion whereby radiation was registered on a photographic film. Not long after that, Erik Berne and Ulf Jonsson in Gothenburg constructed a linear scintigraph which was sold all over the world by their company NUKAB. In 1956 at *Radiumhemmet*, Lars Jonsson and Inger Ragnhult constructed the world's first whole body scintigraph under the initiative of Lars-Gunnar Larsson, which was eventually produced by the company *LKB-Produkter*.

As I have described in chapter 3, while ordinary x-ray tubes with voltages of around 200 kV were used for the radiation treatment of deep-lying tumours, the dose of radiation to the skin constituted a major problem. It was greater than the dose received deep down partly because the intensity of radiation decreases in inverse proportion to the square at a distance and partly because the absorption of radiation reduced the intensity. One way of circumventing this difficulty was to divide the irradiation into several batches with different points of entry to avoid impacting the same area of skin each time. From that point on, it did not take long to come up with a treatment where the patient was rotated in the beam. Such treatment was found to be more beneficial to the patient than treatment with several different fields of irradiation. Early attempts using rotational irradiation were made by *F. Theseuer* 1937 and in Sweden, Professor *Lars Edling* (1878–1962) had a rotational irradiation device installed at the Jubilee Clinic in Lund in 1942, mainly for palliative (alleviating but not curative) treatment. However, curative treatments were also reported from Lund in 1951 by *Inge Gynning* (1914–1986).

In 1950 Sven Hultberg took the initiative for the installation of a rotational treatment device at *Radiumhemmet*. The nearest doctor in charge was Olov Dahl. The device was prepared in consultation with Benner and Thoraeus, but mainly in cooperation with Karl Johan Vikterlöf who, like Dahl, went on to defend his thesis on the experiences.

At the start of the 1950s, it was clear that it would be possible to replace radium with cobalt-60 from nuclear reactors as the source of radiation for tumour treatment. In chapter 3, I told of the way in which highly active cobalt sources also made distance treatment devices ('kilocurie devices') possible and were thereby an alternative to the different types of accelerator for radiation therapy which also started to be developed at the same time. In 1951, the world's first 'cobalt guns' of this type had been put to use in Canada. In my report on the study trip that I did in the same year, I had said that kilocurie devices had to be seen as one possible good alternative to the accelerators which at that time still had significant weaknesses.

The obvious thing was to consider whether or not cobalt was also a better alternative than radium for radiation therapy in the head and neck area. The two 'radium guns' at *Radiumhemmet* with 3 and 5 grammes of radium respectively led to a dose distribution that could be improved with the cobalt, mainly thanks to the fact that the source of radiation using cobalt-60 could be made significantly smaller than the large sources of radiation which consisted of a large number of encapsulated radium preparations. The smaller volume of the cobalt source would also make it possible to improve the very poor radiation protection. Finally, the high activity of the cobalt preparation would make it possible to reduce the long treatment times. The main thing was that this advantage meant that Kurt Lidén had already had a radium gun converted into a cobalt gun in Lund in 1952.

Rune Walstam and I therefore started to construct a 'cobalt gun'. The basic principle was the same as for the radium guns, which were based on a design of Sievert's previous devices which was improved by Benner. When the devices were not being used for treatment, the source of radiation was stored in a large protective container which allowed better protection for cobalt-60 than for radium since the radiation screen could come closer to the preparation and therefore be more effective at the same weight. When it came to treatment, the preparation was transported through a long arm to a small treatment head which also provided some radiation protection but had an opening for the beam towards the patient. You could easily insert different cones into this opening which delimited the beam as required.

We were anxious for the transport mechanism to function safely and therefore got a model to transport a metal cylinder up and down thousands of times to test its reliability. The final device was produced by *Hemlins Mekaniska Verkstad*, Sievert's favourite workshop, and the first example was commissioned at *Radiumhemmet* in December 1954. The source of radiation consisted of 8.5 curies of cobalt-60 in the form of a cylinder that was 6 mm in height and diameter, encapsulated in a slightly larger metal cylinder equipped with a fastener. The arrival of the source of radiation was a little bit risky. It came to Stadsgården in Stockholm by boat from England. It lay in a central aperture of a large metal cylinder which provided protection against radiation with said cylinder in a large, wooden packing case. When Rune Walstam and I came to Stadsgården to make sure that the cobalt source really had turned up, we found the packing case on the quay with a few dockworkers sitting on the case despite the signs warning of radiation. We told them to move, which they reluctantly did. 'Radiation is beneficial,' they protested, 'it cures cancer!'

When the packing case had been transported to the Institute of Radiophysics it was placed outside the emanation laboratory until we had taken the cobalt preparation out of the protective cylinder. It lay at the bottom of the cylindrical hole and was fastened by a string which went up through the hole and was intended to lift up the preparation. However, when we tried to do so, the string broke - maybe it had been destroyed by radiation. It was tricky using mirrors to look down into the hole and to try to fish out the preparation, but we eventually succeeded.

With the smaller dimensions of the source of radiation, it was possible to give the beam a smaller penumbra so it was therefore more sharply delimited than the diffuse field from the large source of radium. We thought that this ought to work in our favour, but this refinement confused the doctors. With the diffuse beam, the way in which the radiation was directed had been less precise. With the more sharply delimited beam, the doctors were forced to look at which of the areas would be irradiated. It was not tenable to say ‘it’s less likely that there are any tumour cells here so we can get away with a smaller dose!’, but that was precisely what a few doctors did persuade themselves to think. At the outset, we were therefore obliged, against better judgement, to use filters to deliberately make the beam from the cobalt preparation more diffuse so that it was similar to the beam that the doctors were used to from the radium guns.

Our cobalt gun was later produced by Elema-Järnh and sold to several clinics in the 1960s, including *Sahlgrenska sjukhuset* in Gothenburg, Umeå general hospital, *Akademiska sjukhuset* in Uppsala and the Regional Hospital in Örebro. Another one was installed at *Radiumhemmet* in 1956. Abroad it was sold to Chicago and India.

In summer 1955, Elis Berven, who was Chair of both the Cancer Society in Stockholm and King Gustaf V’s Jubilee Fund, wanted to ascertain which new radiation treatment devices were the best to invest in. He therefore proposed a meeting between the heads of the Jubilee Clinics - Bertil Ebenius in Lund, Magnus Strandqvist in Gothenburg and Sven Hultberg in Stockholm. In order to obtain physical expertise, he also asked Rolf Sievert to come. He also invited me to participate in the meeting, bearing in mind my study trips to the USA and Europe looking into high voltage device. At Ebenius’ suggestion, it was decided that the meeting would be held at his summer cottage on Stickelön in the Blekinge archipelago outside Ronneby.

I decided to drive down to Blekinge. When Sievert heard this, he wanted to come with me and proposed that I pick him up *en route* as he would then be at his country house, Tvartorp, near Rejmyre in Östergötland. Since it was a detour, he suggested that I spent the night there.

Tvartorp is a country mansion consisting of a main building and two detached wings. Sievert spent much of his time here but it was his wife, Astrid, who was responsible for the practical care of the mansion. Sievert, who liked playing the organ, had also had a small chapel with an organ built there.

At the time, I had a Volkswagen. When Sievert found out that my car was so small, he shook his head. ‘You’ll have to drive carefully with that small car,’ he said, ‘no more than forty!’ I was also wondering how Sievert’s corpulent personage would fit into the car, but the space in the front seat proved to be larger than you would have imagined. We had not been travelling for long – and I dutifully drove at forty kilometres an hour, the road being narrow and winding to start with – when Sievert started to become impatient. ‘Can’t you drive a bit faster?’ he asked. I gradually increased my speed but Sievert was not satisfied. When I drove at one hundred and ten, which was the car’s top speed, he wondered why I had got myself such a slow car. I remembered how three years previously I had driven Harold Gray from Skokloster in the Opel Kapitän that I had at the time. He had asked me in his usual gentle way to perhaps not drive quite so fast. Imagine, Sievert and Gray - there are probably not many people who have given lifts to both of these sizeable gentlemen who have given their names to units of radiation dose.

The meeting on Stickelön was productive and the discussion produced a good basis for forthcoming decisions. While we were taking a dip, Berven showed a blotch on his skin. The other radiologists were alarmed. ‘If it had been one of your own patients,’ they said, ‘you would have given him a thorough telling off for not having sought help before.’ Berven had cancer, maybe from all the radiation he had been exposed to, but he survived for another eleven years.

In October 1955, a Congress of Radiology was to be held in Munich, and Rune Walstam and I had plans to participate with a lecture about our cobalt gun. In documents that I saved from that time I find the following account which I wrote down before the journey. It concerns banalities but may still be typical enough of the time to be interesting to reproduce, and describes our relations with the authoritative gentlemen Berven and Sievert. I wrote verbatim (completed on 29 September at 20.30):

The German X Ray Society is celebrating its 50th anniversary with a Congress in Munich. A technical exhibition is also being held there. There, Elema is intending to show one of the Lindell-Walstam cobalt devices to introduce it to the German market.

Already last spring, Dir. Weber at Elema proposed that one of us travel to Munich to give a lecture on the gun. We therefore both decided to go by car and make it a holiday trip, and wondered whether Elema could make a contribution to the cost of the trip, which Weber said might be possible if necessary.

At the million volt conference on Ebenius' Stickelö at the end of July (24th –25th), I told Hultberg of our plans to travel and said that if we did this we also intended to visit Heidelberg to look at the Siemens betatron at Czerny hospital. Later during the visit to Stickelö, Berven wondered whether perhaps someone like Dahl could give an account of *Radiumhemmet's* rotational therapy experiences at the Munich Congress. Hultberg embraced the proposal, and during my holiday in August it was agreed that Dahl and Vikterlöf would speak about rotation and Walstam and I about the cobalt. Hultberg wrote a letter to Professor Kohler* and told us all. He also thought, as he had already told me on Stickelön, that we ought to apply for a travel grant from one of the usual funds such as the Cancer Society. It was decided that Dahl and Vikterlöf would make a joint application and Walstam and I would make one.

Under these circumstances, we decided to forget the idea of taking the car, particularly as Siemens had promised to arrange a viewing of both the factory in Erlangen (where the 1 000 curie ^{60}Co device was now being produced for *Radiumhemmet*) and the one in Heidelberg. It would be preferable if we all travelled in a group. Our plan was first of all to fly to Hamburg and from there to Munich, but following Hultberg's suggestion we ordered flight tickets all the way to Munich from Stockholm. […]

Hultberg had been told that the State's travel allowance in Germany was 40 Swedish kronor per day and suggested we ask for 50 for 11 days. The total cost for 11 days was 1 500 per person. When Hultberg mentioned this to Berven the day before, Berven was shocked (a) that so many would be travelling: surely one within each specialist area would suffice, (b) that the journey would take the form of a flight, and (c) the amount of the allowance.

When Walstam and I had finished our draft, Walstam took it to Sievert to hear what he had to say. Sievert (who may have already spoken to Berven) recoiled because (a) so many would be travelling, (b) the journey would take the form of a flight, and (c) of the amount of the allowance.

'Isn't the allowance for Germany 45?' he asked W.

'Up to and including 40, no more,' said W.

'The Cancer Society can't give the accountants a good reason for paying 50 and you should change it to 40 so it doesn't look out of place.'

Sievert then called Berven and thereafter told Walstam that Berven had said that the Cancer Society's money had run out for the moment and that B thought it would make 'a good impression' on the Society if W, V and I made a joint application for a grant to travel in a 3rd class sleeper.

When I heard this I rang Sievert and said that of course we did not want to ask for more than the State's going rate for the allowance but that our travel plans had been finally arranged and that we intended to fly irrespective of what we received from the Cancer Society.

Bearing in mind the grant options, it was a bold statement but it hit home.

'We would all like to travel together,' said I.

'Well,' said Sievert, 'what I maintain is that under no circumstances must you travel in worse conditions than the doctors!'

I then went to Berven (for whom I still had a task to perform – I was to drive him to see The Svedberg in Uppsala the following day). B mentioned the trip to Munich and

* Professor *Albert Kohler* (1890–1960) was President of the 1955 German Congress.

> thought that rather a lot of us were travelling. I said I thought we all had a reason to travel, whereupon B beat a retreat.
>
> ‘So, you’re all going to be speaking. I didn’t know that. It’s quite another matter if you’re going to be giving a lecture. I thought you just wanted to travel down to see what was happening at the Congress.’
>
> When I left, Berven said: ‘The Cancer Society certainly has exceeded its payments, but not for the first time, so doubtless something can also be done on this occasion.’
>
> Bearing in mind that it could be Sievert who was upsetting the apple cart, although he blamed Berven, I decided to strike while the iron was hot and ‘hit him with what Berven had said’. I rang him up and explained that Berven had made a mistake and now had no objections to our travel allowance applications.
>
> ‘Oh,’ said S smoothly, ‘in that case, he’s changed his mind since he spoke to me on the telephone,’ (reason and impact already understood).
>
> ‘And the expenses,’ I said, ‘are not 40 as you thought. The State’s travel regulations say 40 excluding hotel expenses.’
>
> ‘Of course,’ said Sievert, undeterred and continuing with childlike innocence, ‘but I presume you’d included those as well, hadn’t you?’
>
> ‘No, the 50 included everything in our eyes.’
>
> ‘Well, that’s another matter; I didn’t understand that at all. It makes a big difference; hotel rooms cost you 14–15 *Deutschmark* don’t they?’
>
> ‘Well, not quite that much, but the difference is at least more than 10 crowns.’
>
> ‘Now I don’t quite understand you.’
>
> ‘Excluding expenses was 40 and we had asked for 50 crowns.’
>
> ‘You’d asked for 50 crowns? I see - I thought you’d asked for 50 Deutschmark! 40 crowns, Of course you couldn’t manage on that. In that case, 50 crowns isn’t enough either!’
>
> ‘No, we can manage fine with that.’
>
> ‘Well, that was a definite misunderstanding. It’s a good job you wrote from the start!

My task to drive Elis Berven to Uppsala involved a visit to The Svedberg at the Gustaf Werner Institute* since proton radiation from the Institute’s cyclotron was now going to start being used for radiation treatment. On 30 September 1955, *Svenska Dagbladet* wrote:

> The biological proton radiation experiments previously heralded in *Svenska Dagbladet* will begin at the Gustaf Werner Institute for Nuclear Chemistry in the near future. As well as being the cyclotron plant in Berkeley, the Werner Institute is the only place in the world to boast adequate technical resources for this type of experiment, and the device which is now largely finally prepared was shown to representatives of the Cancer Society on the Thursday.
>
> On the day, the Society was represented by Professors Rolf Sievert and Elis Berven, both of whom appeared to be very interested in the possibilities of effective cancer therapy which the experiments may be able to provide.

Proton radiation from the Werner Institute’s synchrocyclotron was subsequently used for radiation therapy with stereotactic location and direction methods. Stereotactic operations had previously been used in brain surgery and meant that the patient’s head was fixed in a frame which, when locating tumours or other pathological changes using x rays, defined coordinates. The frame could then also be used as a support for surgical instruments, which allowed great accuracy during the operation.

In Lund, neurosurgeon *Lars Leksell* (1907–1986) had developed stereotactic methods for precision treatment using x rays. Physicist *Börje Larsson* (1931–1988), an inventive, energetic and very popular

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* The Gustaf Werner Institute (GWI) was founded in 1949 by The Svedberg for research within nuclear physics, nuclear chemistry, biology and medicine. The Institute got its name because its primary research instrument, a synchrocyclotron that was commissioned in 1951, had been donated by textile magnate *Gustaf Werner* (1859–1948). The GWI was superseded at the end of 1986 and start of 1987 by the Institute for Radiation Sciences and The Svedberg Laboratory.

man who worked at the Werner Institute, contacted Leksell and wondered whether or not the proton radiation from the cyclotron could be particularly suitable for stereotactic radiation treatment. This saw the start of cooperation between Larsson and Leksell who did ‘bloodless surgery’ using the proton radiation. The experiments were successful but the use was inhibited by those contact difficulties that arise when medical research and development take place outside the actual hospital environment. This risk was something that Berven and Sievert had already warned The Svedberg about when they visited him in September 1955.

Leksell therefore started to look for other ways around this and had the idea of stereotactic irradiation using gamma radiation from a large number of gamma-emitting preparations in directed channels around the patient’s head, a device that Leksell called the ‘radiation knife’. Börje Larsson and Kurt Lidén were given the task of constructing such a device and the intention was to erect it at *Karolinska Sjukhuset’s* neurological clinic where Leksell had been Professor of Neurosurgery since 1960. The device was produced at the Motala workshop and was ready in 1966.

On 15 October 1955, I flew with Walstam, Vikterlöf and Dahl to Munich and the German X ray Congress. It was the first time I had flown and the others were not exactly used to it either. The journey went well, however. At night, two days later, I wrote the following in the hotel room:

> Hello to the future!
> Are you interested in an atmospheric depiction at this moment? [Here] I am, currently lying in the Sonnenhof hotel in nothing but my underpants, writing this, while the monotonous drone of Dahl’s morning lecture is penetrating down through the double floor from the room above. This morning, Walstam and I were at Hellabrunn and saw loads of animals of all shapes and sizes. Siemens invited us Swedes to lunch at 13:00 at *Vier Jahreszeiten* with Messrs. Gellinek[*], Dax[†] and Wachsmann as diplomats. Hultberg was perhaps a little too friendly to be company-neutral but the atmosphere was natural. Yesterday, after Berven had received the Albers-Schönberg medal and the exhibition was opened, relations between him and Hultberg were surly. Hultberg thought Berven did too much when it came to accommodating Brown Boveri when Wideröe had Berven photographed in their new betatron assembly. Berven told Hultberg that the National Society had not granted the Jubilee Clinics the funds for which they had applied for 1000-curie devices. Yet Hultberg does have money; Strandqvist and Ebenius will lose out. All sorts of quarrels!

Director of the Swedish company Elema, Gustav Weber, convinced Rune Walstam and me to come to an informal meeting with some representatives of Siemens to answer questions about our little cobalt gun. We were shocked when we found a good 50 people seated in rows awaiting a lecture from us in German, and we were forced to improvise in broken German - an embarrassing experience.

After the Congress, we (Hultberg, Dahl, Vikterlöf, Walstam and I) made the promised car journey with a talkative driver from Siemens to Erlangen and Heidelberg, where we got to see the Siemens betatron in use. In Heidelberg, we had an uncomfortable yet comical experience when Professor *Joseph Becker* (1905–1983), head of the University’s radiotherapy clinic, was to show us how a packet of radioactive fluid should be applied to the urinary bladder of a patient. Becker, who was head of a big clinic in Germany at the time (it could equally well have been in Stockholm), emanated authority and respect that had the personnel trembling. The poor House Officer who was to perform the application was obviously nervous in the presence of the Professor and was unsuccessful. A big, powerful Chief Physician who accompanied Becker grew impatient and pushed forward to show how it was meant to be done. However, it was more difficult than he had imagined and it was awkward to see how even this powerful man shrunk before the Professor’s critical glances. In the end, the Professor himself took over, but he also failed and left the patient to his subordinates while he, slightly embarrassed, continued to accompany us.

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[*] Master of Science *Wolf Gellinek*, then board deputy at *Siemens-Reiniger-Werke* in Erlangen.
[†] Dr. of Laws *Paul Dax*, then Director and responsible for exports at *Siemens-Reiniger-Werke*.

In response to the new American openness that had started with the 'Atoms for Peace' conference in Geneva in August 1955, and thanks to Sievert's good contacts with Dr. Dorolle of WHO, a first European WHO course in radiation protection was held at Sievert's institution in November 1955. Lecturers were *Elda Anderson* (1900–1961) and *Myron Fair* from the Oak Ridge National Laboratory. The course was set up by Elda Anderson and Karl Morgan and contained a great deal that had previously been secret. The participants included many who had received or would receive important information within the field, such as Sven Benner, Rolf Björnerstedt, Per Grande, Arne Hedgran, Kristian Koren, Börje Larsson, Kurt Lidén, *Walter Minder* and *Walter Seelentag*. I had also been accepted as a participant.

Per Grande and Kristian Koren were both Norwegians. Koren, whom I met for the first time at Rønne-Nielsen's home during the Congress of Radiology in Copenhagen in 1953, would be taking over the responsibility for radiation protection in Norway after Moxnes. In spite of his nationality, Grande would soon be given the equivalent position in Denmark but was still a medical physicist at *Radiumhospitalet* in Oslo. Our course material consisted of thick, stencilled compendia that had been used in the training at Oak Ridge.

The course was intended for radiation protection physicists and the number of participants was limited. The participant from the FOA was Rolf Björnerstedt, but the energetic FOA medic Arne Nelson also wanted to take part and succeeded in convincing Sievert to let him sit in as an observer but not as a course participant. When I came to get my compendia, to my disappointment I found that there were none left. It turned out that the enterprising Nelson had commandeered my copies. When I tried to recover them, his opinion was that I could share a set of compendia with Arne Hedgran. My fury over this prank made itself so strongly felt that the surprised Nelson was alarmed and was decent enough to hand back the compendia, whereupon we remained the best of friends. I am mentioning this incident purely as a good example of Arne Nelson's inexhaustible initiative.

Elda Anderson was responsible for the training at Oak Ridge. The training was a task to which she devoted herself with great ambition. Her obituary in *Health Physics* said that 'she had an intense interest in people and was never too busy or too preoccupied with her many labors and recreations to make a new friend. She was, in both her professional and social life, an example and an inspiration for the many who were fortunate enough to know her well. Possessed of "an incredible zest for life" is the way one of her closest associates describes her'.

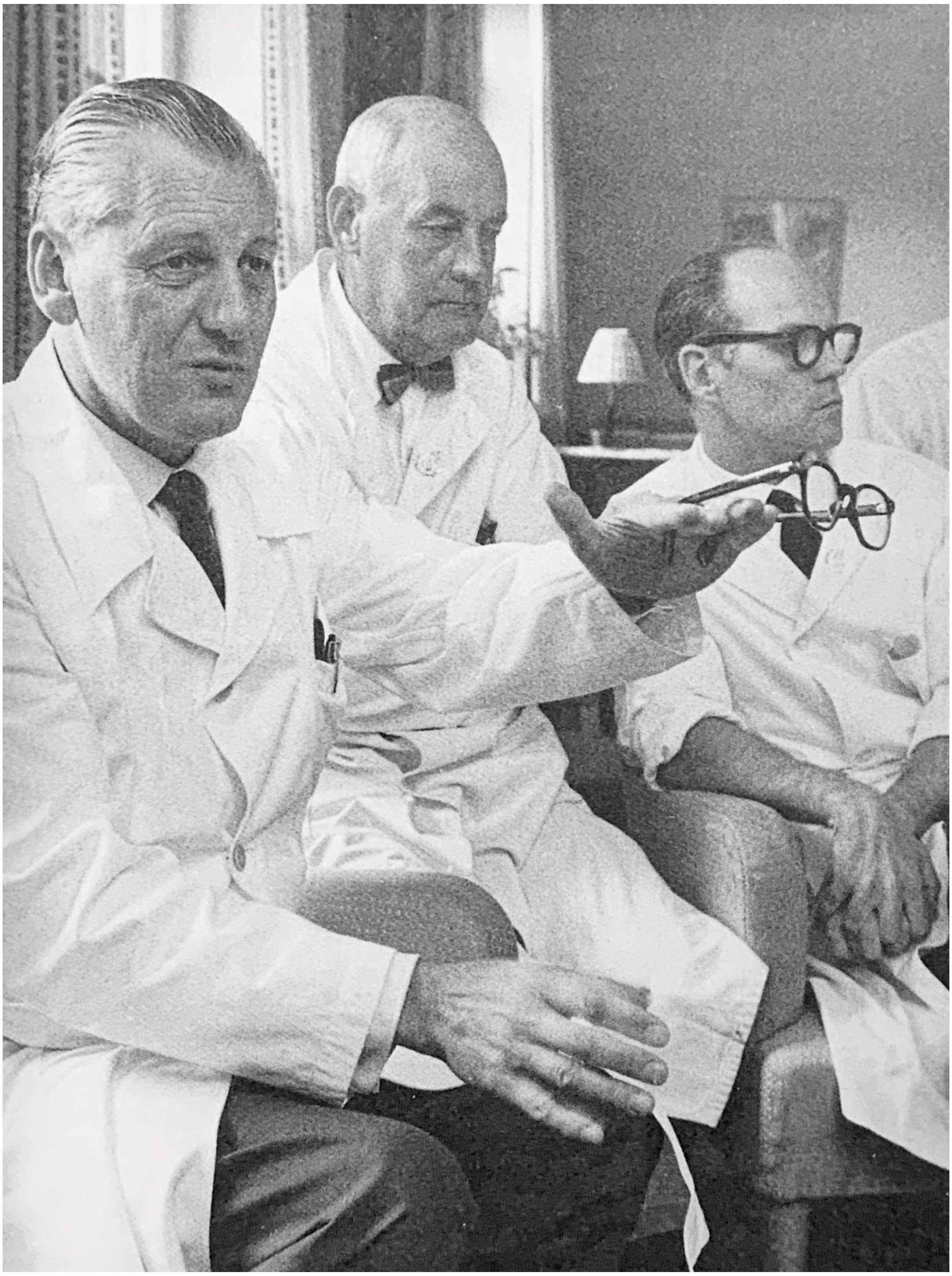
Myron Fair was responsible for most of the lectures and demonstrations under Elda's supervision. He spoke slowly in the clearest English that the course participants had ever heard, and explained that this was because, as an immigrant to the USA, he had been forced to speak like that to his parents who had difficulty learning English.

The WHO course was a success and the additional bonus was the contacts that were established between the participants from different parts of Europe - to Sievert's satisfaction.

In March 1956, Rune Walstam and I applied for a grant from the Cancer Society for a trip to England to study telegamma therapy, mainly the conditions for using caesium-137 as a source of radiation. We flew to London on 7 May and visited Professor Mayneord at the Institute for Cancer Research at the Royal Marsden Hospital on the Fulham Road for the first time.

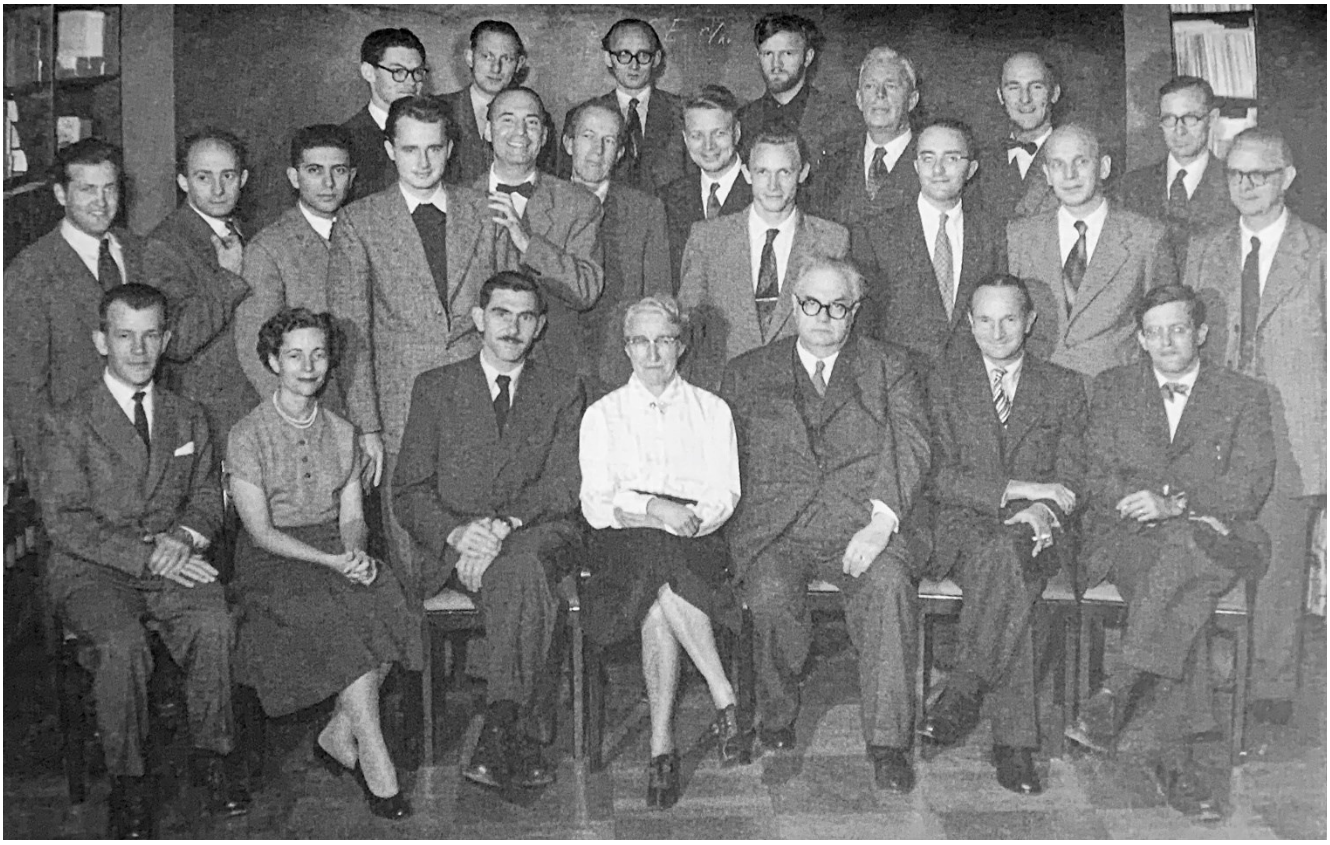
Mayneord received us in his workroom along with physicist Bernard Wheatley. We had previously met Mayneord when he visited Sievert in Stockholm and knew that he was a clever and inventive scientist, if a somewhat eccentric person. He took pains to point out that the red sports car outside the entrance was his. 'People need to be able to see when the Professor is arriving!' he said, partly as a joke and partly with self-assurance.

The visit to Mayneord was more a courtesy visit than a study visit. The best thing to come out of it was probably that he corrected our very Swedish pronunciation of 'caesium', something which he found comical. Our next visit was to Hammersmith Hospital where George Newbery showed us around. We then travelled by train and taxi to Harwell, which was still producing radionuclides on a commercial basis (this was not taken over by the Radiochemical Centre in Amersham until 1959). At this time, Harwell's isotope department was headed by Dr. *Henry Seligman*, but our contact person was Dr. *W. S. Eastwood*, who had pre-informed us that it would not be possible to deliver either cobalt-60 or caesium-137 to us in the forthcoming year. That did not matter, we had replied, since our plans were for further into the future and we wanted to study the possibility of eventually producing a telegamma device with caesium-137.



Sven Hultberg, Robert Thoraeus and Olof Dahl in conference at *Radiumhemmet* in 1959.
Photo: Bo Holst (*Stockholms-Tidningen*)

From Harwell we continued to Cambridge where we would visit Joe Mitchell at the radiation treatment centre at Addenbrooke's Hospital. There, we saw a demonstration of a kilocurie device along with some American visitors, one of whom was a tall man with white hair and a cherub-like face. That was the first time we met Harold Wyckoff, who went on to become one of my most important cooperation partners.



The participants in the WHO radiation protection course at the Institute of Radiophysics in November 1955. The people seated in the front row are: D.L.S. Teglbjaerg (Denmark), Ingrid Pinset (France), Walter Seelentag (Germany), Elda Anderson (course leader from Oak Ridge), Rolf Sievert, Walter Minder (Switzerland) and Josef Braun (Sweden). Standing in the middle row: Sten Hellström (Sweden), Jacques Bouquiaux (France), Giorgio Cortellessa (Italy), Sölve Hultberg (Sweden), Kristian Koren (Norway), Kurt Lidén (Sweden), Gudbrand Jensen (Norway), Søren Mehlsen (Denmark), Myron Fair (lecturer from Oak Ridge), Per Grande (Norway at the time) and Sven Benner (Sweden). Standing in the back row: Lars Wahlström (Sweden), Carl Gösta Rylander (Sweden), Bo Lindell (Sweden), Pall Theodorsson (Iceland), B.M. Woldringh (The Netherlands), Arne Hedgran (Sweden) and I.S. Eve (WHO). Rolf Björnerstedt, Börje Larsson and Sören Linde, all from Sweden, are missing from the photo. Photo: Unknown.

At our hotel, we were given another demonstration which marked the start of the IT age. In the lobby there was where we saw a TV operate for the first time …

Bengt Hultqvist's radiation measurements during the homes study obviously provided a good basis for a doctoral thesis but before this, a licentiate degree was required which Bengt thought he could enjoy the great privilege of preparing while employed by Sievert. Sievert also required such a degree from me, and I was less enthusiastic. To top it all, we were studying for the integrated written exams at the same time and, reluctantly, I had to read the rather advanced literature that Bengt had suggested to our examiner, Professor Erik Hulthén.

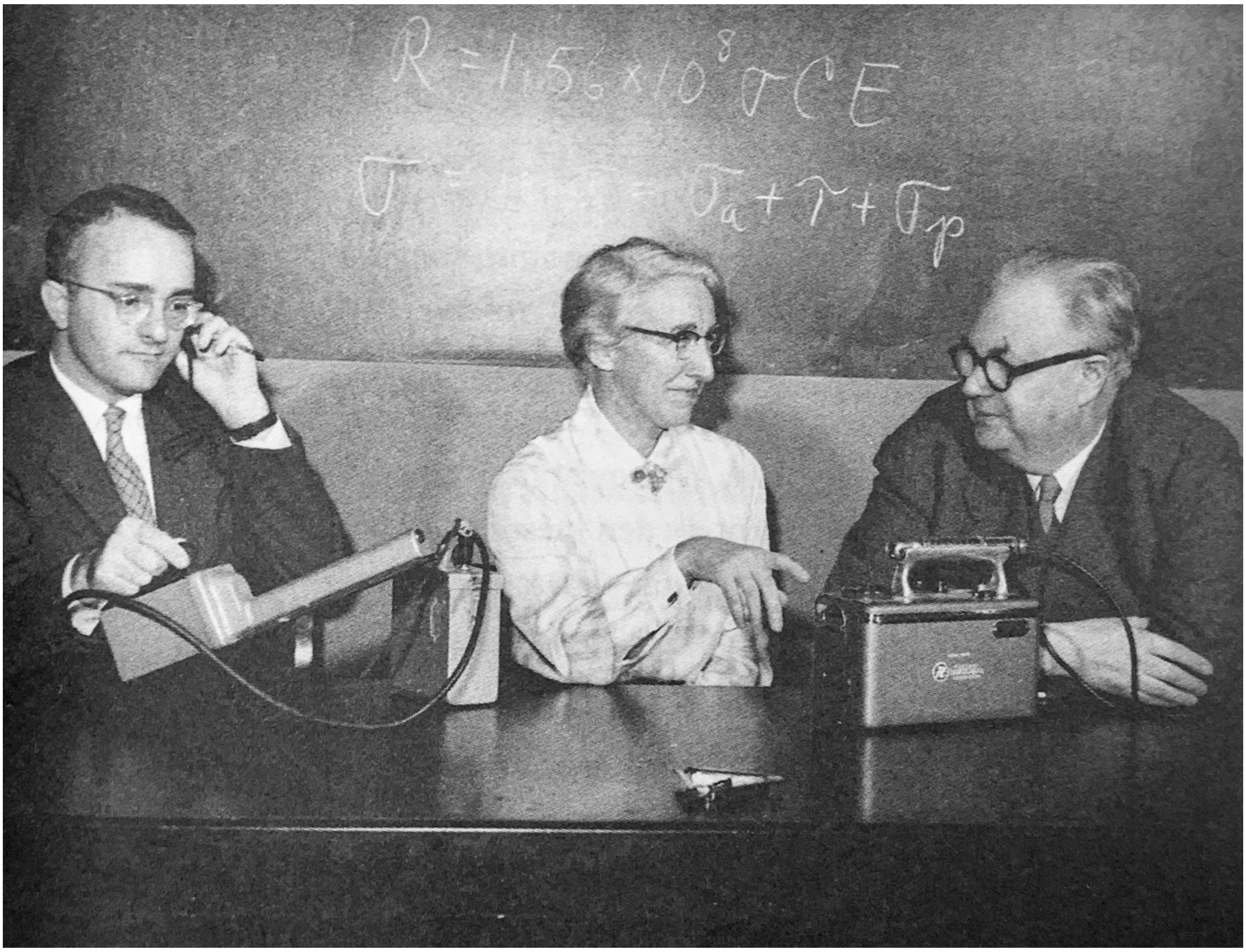
In April 1955, I had an essay on secondary x rays approved as a licentiate thesis and became Ph.Lic. in Physics at what was then Stockholm University in June of the same year, but I could not see a suitable subject for a doctoral thesis for me at the time.

On the other hand, Bengt Hultqvist had already defended his thesis in spring 1956 on the basis of his analyses, a very esteemed thesis which aroused substantial international interest and constituted the start of the awareness of the importance of natural radiation as a risk factor. The levels of radon and thoron (i.e., radon-220) indoors were unexpectedly high and a low level of air circulation meant that the dose to the lungs could be very high. However, Hultqvist found high levels of radon not only in homes containing construction elements made of the most radioactive aerated concrete (i.e., Ytong) but also in brick and timber houses where radon penetrates up from the ground. The problem was therefore seen primarily as a ventilation problem. There were still no international guidelines, no standards and no risk assessments for the radon. Ten years would pass before reason was found to warn people against radioactive construction material and high levels of radon in indoor air.

As far as I know, the results of the health study were never published. My question about this to Bengt Hultqvist was answered as follows:

> Regarding the current health study, I distinctly recall that it was never completed. As far as I remember, the doctors involved lost interest and disappeared off the scene. The health study would have been completed after I left [the Institute of] Radiophysics but I am fairly sure that no final report ever came out. It was probably too difficult to produce significant results.

However, there is an undated manuscript, seemingly from 1957, with *Ingrid Hävermark-Segerdahl* (1918–), C.-O. Segerdahl and Åke Swensson stated as the authors. I have not been able to find any details in the library archive of the manuscript having been published, and nor have I found any details in the Radiation Protection Institute's archive of any formal report on the results of the health study. The summary of the unpublished manuscript is therefore the closest that I can get to a final report. I therefore quote the following from the original English):



The lecturers on the WHO course in 1955, Myron Fair and Elda Anderson, discussing measurement instruments with Rolf Sievert. Photo: WHO

> A technical and medical study was carried out to shed light on the matter of whether the ionising radiation from construction materials may have a harmful effect on the health of those living in different types of home. The group studied in the medical health surveys consisted of more than two thousand women.
>
> In the technical study, the ionising radiation showed noteworthy relative differences between different types of home, depending on the construction material that had been

used. Radiation was at its lowest in timber houses, higher in brick houses and highest in houses made of alum shale-based lightweight concrete. See Hultqvist for details (1956).

> In the opinions of the women themselves, there were no differences in the general state of health between the groups who were allotted these different houses.
>
> A haematological study gave no sign of any effect on the blood-forming organs in the group studied.
>
> A study of the duration of morbidity as it has been registered by social insurance offices over a ten-year period gave no results to indicate any harmful effect owing to the ionising rays.
>
> In this study, no information was provided to indicate that ionising radiation from the construction materials used for the homes could have a negative impact on the health of the occupants.

It is interesting to see that the mass media interest in divining rods and 'terrestrial radiation' (from the German 'Erdstrahlen') in the late 1990s was as avid as it had been half a century before that. Sievert, who was always prepared to examine incongruities in the hope of finding something new and useful, was very critical. In June 1956 he had written the following in answer to an enquiry in a letter to a Professor at the University of Bergen:

> […] around 10 years ago I collected a whole lot of letters and essays about 'Erdstrahlen', particularly by German and Austrian authors. The reason for this was that, although then, like now, I thought the whole matter had been dealt with very amateurishly and effects of the type described were very unlikely, I still thought I should look at the literature. New and important phenomena may be behind even the most fantastical and outrageous articles and the main problem may be that people with lack of physical knowledge have pursued the matters. However, at said point in time, I definitely realised that the whole of this complexity of problems was in the hands of people who were not reliable enough and who were trying to make names for themselves or fulfil their own financial interests.
>
> A few years ago, I came across the same problem in that a highly esteemed forester in Finland believed he had found some areas where the vegetation was different from the surroundings, which he wanted to link with radiation phenomena. One of our leading administrators within the forest area in Sweden had also been interested in the Finnish studies in days gone by and wanted us to do studies here. I discussed the matter with Swedish physicists and chemists. They are of the same view as me, i.e., that the whole of this area is extremely dubious to say the least. One unfortunate thing, as I mentioned above, is that even very prominent people, albeit not experts, are 'believers' in these maintained radiation phenomena, but I have no hesitation at this current stage in advising dependable journals from taking them up for discussion. On the other hand, it could be seen as appropriate from several points of view for physicists and biologists to jointly undertake some sort of control study in places where phenomena have been thought to have been found to determine whether there actually is anything in this matter. […]

Sievert dealt with the 'Danger of Radiation' in a typescript of a lecture from 9 November 1957. Some of his statements are particularly interesting since they illustrate the knowledge situation and policy thinking in the mid-1950s:

> For those who look at the many discussions on the danger of radiation over the past few years from a general natural science and technical point of view, the richness of the flora of the different assessments must appear striking. […]
>
> Regarding the danger of radiation, there is every reason to ask ourselves 'what is truth?' The answer is simple: our rather incomplete knowledge of the effects of radiation on humans in small doses of radiation received under the many different circumstances that may now be relevant do not permit objective opinions regarding the size of the doses of radiation which can be allowed without serious risks to humans.
>
> […]

> It is evident that the worldwide danger of radiation already places it in a unique position. […] There certainly are chemical effects which are also multi-faceted, but the near enough unlimited possibilities of direct and indirect harmful effects to which radiation leads must be considered to be unique.
>
> […]
>
> Many think that all is well and good if we simply fix our maximum permitted doses at a low enough level that those with a very broad safety margin are below those that may reasonably lead to radiation injuries. This cannot be right since we would then be able to raise obstacles to the development within an important area without reasonable grounds. […] However, an endeavour to prevent obstacles to the development is not the only reason why we must be moderate in our safety requirements – we must also be so for psychological reasons. If we were to adopt the recently mentioned line of absolute safety there would, quite rightly, soon be a reaction to unnecessary caution and we could enter a period in which the too-rigorous radiation protection measures were more or less eliminated, which would mean jumping out of the frying pan into the fire.

The 1951 Radiation Protection Committee, which was to analyse the future organisation of radiation protection, did not make its statement (ref. Strå, 1956) until October 1956. The Committee put forward a proposal for a new Radiation Protection Act to replace the original one from 1941, which had been purely a workers' protection law. The Committee's summary now said:

> The purpose of the Radiation Protection Act must be to offer protection not only to people who do work involving ionising radiation and to patients who are being studied or treated with such radiation, but in principle to all people. To this end, the Act must in principle be applicable to and provide the option of supervising all types of radiation source which generate ionising radiation.
>
> On the other hand, the scope of the supervision must be adapted with regard to the risks that are linked with different types of radiation source and the change in the use thereof. The decision in this respect ought to fall to the supervisory authority which will be able to exempt sources of radiation from the supervision to the extent that this can be considered to take place without the risk of radiation injuries.

Regarding the supervisory authority, which had been the Medical Board thus far, the Committee said:

> As regards the organisation of the radiation protection control, the Committee proposes that the decision-making powers be transferred from the Medical Board to a State Radiation Protection Committee comprising representatives of the specialist areas relevant to the radiation protection issues. The Committee shall consist of five members. The head of the Institute of Radiophysics will of course be a member of the Committee. Other members will be appointed by the King in Council for a specific period, perhaps five years. Like the Chair, one of these will be experienced in administrative matters and one of the others will be a medical expert with experience within medical radiology, one a workers' protection expert and one an expert within nuclear physics or nuclear chemistry. The Committee should summon special expertise for the assessment of certain more specialist matters.

A special statement from member Matts Helde was added to the Committee's statement. During the study, Sievert and Helde had disagreed on several matters and the chemistry between them was not the best. Sievert was impatient because, despite being encouraged to do so, Helde did not write a doctoral thesis. Helde in turn thought that Sievert opposed him. Helde was a brooding type of person who found it easy to see difficulties. The antagonism went so far that Helde refused to go along when Sievert invited people for coffee and cakes every Wednesday in the long corridor to the high voltage hall. 'I'm not drinking that man's coffee!' said Helde, and sometimes when I visited Helde in his workroom he could be known to raise his voice, look up at the ceiling and say 'I know you've got a microphone somewhere!' However, Helde was a nice although verbose man who was very conscientious. He thought the Radiation Protection Committee's proposal paid too much attention to Sievert's unique position as someone who was in charge of countless assignments, which prevented some of these assignments from being carried out satisfactorily. But Sievert was particular about his empire and was not happy to give away anything. Looking back, you might say that Helde was right in much of his criticism of the Committee's proposal.

1957 was an eventful year for *Radiumhemmet*. That was when both the Siemens 15 MeV betatron and a kilocurie device with cobalt-60 as a source of radiation were installed. 'The million volt devices' were now starting to be reliable and high activities of cobalt-60 were becoming available. On the other hand, the premises were not completely satisfactory. The room in which the betatron was installed was not fully protected against radiation, which led to demands for restrictions regarding the use of the rooms above. The new devices increased the dose planning requirements. Because the energy-rich radiation gave the highest doses deeper than just the skin, the doctors who were used to looking at the skin reactions to help them decide how strong the irradiation could be lost their 'feeling' for what was optimum irradiation. There was obviously a need for expert medical physicists, and on 29 September 1957 Sievert wrote to Sven Benner and expressed his support for an initiative by Benner, Kurt Lidén and Lars-Eric Larsson to create a medical physics society.

The previous maximum quantum energy for radiation treatment at *Radiumhemmet* had been provided by General Electric's large and unwieldy x-ray therapy device called 'Maximar'. This had been purchased in the USA by Elis Berven, who had been impressed by the fact that it was said to be a '400 kVp' machine. Unfortunately, Berven did not understand that the lowercase 'p' stood for 'peak', which meant that the voltage was pulsating and the top voltage was only 400 kV. The average voltage was much lower so the spectral distribution of the x rays was not that impressive.

Berven had also not realised how difficult it would be to get the very large, bulky and heavy device up to the floor on which it would stand. They were forced to knock out a wall and had significant problems lifting the Maximar using the tools they had available.

In June 1957, the Nordic Society for Medical Radiology held its 21st Congress in Copenhagen with Professor Jens Nielsen as President. At the banquet, verses in Danish that had been written by *Piet Hein* (1905–1996) especially for the event were read out:

Lysets ny oktaver bag dets regnbubånd baerer tunge gaver til vort kunskabsfond. Hvor vi famled blindet, hvor kun mørket var, ligger gennemskinnet legmets dunkle kar.	The new octaves of light behind the rainbow band bring onerous gifts to our knowledge fund. Where we fumbled blindly, where only darkness reigned, lies, shone-through, said body's darkness contained

This time, the programme was very extensive. Dahl, Walstam and I reported the experiences of *Radiumhemmet's* little 'cobalt gun'. Magnus Strandqvist talked about an ingenious use of 'television-röntgen' in pendulum irradiation. Dahl and Vikterlöf spoke about the use of *Radiumhemmet's* pendulum device for 200 kilovolts of x rays. Lars-Eric Larsson spoke of doses of radiation to personnel and patients in modern röntgen diagnostics. Kurt Lidén, Nils Starfelt and Gunnar Hettinger presented a 'Scintillation spectrometric determination of the primary radiation's spectra from therapeutic and diagnostic tubes', a study which was very well received. Lars Jonsson and Lars-Gunnar Larsson described a scintigraph with substantial variation possibilities.

The Association's report from the Congress (Unné, 1984) reads: 'people were now starting to study the conditions of radiation very seriously and were soon able to show that *radiation protection was needed*!' They agreed that the position of the radiophysicists ought to be analysed. Emphasis was placed on the fact that the cooperation of the Nordic radiophysicists outside the Nordic Society for Medical Radiology did not involve any dissociation from the radiologists. It was finally established that the correct translation of the Association's name into English was the Northern Association of Medical Radiology.

In October 1957, Sievert telegraphed Dr. *Pierre Dorolle* in Geneva with congratulations on WHO's second radiation protection course, which was held in Mol in Belgium that year and Elda Anderson was still the lecturer. In November of the same year, the ILO held an expert meeting on radiation protection in Geneva where ICRP was represented by Professor Bugnard.

In November 1957, Lars Ehrenberg and Arne Hedgran published a paper on the possible impact of the temperature in the gonads on the risk of hereditary injuries. It was showed that warm underclothes tangibly increase the temperature of the gonads and could therefore lead to some level of risk. The paper – irreverently referred to as 'the pretentious underwear twaddle' – was taken by many to be a big joke but its intention was serious.

The authors had received help from men in a nudist colony where it had been possible to measure the temperature of the testicles after a longer period of not wearing clothes and following a corresponding period wearing clothes. The temperature difference was more than 3 °C, which could tangibly increase the mutation frequency. If this were believed to be a danger, thought the authors, it would call for a change of dress, and they made reference to the Scottish kilt.

In June 1958, the Northern Society for Medical Radiology held its 22nd Congress in Åbo with *Carl Wegelius*[*] (1905–1988) as President. The board held its meeting on the *S/S Norrtälje* while travelling between Stockholm and Åbo. The Association's anniversary publication (Unné, 1984) reads: 'From the board meeting, it can be noted that the "issue of the radiophysicists was now settled and that the radiophysicists were now members with the right to vote in the different countries' radiology associations".'

A central theme was the x-ray contrast agents and complications caused by them. Planigraphy and tomography constituted another important subject. Erik Poppe showed treatment results from the betatron in Oslo. Lidén, Hettinger and Starfelt continued giving accounts of their studies of x-ray spectra, now for scattered radiation in soft tissue.

In 1958, a pacemaker was implanted beneath the skin of a patient for the first time. The pacemaker is of interest to my story since there were innumerable discussions about whether it would be run on radioactive batteries (which proved to be unnecessary). A pacemaker is a device which sends electrical impulses to the heart, thereby encouraging the heart muscle to make regular contractions when the body does not provoke this in the normal way. The principle was already known at the start of the 1800s but was not put into practice until 1952 in the USA. At the time, the power source stood on a table outside the body and the patient always had to be connected to it. The possibility of implanting the power source in the body was fulfilled by doctors *Åke Senning* (1915–2000) and *Rune Elmqvist* (1906–1996). The latter was also a technical genius and a very clever designer, designing for example the first inkjet printer for medical purposes ('the piddlograph' was his own disrespectful name for it – the usual name was 'the mingograph'). The patient himself, *Arne Larsson*, said the following in a weekly magazine in May 1984, no fewer than 26 years after the first implant:

> Rune Elmqvist produced the first 'internal' pacemaker in his garage in 14 days. Since I am an electronics engineer myself, I saw that it was realistically possible and took the chance. The rib cage has to be opened during the operation, which took several hours. While I remained in the intensive care unit after the operation, it was discovered that the cables themselves had been damaged. That meant it was simply a matter of taking me down for another operation, when I was given my second pacemaker, the one that nowadays is described as being the first in the world. It was the eighteenth of October 1958.

As if that was not enough, Arne Larsson was interviewed by *Dagens Nyheter* on 26 May 2000 because it was his 85th birthday. He had gone through 27 pacemakers by then and was still healthy and alert. In 1966, they were still trying to produce pacemaker batteries that used radioactive substances as a source of energy. Professor Jan Rydberg said the following in an interview with 'Gothenburg's Trade and Maritime News' on 24 November:

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[*] From 1953–1960, Wegelius was Professor of Medical Radiology in Åbo but was active in Sweden after that.

> I reckon that within five years we will have progressed so far with radionuclear batteries that people will start inserting such batteries internally to assist heart activity in those who have heart disease. The batteries will be able to function for 10 years without being recharged. Such batteries are already used in satellite programmes.

But it was the ‘normal’ batteries that won and were given such a long shelf life that there was no need to resort to radioactive batteries.

On 5 December 1958, Sievert spoke before the UN General Assembly’s First Committee in his capacity as Swedish representative and Chair of UNSCEAR. He had begun to have doubts about the risks of very small doses of radiation and said:

> For many years, the geneticists have generally been of the opinion that [the number of] induced genetic mutations is directly proportional to the dose received, irrespective of the intensity or the dose rate of the radiation. However, studies published over the past few months are thought to cast doubt on the general validity of this presumption. The supposition that even with very small doses of radiation arbitrarily distributed over long periods of time – even over generations – there would still be a linear connection between dose and genetic effects is perhaps no longer justified. In that case and if – which is not unlikely – some somatic effects such as the occurrence of leukaemia are due to mutations in somatic cells, the importance of long-term irradiation from small quantities of radiocaesium and radiostrontium is uncertain and may also prove to be negligible.
>
> […]
>
> I imagine that the layperson reading the report from the Scientific Committee [i.e., UNSCEAR] cannot help but be surprised at the fact that, following sixty years’ experience of work with x rays and radioactive substances, there are still many gaps in our knowledge of the effects of small doses of radiation. It is even more remarkable since ionising radiation has been used mainly within medicine. However, there are several reasons for this lack of knowledge.
>
> One reason is that the interest in the period immediately following the discovery of x rays and radioactive substances was principally in the effects of large doses of radiation, bearing in mind the very serious injuries that affected the pioneers within the new area. There was therefore a delay of several years before anyone thought about the risks from small doses. Other reasons are that the impact from low dose rates requires a long build-up period before any harmful effect can be expected to show, and that the effects of small doses of radiation are more delayed than is the case with large doses. However, the most important reason for the shortfall in our knowledge is that the effects of small doses are difficult to detect since such effects are not generally special ones and are therefore impossible to prove without comprehensive statistical studies. The fact that just a small number of people have been irradiated during their work has made it difficult to obtain sufficiently clear results.

The new Radiation Protection Act, which was designated SFS 1958:110, came into force seven years after the Radiation Protection Committee had been established. In May 1958, the King in Council had made the current decision regarding grants for Sievert’s Institute of Radiophysics, but following Swedish Parliament’s decision in summer 1958, the King in Council informed the Medical Board on 28 July that an increase in funds would be granted for the radiation protection activity. The government had taken note of the Radiation Protection Committee’s proposal for a ‘State Radiation Protection Committee’ but wrote that ‘[the King in Council’s] regulations regarding the Radiation Protection Committee [applied] until further notice until the instruction for the Committee had become valid’. The King in Council provided that a ‘Medical Board Radiation Protection Committee’ would be established from 1 August 1958. The Committee would be the central coordinating body for various radiation protection matters in Sweden from 1 January 1959 and be the radiation protection authority in accordance with the Radiation Protection Act.

This name gave rise to a great deal of misunderstandings. The Institute of Radiophysics had been in charge of the supervisory activity but, formally speaking, the Medical Board had been the authority and signed the decisions. When the Radiation Protection Committee came to fruition, the resources for this were a grant to the Medical Board so initially, it was necessary to see the Committee as belonging to the Medical Board. However, the King in Council's decision on the Radiation Protection Committee gave it an independent status, although the letter of 28 July said that 'Under the Medical Board's direction, the Committee shall be a radiation protection authority in accordance with the Radiation Protection Act'. In the future, however, the Radiation Protection Committee did function as an independent authority directly under the Ministry of the Interior and the only nod to the Medical Board was that 'the Director General and the head of the Medical Board or the person who deputised for him was entitled to participate in the Committee's discussions with the right, if the Committee makes a decision that opposes his opinion, to have his diverging opinion noted in the Committee's minutes'.

The King in Council largely followed the Radiation Protection Committee's proposal regarding the composition of the Committee but increased the members with an administrative expert (in addition to the Chair) and a radiation biologist. The term of office was four instead of the proposed five years.

With regard to the Institute of Radiophysics, it would continue to supervise radiological work and the storage of radioactive preparations for the rest of the year under the direction and leadership of the Medical Board. From 1 January 1959 when the Radiation Protection Committee became a radiation protection authority, the personnel concerned would be made supervisors in accordance with the new Radiation Protection Act.

The letter from the King in Council ascertained that, as well as employing the supervisors, the Institute of Radiophysics also constituted *Karolinska institutet's* Institute for Radiophysics and Radiobiology and was available for teaching and research at the Institute and for the work in connection with the healthcare services and research into cancers. A special clinical-physical department would be established for the Institute's work for the healthcare services at *Radiumhemmet*. The new Radiation Protection Committee would be the administrative board for the parts of the Institute of Radiophysics that did not come under *Karolinska institutet's* management. The Professor of Radiophysics at *Karolinska institutet*, i.e., Rolf Sievert, would be the Institute's representative and administrative head 'and lead and coordinate the research there'.

In this connection, Sievert had won the battle against Helde; his empire was intact and he was head of the radiation protection authority and the hospital physics, the radiobiology activity, the research and *Karolinska institutet's* university Institute for Radiophysics. The head of department had been uncertain, however. Several referral bodies had suggested a special radiation protection authority which was separate from the Institute of Radiophysics. However, the head of department had said on this matter: 'In the current progressive development stage and for as long as there is no definite evidence to assess the need for central radiation protection control in the longer term, there is in my opinion reason to observe some caution as regards undertaking organisational changes in this area.'

Sievert did have to give way on one point, however. He did not have the last word when it came to the important decisions; from 1 January 1959, that privilege went to the Radiation Protection Committee and its first Chair, the Director General of the Insurance Council* *Yngve Samuelsson* (1908–1977). Samuelsson was a skilled and experienced lawyer. He had been appeal judge at the Court of Appeal for Upper Norrland from 1948–1955, head of department for legal matters at the Ministry of the Interior from 1947–1948 and head of the Ministry of Health and Social Affairs' legal department from 1949–1955 before becoming head of the insurance field in 1955.

Other members of the Radiation Protection Committee changed over as the years went by and the deputies were more constant meeting participants than the ordinary members. However, one constant participant was Director General at *Karolinska sjukhuset, Gösta Dahlberg* (1896–1976). Dahlberg had started his career as clerk at the Swedish Board of Customs and advanced to Chief Customs Inspector, whereupon from 1944–1950 he was Administrative Director of the State Organisation Board. From 1950–1960 he was Director General of *Karolinska sjukhuset*. The now watered-down name of

* The Insurance Council was an authority that was set up in 1917 to settle complaints and disputes regarding work accidents. The authority ceased to exist on 1 January 1979.

government official in its original, positive sense is a good description of Dahlberg like the examples of the use of the word shown in the glossary of the *National Encyclopaedia*: *the Swedish functionaries are traditionally considered to be non-corruptible; it is clear that the proposal has been drawn up by a functionary, not a politician*. In brief: Gösta Dahlberg was a man of honour.

Other hardworking participants in the Radiation Protection Committee's meetings, as either members or deputies, were Director General of the National Board of Occupational Safety and Health *Hilding Starland* (1902–1997), Arne Forssberg, or alternatively Arne Nelson from the FOA as biologist, Professor *Torbjörn Westermark* (1923–2001) from KTH as nuclear chemist, and Professor *Carl-Johan Clemedson* (1918–1990) who became Surgeon General and head of the Defence Medical Service Administration in 1964. The Medical Board very rarely participated with any representative. The Committee quickly found it to be efficient to allow the majority of the cases to be dealt with by a work committee consisting of Samuelsson, Dahlberg and Sievert.

The establishment of the Radiation Protection Committee meant that it was appropriate to reinforce the activity with a lawyer who could also function as the Secretary of the Committee. The first person to hold this post was a Bachelor of Laws called *Rune Lindquist* (1926–2001) who had served in a district court. Lindquist was a pleasant person but seemed weary and disinterested in the activity. He left the Radiation Protection Committee at the start of the 1960s to become Director General of the Equipment Committee for Universities and Colleges. To the surprise of his former colleagues, he turned into a completely different person there, ingenious and energetic. When he left, tribute was paid to his efforts into turning the Frescati area into one of the premier university areas in Europe in appreciative commemorative words from the four university Chancellors with whom he had cooperated.

Lindquist was succeeded by another lawyer, *Carl-Gösta Hesser*, who rapidly became popular owing to his interest in what was going on and who would, along with the virtually irreplaceable *Svea Forss* (1919–1997), end up becoming the administrative nucleus of the Radiation Protection Committee and later on the Radiation Protection Institute for a long time.

In 1959 the FOA was reorganised and a new department, FOA 4, was formed with Torsten Magnusson as head. The remainder of FOA 2, which Magnusson left, was nuclear charge physics, detonation, detection (i.e., the parts of physics that did not actually concern nuclear physics), effects of radiation and radiation measurements. People started to say 'weapons research is on the way out and protection research is on the way in'.

Since 1957, I had had the good fortune to have a number of articles, eight to be precise, published on *Dagens Nyheter's* cultural page on radiation and risks of radiation. This fortune was not independent of the fact that *Herbert Tingsten* (1896–1973) and Rolf Sievert had been fellow students at the college called *Nya Elementar*. Sievert wrote in a letter to me while I was working at UNSCEAR's Secretariat in New York: 'Have called Tingsten and asked him to tell the relevant editors that you must be treated well and that your articles will probably be very good'. As luck would have it, my articles were also appreciated by *Olof Lagercrantz* (1911–2002) and primarily by the impeccable *Ingemar Wizelius* (1910–1999) of the cultural editorial staff.

In 1959, I wrote in *DN* about 'Pauling and Nuclear Weapons', 'Our Variable Radiation Environment', 'Calculating Radiation Risks' and 'The Superbomb and the Lucky Dragon'. In the article on the calculation of radiation risk, I highlighted the way in which all probability calculations were dependent on the validity of the suppositions made, validity for which there is no objective probability. I wrote:

> In spite of the serious content of these calculations, it is easy for them to be made a laughingstock and to often be viewed as a refined numbers game with little bearing on reality. To make a rough comparison, you might say that a motorist who is stopped when approaching a bridge becomes confused when told that the bridge can probably cope with five tonnes, maybe even ten, but that it is not impossible that it *could* fail under one tonne and perhaps even fully collapse at any time. It is not much help to the motorist to know that he has the option of estimating which risks he has to face *if* the bridge collapses. Where most events are concerned, it is true to say that things might go well, but that you cannot preclude that they will go badly. The weakness in this type of presentation of eventualities is that in principle they cannot be linked with any

> probability value to enable you to weigh them up against other quantities in a summary calculation.
>
> […]
>
> In the case of the harmful effects of radiation, this is exactly where the difficulty lies. You cannot state a ‘probability’ for one theory or hypothesis being more valid than another, although you can probably estimate which risks the individual or humanity would face if an arbitrary hypothesis were definitely true.
>
> […]
>
> So, what is the value of an estimate of a number that it is possible to calculate but which cannot be relied on owing to the uncertainty of the assumptions? The answer is that you cannot perform the full calculation without assuming the numerical values of all the constants and quantities that affect the result or without creating conditions for the mechanisms that are active. This gives you some insight into what is required for a reliable calculation, which makes it easier to weed out less well-founded estimates.

I described UNSCEAR’s assumptions which, provided the probability of radiation-induced leukaemia was proportional to the dose of radiation, led to the estimate that each year of the scope of nuclear weapons testing hitherto could possibly lead to a few cases of leukaemia in the future in Sweden. I concluded with:

> The moment that the risk cannot be considered to be proportional to the dose, particular attention must be paid to the most irradiated individuals. A calculation of the number of injuries then becomes extremely complicated since in this case you have to take into account the combined effect of all sources of radiation.

In February 1959, Sven Hultberg, Erik Hulthén and Rolf Sievert gave joint expert statements on the applications for the new management posts at the Radiation Protection Committee’s x-ray department and nuclear physics department. Matts Helde, Lars-Eric Larsson and Thor Wahlberg had applied for the first job and were placed in the same order of precedence by the experts. The only applicant for the management post at the nuclear physics department had been Arne Hedgran, who was declared as obviously being competent for the job.

In February the Department of Agriculture experts, including myself, arranged to assist the Water Conservation Committee at an investigation into the need for special expertise in matters of radioactive contamination of water recipients. My contribution was negligible, but *Lennart Hannerz* (1922–2019) from the National Board of Fisheries wrote a 60-page memorandum proposing an organisation.

In April 1959, Sievert, Benner and Lidén together wrote to the Chancellor of the University:

> The undersigned, university teachers in radiophysics, have attentively followed the development within the new subject of medical physics which is closely related to our subject. In so doing, we have apprehensively ascertained that, at all Swedish medical teaching institutions at which it is represented, the people in charge of this subject are scientists whose education is purely and fundamentally medical. In our opinion, a full fundamental academic education in physics and conversance with work methods acquired through your own scientific activities is an imperative competence requirement for professors, associate professors or assistant professors in medical physics, with deviations permitted only in exceptional cases where there are special personal merits.

The three radiophysicists then referred to a publication by Gudmund Borelius dated 9 December 1955 from Swedish National Committee for Physics to the Ministry of Education and Ecclesiastical Affairs on the same matter.

On 6–17 May 1959, Rune Walstam went on a study trip to (the then) West Germany, principally to study the radiation protection conditions around the 2 000 curie cobalt-60 Gammatron that was at the Moabit Hospital in Berlin and compare them with measurements taken around the corresponding device at *Radiumhemmet*.

Walstam then travelled to Erlangen, Tübingen, Heidelberg, Würzburg, Frankfurt and Göttingen. He found that the high voltage therapy was now rapidly advancing and that much had changed since I had made the equivalent trip seven years previously. Up to 2 000 curie cobalt-60 Gammatron I cobalt guns were now in operation at several clinics. As yet, Gammatron II existed purely as demonstration examples. Siemens' 15 MeV betatron functioned satisfactorily at the clinics in Tübingen and Heidelberg, with tubes lasting thousands of hours. They were used for both x-ray and electron irradiation. The old betatron at the skin clinic in Göttingen was used solely for electron therapy but still required regular tube replacements. The Siemens 35 MeV betatron for research and industry was also used for radiation treatment at the Max Planck Institute for Biophysics in Frankfurt, but Siemens did not seem interested in developing it any further for medical use. They did not think there was enough to gain by using the higher energy.

In November 1958, the Defence Research Council and Sievert's Institute for Radiophysics had published a joint account of the measurement results that had been obtained thus far from measurements of the radioactive fallout. On 2 June 1959, the two Institutes published a second joint report. The FOA was using 10 stations to measure the activity in precipitation, 5 stations of which also measured the activity in the soil air. The Institute of Radiophysics had 13 stations for measuring gamma radiation from the ground and the activity in soil air and a further two stations to register gamma radiation from the ground. In summer 1958, it was shown that the gamma radiation from the radioactive ground deposition amounted to 10 % of the natural soil radiation. In May 1959, the corresponding share had increased to around 20 %.

Changes were taking place at *Radiumhemmet*. Its most skilled doctor, Lars-Gunnar Larsson, left Stockholm after thirteen years at *Radiumhemmet* and became Professor of Radiotherapy with Tumour Diagnostics in Umeå in 1959. He left behind the isotope laboratory which he had successfully driven forward and where physicists like Inger Ragnhult and *Gunnar Walinder* had made early achievements. This was also where the subsequent head of *Radiumhemmet*, *Jerzy Einhorn* (1925–2000) had worked since 1954 and defended his thesis with Larsson's support.

Radiumhemmet's treatment results had long since aroused international interest and attracted many foreign visitors. One such visitor was Professor Joseph Mitchell from Cambridge who took it upon himself to learn Swedish just to be able to read *Radiumhemmet's* records. One doctor who made a strong impression on me through his insight into the patients' problems was *Arvid Hultborn* (1907–1990), although he had already left Stockholm in 1956 to become Chief Surgeon in Gothenburg. Hultborn cooperated in the short term with Arne Forssberg with regard to studies on the impact of oxygen gas for tumour treatment and was one of the doctors who attempted to obtain successful treatment results using the unwieldy Maximar.

Thanks to Gösta Forssell's forward thinking and initiative, *Radiumhemmet* had collected statistics on all treatment results. Forssell doubtless intended this material to provide knowledge on how the treatments had succeeded or failed and would thereby contribute to better treatment methods in the long term. Unfortunately, his successor did not have the same visions. The information was zealously collected but there was no procedure for processing it and drawing conclusions. The sole exception was when a doctor delved deeply into the archives for his doctoral thesis and was able to discuss a particular problem. I could not help but make comparisons with the business world where sales statistics were regularly processed to provide a basis for continued planning.

One day, Rune Walstam and I were called to see Berven, who had a visitor in the shape of an American scientist who maintained that radiation treatment did more harm than good owing to the risks caused by radiation. Rune and I unearthed statistical information and were able to draw diagrams showing how the survival percentages fell over the years for treated and untreated patients. In all cases, as expected, the survival rate for those who were treated was considerably greater than for those who were not treated. A critic could say that this was not because of the treatment but because those who were not treated constituted a selected group for whom treatment had not been considered worthwhile. But the crucial evidence of the benefit of the treatment was that the curves for the survivors levelled off and, after 5–10 years, their gradients were the same as for completely healthy patients. If after the treatment the annual mortality risk was the same as for healthy patients in the same age group, people had to realise that the treatment was successful for the not insignificant share who survived the first five years. Unfortunately, however, this did not apply to breast cancer, where the remaining risk after ten years was still higher than normal although considerably lower than for those who had not been irradiated.

Haemangiomas are the benign blood vessel tumours which affect infants to a great extent right from the time of their birth and are usually superficial and unsightly. In most cases they disappear of their own accord but there are cases where they grow very quickly before doing so and can give rise to bleeds and necrosis. Bearing this possibility in particular but also cosmetic reasons in mind, radiation treatment for haemangiomas using encapsulated radium started early on. Such treatments started in Sweden as early as 1909. There was no awareness of any risks then. At the end of the 1950s, however, it was thought that the use of irradiation probably did not justify the possible risks. By then, more than 14 000 infants had been given radiation treatment in Stockholm. In Gothenburg, the number was 12 000. Gothenburg doctor *Sture Lindberg* (1923–2015), who had substantial experience of haemangioma treatment, has said: ‘I would like to suggest that the best cosmetic result can be accomplished by letting nature take its course’. A follow-up of the irradiated children has recently taken place at *Radiumhemmet* in Stockholm and at Sahlgrenska University Hospital in Gothenburg and it has been possible to show that there is a greater risk of cancer. Sture Lindberg’s conclusion (Lindberg, 2001):

> As a matter of fact, radiation treatment of haemangiomas during the twentieth century was widely considered a *lege artis* measure. We know today that it was not only unnecessary but also had unwanted side-effects. In retrospect, the large cohorts presented here might somewhat cynically be looked upon as a gigantic radiation experiment from which it is our obligation to learn as much as possible.

Not just haemangiomas but other benign growths such as warts were previously treated with radiation. In 1956, *W. M. Court-Brown* and *Richard Doll* (1912–2005) had also reported on the increase in the prevalence of leukaemia among patients who had been treated with x rays for Bechterew’s disease (Ankylosing Spondylitis), an inflammatory process in the joints of the spine.

The National Association of Medical and Health Physicists had been formed on 27 October 1954. The name reflected the two prevailing focuses of interest - medical physics which was initially dominated by radiophysicists, and the radiation protection activity whose representatives were mainly at Sievert’s Institute of Radiophysics, the FOA and *Atombolaget*. On 2 December 1961 following a period of uncertainty regarding the objective (trade union or scientific activity), the association split into the Swedish Society for Radiation Physics for the trade union activity and the Swedish Radiophysicists’ Association for the scientific. The special interests of medical physics were safeguarded partly through the Örebro Nordic Association for Clinical Physics as discussed with Karl Johan Vikterlöf in 1962.

Rolf Sievert was a very hospitable person. Innumerable visitors to his institution enjoyed this hospitality. Sievert himself saw the visits as good opportunities to visit his favourite restaurant, *Stallmästaregården*, for lunch. He invited me along on an embarrassing number of occasions – embarrassing because every time he insisted on acting as host and refused to allow me to pay for myself.

A characteristic episode from these lunches was the destiny that affected Karl Morgan’s colleague Mary Jane Cook from Oak Ridge when she visited Sweden at the end of July 1960 and obviously then Sievert as well. The usual ritual was followed, Mary Jane was invited to lunch and Sievert asked me to accompany them. We took a taxi to *Stallmästaregården* of course - it was Sievert’s only means of transport since he did not drive a car himself. It was unthinkable that he should actually walk the short distance. In wintertime when it was cold he took a taxi from the Institute of Radiophysics to *Radiumhemmet*,less than 100 m away.

Sievert was in the best of moods and was anxious for his guest, who was a very slender and small lady, to be generously fed and watered. I saw that Mary Jane had difficulties getting through the substantial lunch but Sievert, who politely enquired about the work at Oak Ridge, noticed nothing. Eventually it was time for dessert and Sievert ordered strawberries with whipped cream. Anxious for his guest to receive an adequate portion, Sievert himself piled up a large amount of strawberries for her followed by a huge portion of whipped cream. Mary Jane’s countenance paled but she dared not object. When she had finally cleared her plate, Sievert asked if she liked the dessert and did not notice that the forced nod scarcely corresponded with the guest’s evident feeling of being overfull. ‘So you liked the strawberries!!' exclaimed Sievert with delight. 'Then you must have another portion!' And he started loading strawberries and whipped cream onto the plate once more.

Mary Jane now had a chalk-white face and rose quickly. 'Excuse me …,' she said in a panic and rushed out towards the toilet. Sievert looked after her in surprise. 'What's wrong with that nice young lady?' he asked, concerned and to all appearances clueless.

In connection with the construction of the Ågesta heat and power station, negotiations took place in the Water Court regarding which discharges would be permissible to Magelungen Lake. An association for the surrounding residents was very concerned and protested against the discharges.* Experts were engaged by both parties. One of them was none other than Nobel Prize winner for Chemistry for 1954, Linus Pauling, Professor at the California Institute of Technology. He left a statement which was based on the assumption that the discharges could cause doses of radiation corresponding to 2 % of ICRP's dose limit of 300 milliröntgen per week in genitals and blood-forming organs. Pauling performed a consequence calculation based on the assumption that 10 000 people would receive this dose and concluded that the consequence would be between 1 and 5 deaths from cancer per year and the corresponding risks of hereditary injuries. Pauling's risk calculations were reasonable but his assumptions regarding the doses of radiation were too pessimistic. As nice people often do when they are faced with something they find unfair, Sievert's normally gentle and level-headed radiation biologist Arne Forssberg became very agitated at Pauling's statement and accused Pauling of being scientifically dubious. I describe why in chapter 17.

On 14 November 1960, Thor Wahlberg handed the Radiation Protection Committee a letter about extended holidays for radiological workers. Wahlberg referred to the fact that in April 1959, the Committee had received a report regarding extended holidays and on this basis had drawn up a proposal for dealing with holiday matters. Wahlberg, who like other radiation protection inspectors working for Sievert, enjoyed the 'radiological holiday', realised that the extended holidays would be difficult to justify if one were to take into account the doses of radiation received by personnel engaged in radiological work only. He instead referred to the dose rates and primarily the importance of the 'dose per second' which he and Helde thought they had demonstrated in 1953.

An undated memorandum to be presented to the Committee at the time of Wahlberg's letter said that: 'Strictly speaking, the matter of extending the holidays for personnel employed in radiological work begins with the question of whether persons concerned are actually entitled to a special holiday extension for the work in question'. On 16 December 1960, the King in Council had made a decision to overhaul the holiday legislation and instructed the experts to 'consider whether or not the time has come to completely transfer to the labour market parties the matters of longer holidays than those shown by the general rules of the Holidays Act'. The memorandum pointed out that if the Committee found it appropriate to wait for the outcome of the overhaul, its measures could now be limited to 'issue rules of application for the area on the basis of applicable Law and statutes'.

The extended holiday was on its way out. The last person to have a three-month 'radiological' holiday at Sievert's institution is said to have been Lars-Eric Larsson. Sievert himself ended up being irritated because the holiday extension meant that many of his colleagues were not available when he needed them in the summer. That was when he muttered 'Here I am, ensuring that I've given them three months' holiday, and they've now actually had the nerve to go and take it as well!'

* In 1974, the same association appealed to Vattenfall that the heat and power station should not be closed down. It was now thought that the lake was so clean and the air so clear compared with how they had been when oil had been burned in the heating boilers.

10. RADIOACTIVE WASTE

During his stay in Geneva in April 1956, Sievert had a number of talks with high-up officials at WHO about his ideas for an international radiation protection organisation. He met the Director General of WHO, Brazilian Dr. *Marcolino Candau*, but above all the Deputy Director General Pierre Dorolle and his assistant Dr. *I. S. Eve*. During these talks, Sievert was given confirmation of a promise that had already been made to him during the 1955 WHO course in Stockholm, that of a grant for me to go on a trip to the USA to study the way in which the Americans handled radioactive waste. Sievert's characteristic forward thinking made him realise very early on that the waste handling would create problems.

As far as I was concerned, I had definitely completed the chapter of work with the high voltage hall and I was showing greater interest in medical physics instead. When Sievert started hinting that he had plans for me in the international radiation protection work (the first time this had occurred was as early as 1952 when he and George de Hevesy arranged the unique radiation protection meeting in Stockholm), I was very hesitant. Sievert's plans were too vague and I had difficulty seeing where he was heading, so I listened to him talking about the grant with mixed feelings. My greatest hesitation was that I did not want to leave my family for three or four months. There was no such obstacle as far as Sievert was concerned. 'I'll sort out the money so that your wife and daughter can come too,' he said, as though it were the most natural thing in the world, and not many days passed before the Director of Thulebolagen *Alvar Lindencrona* (1910– 1981) informed me that he intended to honour a promise to Sievert to provide me with financial assistance. The Atomic Committee, which was paying my salary at the time, was willing to also pay it during the study trip, and the Ministry of Education and Ecclesiastical Affairs awarded a travel allowance. The only thing that remained was to get to grips with WHO's bureaucracy.

The only way we could possibly manage the trip in financial terms was to buy a second-hand car in New York and complete the whole trip by car. There was no such option in the minds of the WHO officials and a long exchange of letters followed, interspersed with long telephone conversations with the Swedish contact at WHO, Dr. *Malcolm Tottie* (1909–1996) of the Medical Board. WHO maintained that travelling by car was not advisable. There was much more traffic in the USA than in Sweden, and the number of accidents increased by the number of cars squared. My answer to this was that although there was truth in what he had said, the increase to my personal risk by the number of cars was only linear. The WHO officials expressed misgivings, saying that the presence of my family would reduce the efficiency of my studies, and I insisted that it would actually have the opposite effect.

The WHO officials pointed out that it takes longer to drive than to fly. My response was that since that may well be the case, I was willing to forego payment for the extra time. However, on the other hand, I said, I could reach more interesting plants *en route* by car than if I were to fly. The WHO officials warned me about the large sums of money that could be imposed on me were I to have an accident and that they had to demand that I take out adequate insurance. I said there was no way I would even consider driving a car in the USA without being fully insured. In the end, WHO gave way and we were able to start our journey on the *MS Stockholm* from Gothenburg on 22 May 1956.

Once in New York, we went to a Chevrolet firm on Broadway on 57th street and bought a large, second-hand 1954 Chevrolet station wagon for 1275 dollars. The car was delivered to the hotel one morning and we went on our way through the Manhattan traffic towards the Lincoln Tunnel beneath the Hudson River and continued along New Jersey Turnpike to Washington to meet officials at WHO and visit the Atomic Energy Commission which had set up a programme for our trip. Helpful motorists who passed us were waving and pointing to our car and shouting something which sounded to us like 'Fire!' When we came to a halt to find out why people had been shouting, we found that they had been shouting

'Tyre!' We had a puncture. At the next workshop we found that our tyres, the tread depth of which we had checked before buying the car, had been touched up by someone having carved new tread through the rubber down into the cord fabric. At the workshop we learned some new words of wisdom: 'He who buys somebody else's car buys somebody else's trouble.' On the way to Washington we stopped in Baltimore where I visited Johns Hopkins University and was given information on research into water contaminants and was advised to visit the Institution of Oceanography in La Jolla (California) and Woods Hole (Cape Cod).

On the first day in Washington I visited WHO's Regional Office for the Americas to meet Dr. *Irvin Lourie* who was in charge of scholarships. He said that Lennart Hannerz from the Fisheries Society was going to do a trip around the USA at the same time as me and that the two of us were trial animals in that we were WHO's first two holders of scholarships within the field of atomic energy. Hannerz was obviously also travelling at Sievert's initiative.

The official at AEC who was responsible for our trip was Dr. *Forrest Western* (1902–1972), who was a member of ICRP's committee V for radioactive waste at the time. While Marrit (my wife) and Karin (our daughter) were waiting at Davis House (a guest house on R Street where rooms were priced at $3.10 per adult, including breakfast and afternoon tea), I went to see Dr. Western and saw the security machinery of the cold war for the first time. Throughout the time that I was in the building I was accompanied by a military policeman armed with a pistol and was impressed by the fact that the waste-paper baskets were emptied by two armed police into a special bag so that no word would be seen by unintended eyes.

Dr Western was fairly brusque in asking what my future position in Sweden was and whether the Institute of Radiophysics would end up being responsible for radiation protection supervision within the atomic energy field. We were soon joined by the head of the biophysics section of AEC's Division of Biology and Medicine, Dr. *Walter Claus*, who asked whether we in Sweden were cautious as regards radiation protection matters and whether anyone from the industry thought that our proposed protection measures thought they were unjustified or approved them.

Dr Western described the plans for my next visits, the most important of which was to Oak Ridge National Laboratory where I would meet two people whom I already knew: K. Z. Morgan and Elda Anderson. Forrest Western was evidently in a dilemma when it came to Morgan. He wanted to warn me without making Morgan sound odd. 'Dr. Morgan is not one of us,' he said, 'that is to say he is not employed by the AEC and cannot give opinions on our policy. He is employed by the contractor, Union Carbide and Carbon Corporation. Bear that in mind. Morgan is a clever man, but he is not one of us.' Dr. Western ended our conversation by saying that no information would be given to me spontaneously. All information that was given was selected on a need-to-know basis. Questions would be answered, but no more than that.

I mentioned that in Baltimore I had been advised to visit La Jolla and Woods Hole, but Western did not think there was anything to be gained by doing so. Not only that, he did not think that contaminants in the seas constituted a problem to human health. I wondered whether they could constitute a risk to animals. Dr. Claus said that ecologists might think that something like plankton could be affected so that the nutritional balance in the seas would change. Western thought that ecologists were as good as geneticists at making a fuss about nothing (Hermann Muller had just published a provocative article in *Saturday Review*). I insisted on visiting La Jolla and Woods Hole.

In my notes about the trip I wrote:

> I endeavoured to gain an overview of the current position of the waste issue but people avoided answering me. I asked whether what had been said in Baltimore was correct, i.e., that processing [reprocessing to extract plutonium from the spent nuclear fuel] took place in just three places, i.e., Hanford, Savannah River and Idaho. That was correct, plus also to some extent in Oak Ridge. I pointed out that these were the places that had the waste problems for as long as all [used] fuel was transported there. Claus said that it was not a matter of major transportations taking place since said places were right next to the reactors. My answer was that as soon as nuclear power reactors were built, this led to the necessity of transporting the fuel to said places or planning a new station. Claus said that nothing to speak of was planned for nuclear power reactors since

> the United States was not dependent on atomic energy; the main emphasis was instead on experimenting with new types of reactor. Claus then said that no waste was currently being disposed of – it was simply stored. I said that that was a question of terminology.
>
> I would not be given access to any of the three said stations. Arrangements could be for me to be able to talk to people, and I could always 'look at the Columbia River through the hotel window if I wanted to'.

In receipt of this friendly encouragement, I visited Dr. *J. A. Lieberman*, also at the AEC's headquarters in Washington, the next day. Lieberman worked with waste issues in a section for Sanitary Engineering under the Division for Reactor Development. The section was run by a man by the name of *A. E. Gorman*. Lieberman gave me a quantity of useful literature and references but did not see the waste handling as a major problem.

At the time of our visit there were still no commercial reactors in operation in the USA. It was estimated that Shippingport would be complete in 1957. The large Dresden and Indian Point nuclear power plants had certainly been given planning permission but were not expected to be commissioned until the 1960s. There were no nuclear fuel reprocessing plants in the planned reactors as yet. There was an awareness of the future waste problems and research was ongoing. However, the practical problems with reprocessing and reactor waste remained only in the Atomic Energy Commission's plants such as the weapon-grade plutonium-producing reactors in Hanford, the reactor testing institute in Idaho and the national laboratories of Argonne, Brookhaven and Oak Ridge. Sievert had taken early action in taking the initiative to organise my trip.

We continued to Oak Ridge National Laboratory in Tennessee. My contact person there was Elda Anderson. She and her assistant from the WHO course in Stockholm, Myron Fair, took good care of us. Tennessee was a 'dry' state and Elda Anderson made sure that she rolled down all the blinds before offering us a beer. We talked about the WHO course and my waste assignment, and I was warned about Karl Morgan once again, although this time from another perspective. 'People say that Dr. Morgan is incredibly stubborn,' said Dr. Anderson, 'but it's more than stubbornness; he has a burning conviction. He's living in the wrong century; he should have lived in the times of the Crusades.'

However, the thus-described head of radiation protection Karl Morgan received us amicably and allowed us to meet his most important colleagues, including *Ed Struxness*, who told us about the attempts to have the radioactive waste embedded in a ceramic mass. I was able to see how the emission of radioactive substances had been dealt with and, to Elda Anderson's horror, I committed the dodgy act of stealing a leaf from an oak tree at White Oak Lake to take it home and measure the activity of any radioactive substances absorbed. White Oak Lake was a reservoir in the form of an extension of White Oak Creek, the river into which the wastewater from ORNL was released.

The waste was low-level laboratory waste in liquid form which had first gone through a large sedimentation damn before being released. The purpose of White Oak Lake was to act as a buffer before the river containing the contaminated water flowed into the Clinch River. The reservoir meant that there was some delay to the flow of water so that the short-lived substances had time to decay before the water reached the river and Tennessee Valley. Dr. Morgan took us on an excursion to the nuclear power plant Morris Dam and this gave us an idea of the scope of the enormous Tennessee Valley project.

There were four businesses at Oak Ridge which came under the Atomic Energy Commission. Three of these were run on a contract by the Union Carbide and Carbon Corporation. One of these three was Oak Ridge National Laboratory where K. Z. Morgan was head of the radiation protection activity. The other two were the separation plants for uranium-235 from the Manhattan Project, i.e., the diffusion plant and the electromagnetic separation plant. The current activity at these plants was still secret. It is interesting to note that the competent radiation protection activity that was run within the ORNL did not cover the separation plants, which presumably explained the general lack of knowledge shown by the personnel at the subsequent plants when it came to the risk of criticality accidents.* The fourth plant,

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* See 'The Sword of Damocles'.

which was not run by Union Carbide, was the Oak Ridge Institute for Nuclear Studies (ORINS) which was outside the enclosed area.

An interesting phenomenon was the air-cooled graphite reactor, now the oldest reactor in the world, which was still in operation and which was within the ORNL. The reactor core consisted of 1200 fuel rods with natural uranium and the thermal output was 3.5 MW. A great deal (40 %) of the time and cost was used to produce isotopes.

I also visited the ORINS cancer clinic under Dr. Marshall Brucer which had the world's first and only telecurie device with caesium-137 (1500 curie, i.e., around 55 terabecquerels) fitted for rotational treatment.

From Oak Ridge we continued to Ohio and Cincinnati to visit *Conrad Straub* at the Robert Taft Sanitary Engineering Center. I knew that Straub had recently been elected as chair of ICRP's Committee V which would give recommendations for radioactive waste. Lennart Hannerz made an appearance at that point so we went on the visit together. Straub had not yet formally started his job; he had previously worked with Morgan at Oak Ridge. I had a long discussion with Straub, who recounted his experiences of reprocessing. We also spoke about the requirements that ought to be set regarding the treatment of discharges in a watercourse. In Washington, the AEC officials had said that it would be sufficient if it were possible to keep the concentration of the different radionuclides below 1/10 of ICRP's recommended maximum permissible concentration ('the MPC value'). Straub said that such a principle was unsustainable. They had to take into account the possibility of several different discharges, the possibility of concentrations in biota and the possibility of bed deposits which could become loose after some time and flow towards water catchment areas.

The Robert Taft Institute came under the health authority, which was the Federal Public Health Service (PHS), and carried out training and research. The PHS undertook no supervision - that was the job of the different sub-states - but advised the sub-states instead.

From Cincinnati we continued west over dry prairies, the same routes that were once traversed by the settlers with their ox-wagons. When we approached the Rocky Mountains it was like being on a ship at sea and seeing the land ahead.

My first visit was to the AEC's Operations Office in Albuquerque, New Mexico. Forrest Western had told me that a visit there to discuss the waste problems in Los Alamos had been given the green light. A visit to Los Alamos was inconceivable in 1956. I was now being referred to a contact person in Albuquerque, Mr. *Everett Matthews*, whom I rang up. Matthews had heard nothing about my visit, however, but agreed to meet me at the entrance. When I arrived there I had to wait in the guard room until Matthews came and told that he had not been able to obtain any clearance for me and was therefore unable to allow me in through the gates. He cross-examined me apprehensively and called Forrest Western in Washington, but it was now too late to get through the bureaucracy. Matthews took me to University of New Mexico, but told everyone we met there that all calls had to take place on a purely unclassified basis. Therefore, the response to every question I asked was that it touched on classified information. I ended up with nothing as a result.

Kurt Lidén was to become the first non-American scientist to gain access to Los Alamos in September 1957; the barrier to foreign scientists had been lifted just a few months previously - maybe Forrest Western's irritation that I had been refused entry had influenced the decision.

We left Albuquerque disappointed and continued west past Flagstaff and the Grand Canyon and across the deserts of eastern California. The heat prevented us from stopping the car and standing beside because doing so burned your feet. The Pepsi Cola that we had in a bottle was hot rather than warm and felt like coffee that had just boiled.

Outside San Diego is the Scripps Institute of Oceanography which lies next to the little village of La Jolla and which was my next destination. The scientists I mainly spoke to there were *Theodore Folsom* and Swede *Gustaf Arrhenius* (1922–2019). Folsom was busy taking measurements of radioactive substances in the sea. Arrhenius was examining radioactive substances in bed deposits. I also met electronics expert Bruno Rossi, the man who I had so embarrassingly mistaken for Harald Rossi while visiting Failla in 1951.* In my notes I wrote:

> Scripps is to examine whether any activity can be measured from the waste which was dumped off the Continental Shelf. Folsom was cogitating over suitable field measuring instruments to measure gamma radiation at great depth from a ship. We ate lunch in San Diego with Bruno Rossi, who happened to be at the lecture [a lecture which we had attended in San Diego together] and happened to be staying in La Jolla. Folsom and Rossi discussed the possibility of using scintillation counters. I asked why they did not consider using a pressure ionisation chamber for this purpose. Rossi said that the only place where anyone had succeeded in getting an ionisation chamber to function satisfactorily with regard to isolation was Sweden; nowhere else had been successful with what Sievert had achieved. Folsom said that ionisation chambers were not efficient enough. I asked what the degree of efficiency was. He guessed at 5 % of the crystal. I said you could make the cross section of the chamber more than 20 times larger and in so doing obtain at least equally good net results and maybe a more reliable device into the bargain. Folsom doubted whether it was possible to get the registration device and amplifier to work adequately. I said that in this case there would be no need for any special amplifier, but chose not to persist further.

In his younger days, Folsom had worked with Failla and Marinelli at the Memorial Hospital in New York. He was very interested in Bengt Hultqvist's calculations of the dose of radiation above the ground since he was in the process of making equivalent estimates of the dose from radioactive fallout in the water. Since the substances falling into the oceans mix quite rapidly vertically but to only a fairly shallow depth, superficially-added contaminants are also at a fairly 'shallow' depth in the deep oceans.

I also spoke to Professor Yasuo Miyake, a Japanese geochemist who was working for Scripps at the time and who specialised in fallout measurements in Japan. He had been on the expedition on the *Shunkotsu Maru* to measure the activity in the Pacific Ocean after the Japanese fishing boat 'the Lucky Dragon' had been exposed to radioactive fallout. From La Jolla, we continued the short distance to Los Angeles in order to visit the University of California (UCLA) there. UC's head office was in Berkeley next to San Francisco, and the best known of its medical centres was in San Francisco where its radiological clinic, which did advanced research, was run by Bob Stone. There was another medical centre in Los Angeles, but it was more like a university clinic which focused on training doctors. It was run by Dr. *Stafford Warren* (1896–1981) who, like Stone, had been active in the Manhattan Project.

In Los Angeles, the Atomic Energy Commission had drawn up a contract with part of UCLA (called the UCLA Atomic Energy Project) which was the first Laboratory I visited there. It was performing extensive studies of the radioactive fallout, its chemical characteristics and the size of the particles, i.e., the circumstances that affected the uptake in plants and grazing animals. I wrote about my conversation with the radiation protection managers in the notes I took:

> The next man I spoke to was health physicist *L. Silverman*. The latter said that, although they were working with little more than tracking activities, they still had a great deal of animal waste which was difficult to dispose of. They had attempted to store the cadavers in containers which would be thrown into the sea, but they rotted and the gases made all sealed vessels explode. They were now burning the bodies and making sludge from the ash using water and cement, whereupon this concentrated waste was eventually exported. However, a few years ago, one of the State of California's authorities, the Fish and Game Commission with its head office in Sacramento, had banned the private companies that had undertaken waste dumping for laboratories, etc. from dumping more into the sea, and the ban had recently been made stricter so those at the AEC could not dispose of anything either, despite having attempted to discuss the point at issue. I asked how come the Scripps lot had not mentioned any difficulties with

* See Chapter 3.

> dumping waste since this was, after all, what they were assisting many authorities with. Silverman said that the Fish and Game Commission's authority was in a position to be able to refuse ships permission to sail from the harbours if they were carrying loads from activity but that Scripps' ships sailed without special permission and probably thereby circumvented the difficulty.

The next day I travelled firstly to Canoga Park on the other side of the coastal mountains and visited Atomics International (a subsidiary of North American Aviation) which was competing with General Electric and Westinghouse for the production of research reactors. I then had lunch at UCLA Medical School and was surprised to see Sven Benner there on his way to the International Congress of Radiology in Mexico City. We had to settle for looking at the place between the clinic buildings where a planned reactor was to be situated, submerged and with tall chimneys. The construction had been delayed owing to lack of money. We continued to San Francisco where we were very well received by Bob Stone. In a letter to Rune Walstam in July 1956 I wrote:

> It is devilishly cold here in San Francisco, approximately 10–15 degrees Celsius and windy and misty. People are wearing leather jackets and furs. The radiators are on day and night at the motel. We have now travelled 800 miles without the car packing up and it looks pretty much as though we will be able to continue. In Los Angeles, I met Benner who was on the way to Mexico. Los A. was where the first medical reactor was going to be commissioned but the housing had not yet been built. There is no medical reactor operating anywhere in the world at the moment. Only Brookhaven's reactor has been used for therapy irradiation. Everything else is only at the planning stage. Here in San Francisco, the day before yesterday, I was present at the treatment of the first patients to be treated with General Electric's 70 MeV synchrotron – which had just been delivered when I was in America five years ago. Five years have been spent on getting the device to function: an initial series of trial treatments was started one week ago. I saw three patients with tumours around the angle of the mandible treated with this mammoth contraption and with enormous circular paths.

I forgot to add that under Stone, Rune's proposal to use the beds for the patients who had radium applicators had been embraced. In the letter to Rune, I also answered an enquiry he had forwarded from Sven Hultberg at *Radiumhemmet* as to whether I would be willing to write a chapter on radiophysics in a planned Nordic textbook on radiation therapy. I was happy to do so and it would be informative. The editor of the textbook project was *Loma Feigenberg* (1918–1988), who had recently been employed as House Officer at *Radiumhemmet*. Loma came as a refugee from Denmark and first trained as a pathologist but found the textbooks so inadequate that he became interested in contacting a textbook publisher's and devoting himself to textbooks. He was the editor-in-chief for the Nordic textbooks on radiation therapy and tumour diseases and changed over to oncology, ending up at *Radiumhemmet* where he became a pioneer as regards the matter of getting his colleagues to improve their attitude towards dying patients. 'Why did you take care of the patient when there was hope of life but send him home to die when he needed you the most?' asked Loma, who gradually became known as the breaker of new ground where palliative care and psychosocial activity within the healthcare services were concerned. Loma was a demanding editor and taught me a great deal.

On 16 and 17 July I visited Berkeley Radiation Laboratory. On the first day, I spoke to the head of radiation protection, Nelson Garden, who gave an account of the way in which the waste was prepared for dumping at sea. I mentioned that Silverman at UCLA had said that the Fish and Game Commission had banned all dumping of radioactive waste into the sea. 'That's not true,' said Garden, 'I'm one of those in Sacramento and I know it's not true. Silverman isn't allowed to export anything but we do what we like. At UCLA they incinerate the waste to reduce the volume. This introduces a risk of radiation. Incinerating it is like spreading it out all over neighbours' land without asking permission!' Garden was very self-assured and I wondered whether he was as efficient as he sounded.

The next day I was joined on my trip to the Radiation Laboratory by chemist *Lennart Holm* (1926–2009) from the Nobel Institute (and later the FOA) who worked at the laboratory for a year. Garden showed me the protection devices. In my notes I wrote:

> Garden then showed me the devices for the handling of activity [I ought to have written 'the radioactive substances']. Big money had recently been spent on these, although Garden said that his system was the most economic since no contamination occurred outside his glove boxes. We cannot afford contamination when we are spending millions on finding the new elements by counting through a few lots of fallout. We have no activity in our washing – we never have any contaminated clothes. The personnel can eat their lunch in the room in which they work with activity. This all means that we will recover our outlay on an expensive primary protection system.
>
> I have looked at the devices and need to change my point of view to a certain extent – they did appear to be quite safe.

In San Francisco I also visited the Navy's laboratory, the Naval Radiological Defense Laboratory at Hunter's Point, a headland out in San Francisco Bay. I drove my wife and daughter by car and was of course stopped by the guard at the entrance. Yes, the boss, Dr. *Paul Tompkins*, was expecting me and my wife was allowed with us into his office but Karin, five years old, was deemed to be a security risk and had to stay with the guard. Dr. Tompkins thought this was unreasonable but they did not concede to him until after a lengthy discussion on the responsibilities involved.

I was told that the NRDL was in charge of all exports of radioactive waste from California. 90 % of the waste came from the University of California's two laboratories, Livermore and Berkeley Radiation Laboratory. The waste was sealed inside concrete and dumped 50 km off the coast, which was off the Continental Shelf. It would take a 14-hour return trip to get there. Those I spoke to did not think the waste containers would tolerate the pressure at a great depth but would probably break. Not all containers sank straight away – some had to be shot to bits.

I got to see barrels for solid waste and plastic containers for liquid waste. The latter was cast into concrete in the barrels. I was also able to visit the barges that shipped out the waste.

After San Francisco, our next destination was Seattle, which involved a car journey of 1500 km along the coast of California and Oregon. The journey went smoothly until we came to a desolate strip of coast in Oregon with the foreboding name of Devil's Elbow.

We travelled on US Highway 101, which passed high above the coast, and looked for a place to pull over. We found a small, winding turn-off which led down to the beach where a number of motorists were already taking a break, and watched sea lions resting on the craggy rocks. When we were about to return to the main road when we suddenly heard a thump and the car began to slide backwards. For some reason I thought we had suffered a broken propeller shaft and therefore moved over to the side so as not to block the narrow road. But it was an axle that had broken and my manoeuvre must have meant that the car moved but that the left rear wheel remained where it was in the middle of the road while dark oil began to ooze from the car like blood.

By now, other motorists had also had enough rest and were ready to leave and wanted to move past us on the road. It was narrow and I was forced to direct them since the edge of the road hid a steep precipice. One of the cars stopped behind us. It was an older couple and the lady asked if we needed help. Her husband could drive me to the nearest garage while she stayed behind to keep my wife and daughter company.

We gratefully accepted the offer and the man drove me away northwards. The nearest place was scores of km away and Highway 101 was also narrow and winding. There were steep cliffs on our right and there was an equally steep drop to the sea on the left. The man was driving unnervingly fast and I started to feel ill at ease. Suddenly he picked up a hip flask, unscrewed the lid and took a good slug of something which smelled like whisky. His apologetic defence was: 'The other day, my doctor told me that I have untreatable cancer. So I don't actually give a monkey's about anything!'

So then we arrived at the garage which sent a recovery vehicle, and we also got back in one piece. I realised that radiation risks were not the only dangers in life.

In Seattle, I visited Lauren Donaldson at the Applied Fisheries Laboratory, unaware that the laboratory's name was made up to conceal the actual purpose of the laboratory.[*] The laboratory was to examine the impact of the radioactive substances on fish. After Donaldson had first been employed as head of the laboratory, a year passed until he found out that the research was part of the Manhattan Project.

Donaldson was seemingly a serious man who readily showed me the laboratory and recounted the way in which different types of fish reacted to doses of radiation and temperature changes. The latter was probably the most serious consequence of releasing cooling water from the Hanford plant into the Columbia River. A number of the aquaria containing trial fish consisted of concentric vertical cylinders with the water between them. The fish could then swim around, explained Donaldson, thinking that they had endless volumes in which to swim. It brought to mind the cartoon showing a drunken man groping his way around a cylindrical advertising column, feeling confined and never finding a way out.

In Seattle, we once again encountered Lennart Hannerz who was travelling around. From there we continued over the mountains to the Hanford plant, a remarkable trip from the rainy and thriving area in the west to desert-like areas in the shadow of the rain-soaked mountains. Hanford was not yet open to visitors so I was obliged to stay in Richland by the Columbia River just south of the prohibited area. Richland smelled like urine and my guide explained that the smell came from evaporated urine samples from the personnel. I had the opportunity to discuss the discharges in the Columbia River. The discharge problem in Hanford was different from that of other reactors since the plutonium-producing reactors were cooled directly with water from the Columbia River which was thereby contaminated with the radioactive substances induced by neutron radiation. Before this cooling water was released back into the river, it passed pools which delayed the release in order to give the shortest-lived radioactive substances time to decay.

The Hanford plant was doubtless also the greatest source of high-level radioactive waste, i.e., used fuel elements and residual products after the fuel had been reprocessed. For the moment, such waste was being stored in large tanks until a final solution to the waste problem had been found. I often heard that not only did the fission products constitute waste, they could also be useful. My travel report of January 1957 shows that I was sceptical about this. I wrote the following in English:

> The fact that the United States have produced substantial quantities of radioactive fission products and have spent considerable amounts of time and effort seeking industrial uses for these since 1944 leads to the conclusion that the absence of a natural market today means that there never will be one unless a completely new demand arises. This does not mean that there is nothing to be gained from using radioactive fission products, but it is doubtful as to whether they are ever likely to represent any financial significance and they will probably create health problems to the same extent that they are able to reduce the immediate waste disposal problems. There is no doubt that some applications are worthy of this price due to an indisputable net benefit such as [the use of] caesium-137 as a source of gamma radiation for radiation treatment.

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[*] See 'The Sword of Damocles'.

Experimental breeder reactor

An experimental breeder reactor * (EBR-1) developed by the Argonne National Laboratory was commissioned in December 1951 and appears to have been the first in the world to supply electricity, albeit on a very small scale. In June 1953, it had been declared that the reactor had been used to demonstrate the possibility of breeding, i.e., that the reactor produced fissile plutonium to at least the same extent as the primary reactor fuel, uranium-235, was used.

Materials testing reactor

The materials testing reactor (MTR) was the second in the order of the reactors at NRTS to get started, i.e., 'become critical', which took place on 31 March 1952. It was said to have the world's most intensive neutron flow and was used to test materials and to produce radioactive nuclides such as cobalt-60 for the healthcare services. The MTR was run by Phillips Petroleum.†

Submarine reactor

This was also the home of the prototype for the first submarine reactor, a pressurised water reactor. It was the result of a joint project between the Argonne National Laboratory and Westinghouse and was built inside a steel hull of submarine dimensions. The STR reactor (short for Submarine Thermal Reactor) had become critical in 1953. It was the prototype for the reactor that was then used for the first 'nuclear submarine', 'Nautilus', which was launched in 1954 and reached the North Pole beneath the ice in 1958.‡ The STR was run by Westinghouse.

Boiling water reactor

The first boiling water reactor (boiling water reactor, BWR) was developed by the Argonne National Laboratory at the Idaho station in a series of reactors with the type designation 'Borax' (Boiling Reactor Experiments). These experiments were based on a proposal from 1952 by Samuel Untermyer. The previous belief was that steam bubbles in the reactor core (the 'void') would lead to instability in the nuclear reaction, but Untermyer showed that the bubbles in a boiling water reactor could instead help to stabilise the reaction (a 'negative void coefficient'). The first reactor in the BORAX-I series was built in 1953 for the purpose of analysing the effect of the steam bubbles on the stability. The next experimental reactor (BORAX-II) was modified in March 1955 and, unlike its predecessors, was supplied with a turbine generator for the production of electricity. The modified plant, BORAX-III, could generate 12 MW of thermal energy and 2.3 MW of electrical energy. This was the plant that supplied the small town of Arco in Idaho with 500 kW for electric lighting for two hours on 17 July 1955. The boiling water reactor had been born.

Irradiation plant

In the proximity of the materials testing reactor was an irradiation plant with very intensive gamma radiation. It consisted of a water-filled channel which was five and a half metres deep and in which spent nuclear fuel had been placed. Here, it was possible to generate radiation intensities that were thousands of times higher than was possible with a source of radiation from cobalt-60. The plant, which was run by Phillips Petroleum, was used to study the impact of the radiation on materials such as glass and plastic, but also to sterilise medical equipment and foods.

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* A *breeder reactor* is a reactor that produces more fuel than it uses. This occurs when the surplus of neutrons is allowed to be absorbed into the normally non-fissile but the most common naturally-occurring uranium isotope uranium-238. This forms the plutonium isotope plutonium-239 which is fissile and can be used as reactor fuel (and unfortunately also for nuclear weapons). This means that it is also possible to utilise uranium-238, which cannot be directly used as nuclear fuel. The breeder reactor is at its most efficient if the brakes are not applied to the neutrons in a moderator and the neutrons are allowed to retain their original energy and high speed. This is known as a fast reactor. Such reactors are not normally cooled with water but with liquid sodium.

† The Phillips Petroleum Company was formed in 1917 in Bartlesville, Oklahoma. The company's interests are in prospecting and extracting oil and natural gas but also in chemical industry and the production of plastic.

‡ See 'The Sword of Damocles' for the birth of 'Nautilus'.

Reprocessing plant

A chemical reprocessing plant had been commissioned in 1953 to separate fissile material from spent reactor fuel. It was also run by Phillips Petroleum. This was obviously where the greatest quantities of waste could be expected to come from.

One observation concerning protection, if not radiation protection, was that the management of the Idaho station required the employees and their visitors to use the safety belts in their cars. It was the first time that I had come across this idea and there were no belts in our own car. The protection awareness made a strong impression on me. Our trip continued eastwards through Yellowstone Park and over the high mountains between Wyoming and Montana. On 17 August I wrote to Rune Walstam:

> After two days at the Idaho Falls Reactor Testing Station, we travelled eastwards across Yellowstone Park and spent the night in the car in the park and had ice on the window panes and saw 19 bears, 3 large deer and a cougar which jumped down onto the road in front of the car, and I dipped a few flies into a river and caught four sea trout which we cooked in the great outdoors. We then left the park over a road which climbed to an altitude of more than 3 336 metres with snow on the road and wore out the clutch (which was not good to start with) and had it replaced by a nice gentleman called Sigmunstad who the whole village where we ended up staying said was the best car mechanic in the world.

Our next destinations were Minneapolis in Minnesota and Milwaukee in Wisconsin, where I would once again visit the Allis Chalmers betatron laboratory and General Electric's x-ray laboratories. Dale Trout had written and apologised, saying that because of a family death he could not take care of me but said that John Kelley would meet me. I wrote the following to Forrest Western about my visit to Milwaukee:

> In Milwaukee I visited General Electric's x-ray laboratory and Allis Chalmers' factories. Both produce accelerators for high-energy irradiation and I was interested in finding out something about their plans to use radioactive waste products for irradiation purposes since this is said to be one way of safely disposing of the long-lived caesium-137. However, they showed little interest in going into the field of radioactivity. General Electric represents Atomic Energy of Canada rather than producing its own radiotherapy equipment [with radionuclides]. This hints at an aversion to radioactive substances as sources of radiation, which is interesting to note.

From Milwaukee we continued to Chicago to visit the Argonne National Laboratory. In order to understand Argonne's involvement in the project which I had heard about in Idaho, I had to familiarise myself with The Atomic Energy Commission's organisation once again. There were three divisions under the Deputy Director General for Research and Industrial Development. The Division of Biology and Medicine was responsible for research within these life sciences. The division that was simply called the Division of Research was responsible for physics research. Finally, the Division of Reactor Development was responsible for all activities concerning reactor development, both civilian and military. The reactor development was located primarily at the Argonne National Laboratory, which had been established for this purpose in 1946. This research station was administered by the AEC's Chicago Operations Office, which reported to the Division of Reactor Development. However, medical and biological research did also take place at Argonne.

I already knew of Argonne's larger waste problem projects after my visit to Idaho. The thing that now primarily interested me was the idea of meeting the two physicists, Argentinian-born Leonidas Marinelli and Charles ('Chuck') Miller who had used scintillation spectroscopy to be able to show caesium-137 from the experimental explosions in the Pacific Ocean in the bodies of people in Illinois.

I met them along with their boss *John Rose*, a well-known radiophysicist who had previously cooperated with Failla. I realised that scintillation spectrometry to measure content of the radioactive substances in the body would be a necessary supplementation to Sievert's pressure ionisation chamber for the same purpose. Sievert could measure very small quantities of radioactive substances but he could not identify them in other ways except by estimating their decay rate. I told them about Sievert's low-level activity laboratory in the rock by Henriksdal's treatment works. Afterwards I wrote home to Sievert:

> I have looked at the scintillation measurements in Argonne a bit more thoroughly and there is no doubt that you acted wisely if you attempted to supplement Henriksdal with such equipment. Miller, who is actually the father of the instrumentation, is as green as spinach with jealousy because he has no access to any grotto laboratory with reduced cosmic radiation, and I heard him discuss with Marinelli whether or not they should ask you if they could take some measurements in the grotto in September when they have complete measurement equipment plus a few technicians in Stockholm for the instrument exhibition in any case.

From Chicago I also summarised my experiences so far regarding the waste problems in another letter to Sievert:

> I have heard all opinions on the danger of the atoms at this place. Of course waste should be dumped into the sea. It is generally stupid to think of releasing waste into the sea. The waste problem will be crucial to the profitability of atomic energy. There is no waste problem. We can generate damage for future generations. Our current disposal is completely satisfactory. Etc.
>
> Overwhelmed by all of the statements, I cannot give any of my own views at the moment: I must firstly lie like a python and digest everything that has been stuffed into me. I can then reproduce some impressions but I cannot promise [that] they will stack up.
>
> America has not planned atomic energy - yet. However, America does have energy from atoms. I have heard from one man for whom red tape was not as sticky as it ought to be that the industry had calculated that 2 % of all energy would come from reactors in 1975 *and that this would only involve doubling that which is currently available*. So there is quite a lot now. But the first reactors were built without taking into account what it would cost to produce atomic bombs. There was no knowledge of waste problems then. And then they had to improvise with regard to dealing with the waste when it did arise. The waste was not the best to deal with either - large quantities of salpeter acid containing loads of aluminium. The large volumes were what really created the storage problem. It could not be concentrated a great deal through evaporation due to the high salt content. If it was neutralised you ended up with an aluminium hydroxide gel.

At the start of my trip, in Washington, I asked Mr. Lieberman, who was the next person down from Gorman to deal with the AEC's waste research, which different types of waste were currently being taken care of, and that was when the lovely abovementioned mess was described to me. I then asked, as you would, whether they were doing any research regarding any other type of waste since in the future it would probably not be plausible to persist with what they had ended up with following the first tentative atomic bomb reactor experiments. Lieberman's reply was:

> The Atomic Energy Commission has considered that this waste is what we will have to handle for the next ten years, so all research is concentrated on that.
>
> Yesterday, John E. Rose at the Argonne National Laboratory here (which you may know) said that Gorman was 'the most incompetent idiot ever to have talked through his hat' and that Lieberman was probably 'even more stupid' since he was working under Gorman of his own volition. I include these declarations of stupidity purely as examples of the varying views that I have come across but, luckily for America, what Lieberman said is clearly not true. Representatives of private companies such as North American Aviation say that it would be unreasonable not to take the trouble to construct reactors and [fuel factories] whose end product was a waste which would be easy to handle.
>
> There is no reason to believe that atomic energy plants constructed now and in the future for peaceful purposes will cause the same waste problems as the first reactors which were built before the problems became known. Few industries are so 'infamously

> watched' with regard to the risks as the atomic energy plants. In America they are trying to make the best of a *fait accompli*. In Sweden, we have the option of being able to plan to take the risks into account right from the start. Struxness and Morton from Oak Ridge, who were consulted by *Atomenergi* about Studsvik say: 'It's very laudable that the requirements regarding the safe disposal [of the waste] are considered to be so important that the development of waste processes and plant construction has been influenced largely by this consideration. We can honestly admit that this isn't the case in our own country.'

After Chicago, we continued to Ann Arbor to visit the University of Michigan where we were well received by radiation protectionist *Claire Palmiter*, who showed us the reactor that was being built there. We were then going to travel to Pittsburgh to look at the construction of the first commercial nuclear power plant, Shippingport. We took the road past Cleveland in Ohio, the location of the Harshaw Chemical Co., which produced sodium iodide crystals for scintillation spectrometry. We had heard from Miller and Marinelli that Harshaw had already sold a large crystal to Sweden. We now found out that the customer was Kurt Lidén. I wrote to Sievert:

> That crystal which I mentioned in my previous letter which was sold to Sweden has gone to Lidén, so in principle he has the same measurement options as Marinelli's group provided he can arrange a radiation-free room. On the way to Pittsburgh I took the opportunity of visiting the Harshaw Chemical Co. […] which without doubt produces the best sodium iodide crystals in the world. […] Lidén's crystal, whose dimensions were 4 * Ø5 inches (the largest dimension that is included on the pricelist) had been mounted in extra radiation-free material like the crystals used in Argonne. This means that the casing around the crystal is made of electrolytic copper instead of aluminium and that the window that is to be connected to the photomultiplier is made of quartz rather than glass. A normal, mounted crystal of this largest size costs 1 885 dollars and a quartz window is an additional 150 dollars. The only difficulty with the delivery is that you have to wait around 3–4 months for the quartz window if you order one.
>
> I obtained said information through other activities. A more tangible result was that I paid cash for a small crystal for my measuring table at R.I. for 50 dollars.

Shippingport is a small town by the Ohio River outside Pittsburgh. When we were visiting, the USA's first nuclear power reactor was being built there, a pressurised water reactor which had been developed from the submarine reactor which was used in 'Nautilus'. From Pittsburgh I wrote the following to Sievert:

> Today I have visited the building for the USA's first nuclear power reactor at Shippingport, a small town by the Ohio River below Pittsburgh. The actual reactor and heat exchanger are being built by Westinghouse under contract from the A.E.C. Turbines and generators for the electricity are being built by the local electricity company, the Duquesne Light Co., which will then buy steam from the reactor for the A.E.C.'s generators. The reactor section is enclosed in 4 gigantic steel tanks.
>
> I am slightly concerned as to how I will deal with Alvar Lindencrona; I ought to have written him something but I really wanted to wait until I had actually met the researchers here and really discussed such matters, and it now looks as though it will not happen until Boston at the end of September. As you know, I also have misgivings as to whether it is correct to give any type of report to a single company considering that the State is sponsoring me for the trip. I will of course have to send him a copy of my final travel report, but he might consider me ungrateful until I do so.
>
> The weather over here is a joke. In California we froze to death and, like everyone else, had the radiators on night and day. The day before yesterday we were dripping with sweat on the way to Cleveland. Yesterday it was horribly cold all day. Today there has been lovely autumn weather. Frost is forecast overnight. In a few days' time we will probably be sweating again.

My next visit was to John Hursh at the University of Rochester Atomic Energy Project and his measurements of the level of strontium-90 in the ashes from parts of skeletons. It made me think more about the radioactive fallout from the nuclear weapon tests. The possibility that the radioactive substances which are spread from a nuclear charge explosion could constitute a global hazard had been discussed early on but was not the primary concern in people's minds to start with. The radioactive fallout worried only a few people before the American detonation of a 'dirty bomb' injured inhabitants of the Marshall Islands of Rongelap and Rongerik and the crew members on the Japanese trawler the *Lucky Dragon* in 1954. It was the immediate and terrible destructive capacity of the bombs that the ninety-three year-old *George Bernard Shaw* (1856–1950) had in mind when he wrote a letter to the editor of *The Times* on 24 December 1949, which read as follows:

> Much of your space and time is being wasted on the subject of atomic warfare. The disuse of poison gas in the 1939-45 war, because it was as dangerous to its users as to their targets, makes it very unlikely that atomic bombs will be used again. If they are, they will promptly make an end of all our discussions by making an end of ourselves. […] still, give me space for another cry in the wilderness, that my unquiet spirit, wandering among the ruins of empires, may have at least the mean and melancholy satisfaction of saying: 'i told you so'.

That same year, 1949, a scientist by the name of *Nicholas M. Smith* submitted a report to the American Atomic Energy Commission about the result of a study of the global consequences of detonating a large number of nuclear weapons. The report was classified and Smith's study was given the sombre codename 'The Gabriel Project', an ambiguous name since the Archangel Gabriel was said to carry out orders of punishment and predict future events. A similar but less comprehensive report was reproduced in the following year as a section of the publication called *The Effects of Atomic Weapons* (The Los Alamos Laboratory, June 1950).

In 1953, the matter was taken up again at a conference on the long-term effects of the atomic bomb which was held by Rand Corporation in Santa Monica in California.

The conclusions of the conference were summarised in a secret report (R-251-AEC) to the AEC. An open version of the report, which proposed a study under the name of Project Sunshine, was published by the Rand Corporation under the name of R-251-AEC (Amended) on 6 August 1953. The report refers to the fact that the Gabriel Project had already concluded that the most dangerous nuclide among the fission products is strontium-90, which makes for the skeleton and has a half-life of 20 years (or so it was thought at the time; the half-life is now said to be 29 years). Since strontium is similar to calcium in terms of its chemical characteristics, the two substances are followed and it appears to be beneficial to state the quantity of strontium-90 (or rather its activity) in relation to the quantity of calcium in samples of substances like milk or bone tissue. The unit of measurement used to be one picocurie* of strontium-90 per gramme of calcium. This unit was sometimes called a Strontium Unit (S.U.). Given that the project name was Project Sunshine, it could also be referred to as the Sunshine Unit but this was not really taken seriously in the long term.

Rand's report contains information stating that the detonation of a nuclear charge corresponding to 1 kilotonne of TNT produces 1 gramme of strontium-90, which with the conversion factor given elsewhere in the report would have given an activity of 200 curies. With the correct value of the half-life, the activity would be 137 curies (5 terabecquerels). You raise your eyebrows when you read the following about strontium-90 at the start of the report:

> We have not attempted in this preliminary report to define a 'threshold'–damaging dose, a 'mean lethal' dosage, etc. The terms are misleading and the magnitude of the dosages is unknown. Instead, we have normalized our studies to the Maximum

* A picocurie (pCi) or micromicrocurie (µµCi) is a millionth millionth of a curie (10^{-12} Ci) which is $10^{-12} * 3.7 \cdot 10^{10} = 0.037$ Bq, i.e., around two nuclear decays per minute.

> Permissible Concentration (MPC) set by the International Commission on Radiological Protection. This is the amount that, it is believed, may be retained safely in the body without causing minimal damage.
>
> The MPC for ^{90}Sr is 1 microcurie (μc*) (i.e., one two-hundred-millionths of a gram). This is an industrial standard for small numbers of people. It may be necessary to reduce the MPC values for large populations.

This is eyebrow-raising because here, the Rand report talks about what ICRP called the maximum permissible body burden of strontium-90. This is something quite different from the MPC value which stated a maximum permissible *concentration* in air or drinking water and was expressed by ICRP in microcuries per cubic centimetre in the 1950s. When the Rand report was written, ICRP's tables had not yet been published by ICRP but had been printed in the American Bureau of Standards' handbook 47 from 1951.

The name Sunshine Project aroused widespread ridicule and irritation. This affected people such as Lauriston Taylor when on 3 June 1957 he gave evidence before the sub-committee for radiation which was set up by the American Congress' Joint Committee on Atomic Energy. Taylor was to give an account of the different units that are used. When he comes to the unit which indicates strontium-90, the printed minutes† show that the following exchange takes place. Taylor is the first to speak, and when he says 'strontium' he is referring to strontium-90:

MR TAYLOR. One other unit that has been talked about is the sunshine unit, it is based on the maximum permissible body burden of strontium. Where we have set for occupational purposes a permissible body burden of one microcurie of strontium, this means one microcurie of strontium for the whole body. Since there are approximately a thousand grams of calcium in the whole body, so it means one microcurie of strontium 90 per thousand grams of calcium. When you go through the arithmetic, this would turn out to be a thousand micromicrocuries of strontium per gram of calcium.

Now the sunshine unit, I believe, relates primarily to the body burden for people not occupationally exposed. Those levels are considered to be one-tenth of the levels for occupational exposure [*Taylor presumably meant that the permissible body burden for those who are not exposed to strontium at work shall be one tenth of that which is permissible for the workers*]. So one sunshine unit would then be one one-hundredth of the average permissible body burden for the general population [*here, Taylor forgets that we do not want to permit every single person in the population to receive as much as the maximum that we want to permit for each individual person*].

A word as to the status of the units. On the whole I think it can be said that they are fairly well in hand.

Representative HOLIFIELD (*Chairman of the Sub-committee*). Excuse me, Doctor; was there any scientific reason why the word 'sunshine' was used in this instance?

MR TAYLOR. The term 'sunshine' itself?

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* The Americans wrote 'c' rather than 'Ci' for 'curie'.

† Reference USCo 1957, Part I (pp. 781–782).

Representative HOLIFIELD.	Yes.
Mr. TAYLOR.	There may well be, but I am not aware of it.
Senator ANDERSON.*	Did it not grow out of the fact it was called 'Project Sunshine'?
Mr. TAYLOR.	I suspect this is the source of the name; yes. I do not think there is any scientific reason.
Representative HOLIFIELD.	The word 'sunshine' has a cheery note to it, and I was just wondering if we were allowing, let us say, propaganda to creep into our scientific terminology. Why did you not put it 'happy' units, or something like that?
Senator ANDERSON.	I do think it should be said that he *[i.e., Taylor]* not only does not have anything to do with the naming of it, but never heard of it until he came to these hearings. Was it not the fact that the whole project making this study was given the name 'Sunshine Project'?
Mr. TAYLOR.	Yes.
Senator ANDERSON.	Just as we have 'Operation Plumbob ' going on. They have put out Project Sunshine as the most enlightened and happiest look on radiation damage.
Mr. TAYLOR.	If I might divert just one moment, when I was with the Atomic Energy Commission myself for a year some years ago, we started a problem closely related to this, and we called it 'Project Gabriel' – you know, Gabriel blow your horn. That had kind of a sinister sound to it, and it may be that they wanted to undo a little of that. - I think I was about to point out…
Representative HOLIFIELD.	I did not know but what maybe this might be a tranquilizing pill.
Mr. TAYLOR.	I obviously have had no close connection with this particular project, as you can see.

The early-established conviction that strontium-90 is the most hazardous nuclide of the fission products has been around for a long time. The characteristics of strontium were thought to be insidious.† The substance stays in the body for a long time and is a 'bone seeker'. The skeleton comes into the picture and adds a touch of gloom. On the other hand, the other long-lived fission product, caesium-137‡, seeks out muscle tissue, giving a 'fresh' impression. Caesium is also excreted from the body fairly rapidly. On the plus side for caesium, it was also worth noting the fact that, thanks to its gamma radiation, it is easy to detect and measure. Strontium-90 is a beta-emitter and therefore much more difficult to show; it needs to be turned to ash – another macabre word – and needs chemical analyses. Caesium-137 'comes openly' and strontium-90 'sneaks insidiously' into the skeleton. This is how words and associations can mislead us. In actual fact, caesium is the one that poses the biggest problems, but in the 1950s, strontium was the one that aroused fear.

I therefore felt uncomfortable listening to John Hursh when he showed how he turned bone samples to ash in order to measure strontium-90. They were still body parts and to me, Dr. Hursh's smile seemed

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* Senator Clinton B. Anderson (New Mexico) was Deputy Chair of the Joint Committee on Atomic Energy.

† A mineral containing strontium (strontianite) was first discovered in 1787 in a lead mine near Strontian in Scotland. In 1790, *Adair Crawford* (1748–1795) in Edinburgh was able to show that the mineral contained a new element which was named after the place near to where it was found. The level of strontium in the Earth's crust is 384 grammes per tonne. The metal is easily oxidised in air and reacts strongly with water during the development of hydrogen. The chemical characteristics of strontium are similar to those of calcium.

‡ The metallic element caesium is so reactive that in nature it does not occur in free form, only in chemical compounds. The average content in the Earth's crust is just 2.6 grammes per tonne. The chemical characteristics of caesium are similar to those of potassium. Caesium metal melts at 28.4°C. Caesium was discovered spectroscopically in 1860 in Heidelberg when *Robert Bunsen* (1811–1899) and *Gustav Kirchhoff* (1824–1887) found blue spectral lines in spectra from mineral water concentrate and were able to show that these lines belonged to a new element which was named after the colour of the lines (from the Latin *caesius*).

askew and insensitive as he stood before a fume cabinet in which a sample that had been turned to ash was being dissolved in salpeter acid. At the time, I did not know that John would become one of my best friends and that our families would socialise for the rest of their lives. John looked as though he had a crooked smile that time in Rochester in summer 1956 because, as I now know, of his shyness.

From Rochester we continued past the Niagara Falls to Canada. At that time there were four big attractions in the USA for a Swede: the Empire State Building, the Grand Canyon, Yellowstone Park and Niagara but, unlike the first three, the Niagara Falls were a disappointment for us. You can get close to a small waterfall but you have to keep your distance from a large waterfall. The dihedral angle occupied by the scene is largely the same, hence the disappointment.

The destination in Canada was the research station at Chalk River and a discussion on the Canadians' waste problems. It turned out that there was a competitive situation when it came to the plant's radiation protection. The person who was directly responsible for the radiation protection was physicist *Cam Tait*, a very eccentric but friendly man who invited us home to dinner and complained about his lack of influence. The person who did have greater influence was the head of medicine, Doctor *Gordon Stewart*, who went on to become chair of ICRP. Stewart did not invite us to dinner but instead tested my capacity to tolerate Canadian beer at a pub in the next small town of Deep River. Cam Tait left Canada soon after to do a job at IAEA in Vienna. The antagonism between Tait and Stewart was based on the fact that they each came under a separate main division of the organisation. Tait's radiation protection department was within the division for biology (with *C. A. Mawson* in charge) under the main division for research and development. Stewart ran the medical division under the main division for administration and operations.

The situation at Chalk River was of particular interest to a Swede because the land and climate conditions were similar to those in Sweden. In the report that I submitted to WHO after the trip (Lindström, 1957) I wrote:

> There is an obvious difference between the land use in Hanford and Chalk River. In Hanford, the groundwater flow is 7 metres per month, although the conditions in the disposal area are affected by the formation of groundwater mounds from the very substantial disposal of non-radioactive cooling water in surface pools. The estimated flow time from the separation pools to the river is 50 years. The fundamental policy in Hanford is also to avoid groundwater contamination – the soil columns are filled only until there is a risk to the groundwater, and they are then sealed. This is possible thanks to the insignificant precipitation; the Hanford area is practically a desert. At Chalk River, the waste has in reality been conveyed to the groundwater largely owing to the practice that was imposed on the laboratory owing to the NRX accident. Where terrain, climate and precipitation are concerned, the Canadian area is reminiscent of Sweden while the Hanford region seems very unfamiliar compared to Sweden.

We left Canada to travel to Schenectady and Knolls Atomic Power Laboratory, where I was given a thorough briefing of the waste handling. We then continued to Boston and Cambridge where I visited the sanitary institutions at MIT and Harvard, which were researching different methods of water treatment. In Boston, I had a conversation with the renowned Charles Williams about the insurance problems when it came to reactors which, in the event of an accident, would be capable of causing very substantial financial losses in addition to the compensation claims which could be anticipated owing to injuries to people.

Our final visit on the trip was to Brookhaven National Laboratory where I interviewed the head of radiation protection *Fred Cowan* and the man, *Lee Gemell*, who was responsible for environmental control. The greatest source of radioactive discharges and radioactive waste* was the air-cooled research reactor.

* At the time, the Americans differentiated between discharges (which were seen as unavoidable yet possible to regulate) and radioactive waste (which was something that was not conveyed to ground, water or air unless a decision were made to do so). The British saw radioactive waste as coming under both of these categories.

With this, both the air that was sucked in and the air that was blown out were filtered. There were a dozen measurement stations around the reactor, at up to a distance of 6 km, which continually registered the level of radioactive substances in the air. Unlike Hanford and Chalk River, there was no large watercourse that could receive and dilute the radioactive substances and, as with Chalk River, there was a high groundwater level. They therefore had to be very careful with liquid waste.

Part of my visit to Brookhaven was with the head of radiation protection in Studsvik, *Lars Carlbom*, which increased the sharing of knowledge.

It was then time to sell our car in New York (for 770 dollars) and prepare for the journey home, which was on 13 October 1958 on the *MS Kungsholm*. I made a naive attempt to be polite as we Swedes are and tell the car dealer who bought our Chevrolet that the brakes were now very poor. His reply was: ‘Don’t worry, mister, I could sell a car without an engine if I had to!’

After we arrived home, I wrote WHO an 80-page trip report (Lindström, 1957). When I glance through it now, I am surprised at just how much was known about the waste problems almost fifty years ago.

11. UNSCEAR AND ICRP, 1956–1957

At the start of 1956, before I went on my trip to America, I had been called to Rolf Sievert's workroom for an urgent assignment. He had given me a sheet of paper showing a list of names. 'You have four hours,' he had said. 'That'll show me what sort of blokes they are!'

I looked at the list. There were fifteen names, including Sievert's own. I recognised another one: Professor Mayneord. The others were unknown to me.

'The UN has set up a scientific committee on radiation,' explained Sievert. 'It consists of fifteen countries, including Sweden. It's now clear who will be their representatives. I want to know what sort of blokes they are.'

It was no easy task to complete in four hours, but I got hold of some information through reference books at *Stadsbiblioteket* (the City Library) and telephone calls to a few embassies. The concern about the nuclear weapons tests had led the United Nations' general meeting to adopt a resolution 913(X) on 3 December 1955 in accordance with which a scientific committee was set up consisting of fifteen countries: Argentina, Australia, Belgium, Brazil, Canada, Egypt, France, India, Japan, Mexico, the Soviet Union, the United Kingdom, Sweden, (the former) Czechoslovakia and the USA.

According to the second paragraph of the resolution, the Committee's task was primarily to collect information on observed levels of radiation and results of research from the Member States and the UN's special organisations into the effects of radiation, and to assess and compile this information into annual status reports and now and then into more extensive reports intended for publication.

The Committee was named the United Nations Scientific Committee on the Effects of Atomic Radiation, a name which was both scientifically misleading ('atomic radiation' is strictly speaking not just radiation from the atomic nucleus but primarily from the electron shell, i.e., thermal radiation, visible light and UV radiation as well) and intentionally if challengingly chosen so that the acronym UNSCEAR could be pronounced as 'un-scare' to convey the meaning of calm. The Swedish government appointed Sievert as the Committee's Swedish representative, with cell researcher Professor Torbjörn Caspersson as his deputy. The government also summoned an advisory expert group of nine experts who held their meetings at home in Sweden in the early days but never ended up playing any great role. Sievert also asked *Tage Kemp* and Reidar Eker to act as contact persons for Denmark and Norway.

When UNSCEAR had found its working method, the typical representation of the member countries was a representative, a deputy and a number of advisers. Since the UN paid the travel expenses of the representative only, the level of participation from others varied strongly from country to country. The USA might come up with a dozen advisers while the typical delegation from one country ended up being around five people. Australia, Egypt, India and Mexico often had just one or two participants. Rightly or wrongly, it was generally thought that some of the advisers in the delegations from the Soviet Union and the USA had political rather than scientific information.

UNSCEAR's first meeting was held in the UN House in New York on 14–23 March 1956 and its next meeting on 22 October – 2 November 1956. The UN supplied a scientific secretariat consisting of experts who were employed for one or a couple of years, supported by staff writers who, according to the custom of that time, consisted exclusively of women. Initially, the head of the secretariat was the Norwegian military physicist Gunnar Randers, although he was soon succeeded by the Canadian geneticist *Raymond Appleyard*.

At the Committee's first meeting in March 1956, the Member States were represented by the following representatives in the order in which they sat around the table:

Argentina: Captain Dr. Constantino Nuñes
Australia: Dr. C.E. Eddy
Belgium: Prof. Zenon Bacq
Brazil: Prof. Carlos Chagas
Canada: Dr. E.A. Watkinson
Czechoslovakia: Prof. Ferdinand Herčik
Egypt: Dr. A. Halawani
France: Prof. Louis Bugnard
India: Dr. V.R. Khanolkar
Japan: Dr. Masao Tsuzuki
Mexico: Dr. Manuel Martínez Báez
Sweden: Prof. Rolf Sievert
The Soviet Union: Prof. A.V. Lebedinskij
The United Kingdom: Prof. W. V. Mayneord
The USA: Dr. Shields Warren

The Swedish delegation included Torbjörn Caspersson as the deputy representative while the FOA experts Bo Aler and Arne Nelson were advisers.

One of UNSCEAR's scientific secretaries visited Sievert in the first year. I was summoned to Sievert's room where I found two people smoking cigars: Sievert himself and a younger copy of him, a voluminous young Argentinian whom he introduced as Doctor *Dan Beninson* (1931–2003), Scientific Secretary of UNSCEAR in New York. Sievert asked me to show Beninson around the interesting places in Stockholm, including the FOA. Dan remembers that he perceived me as a stiff person with no sense of humour, something which he has not failed to remind me of over the years despite admitting that he has changed his opinion.

As for me, I had nothing to do with either ICRP or UNSCEAR in 1956. As I have already said, I was on a trip to the USA and Canada at the time to study the handling of radioactive waste. However, Sievert threw himself into the UNSCEAR work with great enthusiasm and organised a collection of reports at home which would officially be sent from Sweden through the Ministry for Foreign Affairs. His only concern was the travel. Sievert hated travelling and did not trust public transport. It was a fact that aircraft could crash, but he was also worried about the low air pressure (he always had a barometer with him) and thought that flying was uncomfortable with seats that were too narrow for him to sit still in for so many hours. Travelling by boat was more comfortable but he always suspected that the ship could sink; that was what had happened to the *Titanic* after all. Sometimes he booked a seat on both a plane and boat so that he need not make the difficult decision until the last minute.

The Swedish government accepted UNSCEAR as a purely scientific committee. In June 1958, Sievert was notified by Foreign Minister Östen Undén that the 'government was not giving any instructions to its representative at UNSCEAR regarding his point of view'.*

However, it was not just UNSCEAR which was active in 1956. The year was also a significant year for ICRP. During the UN's Atoms for Peace conference in Geneva in 1955, some members of ICRP had contacted Dr. *R. G. Gustavson*, Chair of the board and Managing Director of Resources for the Future, Inc. which belonged to the Ford Foundation, and discussed the possibilities of financial support. Gustavson had later said that the Ford Foundation was positive in its attitude. This had encouraged Sievert's plans for something more prodigious, for the establishment of an international radiation protection academy which, instead of the International Congresses of Radiology, would be the parent organisation of ICRP. He had sent the members a draft with a proposal for such an organisation. According to Sievert's proposal, the new academy would have a secretariat in five places: Braunschweig, London, Paris, Stockholm and Washington. In 1955, Sievert estimated that a starting capital of 5 million dollar would be needed.

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* I received the same notification when I succeeded Sievert as representative in 1965.

The reaction from the British members, i.e., the Chair Sir Ernest Rock Carling, the Secretary Walter Binks and Professor Mayneord, was negative. They thought it would be risky to pull apart everything that had already been built up and thought that ICRP's name aroused such respect that it would not be a good idea to change it. Nor would ICRU like to be swallowed up by the proposed academy, they thought. The three Brits also thought it was inappropriate to seek national support in the five countries in which Sievert had proposed as the locations of the Secretariats. It would be better to concentrate on strong support from the Ford Foundation, wrote Binks.

The Brits received support from Cipriani in Canada, who wrote that it would be difficult to find funds for such a large organisation but that something ought to be done to make ICRP more efficient.

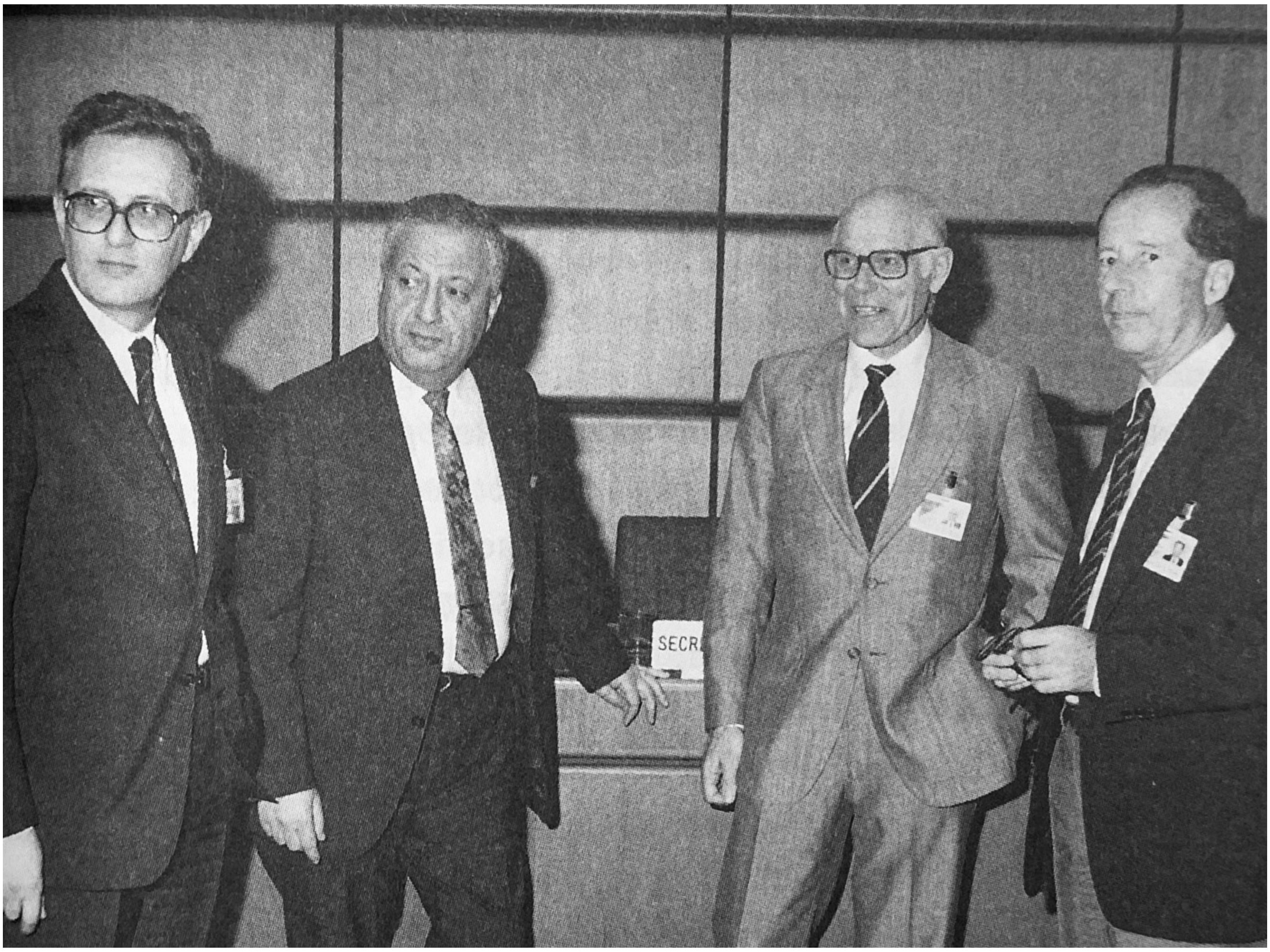
The German members, Jaeger and Holthusen, were not quite as negative but thought it was important to retain the link with the Executive Committee of the Congresses of Radiology. However, many of Sievert's proposals were valuable, they wrote to Binks, and ought to be considered in the attempts to expand ICRP's achievements.

The American reaction was mixed. Karl Morgan was largely positive and saw no risk in changing the name. However, he did warn about changing the organisation which would involve ICRP's freedom being curbed by State influence. Lauriston Taylor, Chair of ICRU, also reacted in a mainly positive way but still thought the proposal unrealistic. On the other hand, he was not worried about ICRU – it would always be possible to solve the cooperation issues. Robert Stone thought the problem was difficult. It would be fatal to make the new academy dependent on a number of countries' governments, but it would also be unfortunate to be solely linked to the Congresses of Radiology since the radiation protection problems were now starting to become so much more extensive. Making an organisation fully independent would also involve risks. Finally, Failla wanted more time to think about the proposal.

While Sievert was sketching out these plans for an international radiation protection academy, talks were taking place between ICRP and WHO on the subject of the best form of cooperation. Those at WHO who had been contacted were the Director General of WHO M. G. Candau, and also (and primarily) the assistant Director General Pierre Dorolle, with whom Sievert had close contact.

On 3 April 1956, a joint meeting of ICRP and ICRU began in *Maison des Congres* in Geneva, with Sir Ernest Rock Carling as Chair and Walter Binks as Secretary of ICRP for the final time. Both the Main Commission of ICRP and its Committees were represented. The list of persons present at this important meeting for ICRP was as follows (members of ICRP's Main Commission are in bold):

Sir Ernest Rock Carling
Walter Binks
Sven Benner
Carl Braestrup
C. E. Eddy
G. Failla
L. H. Gray
Hermann Holthusen
J. C. Jacobsen
Robert Jaeger
H. E. Johns
A. W. Kenny
Wright Langham
John Loutit
W. G. Marley
W. V. Mayneord
J. S. Mitchell
Karl Z. Morgan
W. J. Oosterkamp
E. E. Pandin
Rolf Sievert
Eric Smith
David Sowby
Gordon Stewart
Robert Stone
Conrad Straub
L. S. Taylor
Maurice Tubiana
F. Wachsmann
Harold Wyckoff
J. Zakovsky



Four of UNSCEAR's secretaries: Giovanni Silini, Dan Beninson, Raymond Appleyard and Francesco Sella (the picture is from the 1990s). Photo: the United Nations.

At the time, Sven Benner was a member of ICRP's Committee III on protection against x rays up to energies of 3 MeV, which was something of a strange arrangement since at Sievert's institution he had dealt with radiation protection against radioactive substances. He was later replaced by Lars Lorentzon. Walter Binks' secretarial post was an unpaid honorary task which involved a heavy burden for the conscientious but nervous Binks.

I will give a pretty detailed account of the 1956 meeting in Geneva since it was ICRP's first more comprehensive meeting which was not held during a Congress of Radiology and the first meeting at which plans were made for the continued activities.

The meeting started with a minute's silence for André Cipriani from Canada who had suddenly and completely unexpectedly died seven weeks after he had sent his points of view on Sievert's proposed academy. Cipriani's death had come as such a surprise that, according to his colleagues, his workroom was a confusion of papers about ongoing activities, something which took them some time to sort out. Cipriani was generally missed as a very capable man. David Sowby recalls the following (Sowby, 2001):

> André Cipriani, the medical director of Atomic Energy of Canada Ltd, was the most dynamic individual I have ever met. He was extremely direct, to the point where he could appear to be rude. He was then one of the outstanding international experts in radiation protection and was a member of ICRP. If he had lived he would have had an enormous influence on the way radiation protection developed, but this was not to be. In early 1956, when he was only 49, he died from a massive stroke. His death was a huge loss to Canada and to the international radiation protection community.

At the meeting in Geneva, Maurice Tubiana would leave the Main Commission and be succeeded by Professor Louis Bugnard, who had a post in France corresponding to Sievert's in Sweden. Gordon

Stewart, the Doctor at Chalk River, would replace Cipriani on Committee II, and Canadian David Sowby would replace another Canadian on Committee V. I mention Stewart and Sowby in particular because they ended up spending a few years working together as Chair and Secretary of ICRP during the 1960s.

The meeting was the last of this Commission's meetings before the 8th International Congress of Radiology in Mexico in summer 1956. The chairmanship of ICRP would then transfer from Sir Ernest Rock Carling to Rolf Sievert, who in this connection would be given the Commission's task of continuing the negotiations with the Ford Foundation and WHO. However, what the Commission did not know was that the Secretary, Walter Binks, would fall ill after the Geneva meeting, which would create a great deal of confusion when it subsequently came to documenting what had actually been decided.

Until now, ICRP had had only one meeting every three years in connection with the Congresses of Radiology. Lauriston Taylor established that the rapid appearance of new radiation protection problems would necessitate more frequent meetings. The Committees would also need to meet more often. They realised what a work burden this would mean for the Chairs of the Committees and thought they would need support from Scientific Secretaries of their own choosing, perhaps some younger colleagues at the same institution. It was difficult to see a solution to these problems since ICRP had no financial resources to facilitate a greater work input. The matter would end up being closely linked with the structure of the future organisation and the financing thereof. The task of working with the Chair of ICRU, Lauriston Taylor, to find a solution would fall to the new Chair, i.e., Sievert.

Failla thought that ICRP had now reached a critical stage and must take the initiative of establishing itself as the leading radiation protection authority in the world. This might require greater representation, he thought; for instance, it would be important to find representatives of 'the ordinary everyday person' as well as all experts. ICRP's integrity and reputation as an independent organisation must not come under threat. On the one hand, said Failla, it would be important to make ICRP's programme sufficiently appealing to different patrons for the Commission to be able to obtain adequate finances, but on the other hand, nothing less than full independence would be acceptable.

Lauriston Taylor largely agreed with Failla but was a little concerned about the idea of breaking the link with the Congresses of Radiology (ICRs) and the recently-formed International Society of Radiology (ISR) which would serve between the Congresses. It could do no harm to obtain support from several places such as both the ISR and WHO.

Holthusen agreed with the view that it was time for ICRP to expand its activity beyond the field of medical radiology, but that it could perhaps be worthwhile maintaining the link to the parent organisation, the ICR. Sievert put an end to this discussion by saying that the Commission clearly did not want to limit its field of work but that there was no point having discussions about the way the work was structured until the matter of sponsorship had been solved.

On the following day, 4 April 1956, the Chair, Sir Ernest, reported that Dr. Conrad Straub had been appointed as Chair of ICRP's Committee V on Radioactive Waste. Taylor then gave an account of the ICRU work. The sister commission's view regarding a new future organisation was that ICRP should be allowed to conduct its own negotiations, trusting that what was good for ICRP would also be good for ICRU.

Of the scientific matters discussed by ICRU, one that was noted was the RBE problem, i.e., the matter of the different biological effectiveness of different types of ray in relation to the effectiveness of a reference radiation such as gamma radiation. For radiation protection purposes, the absorbed dose (at the time stated in 'rad', and now in gray) was converted into a dose stated in 'rem' (later called dose equivalent and now equivalent dose) by multiplying it by a weighting factor that was first stated as equal to the biologically-observed RBE value, but was later distinguished from this by means of other denotations (first RBEp – 'P' for protection – then QF [for Quality Factor], then Q and then w_r). For x rays and gamma radiation, the weighting factor was 1.

It was 1956 when an agreement was reached regarding which value this weighting factor ought to have for different types of ray, primarily for alpha radiation and neutrons. Equal amounts of absorbed radiation doses (stated in rad) have different biological effects while equal amounts of weighted doses (stated in rem) were expected to have the same biological effect with regard to determined types of radiation impact. The unit rem has now been replaced by the unit joule/kg, for which the special name sievert (1 Sv = 100 rem) is used when it comes to the sizes of equivalent dose and effective dose, but more about that later on.

After Lauriston Taylor's report on the ICRU activities, discussions returned to the future organisation of the Commissions. On 1 October 1955, Sievert had written to Mayneord who had reacted negatively to the proposals which Sievert was putting forward at the time, starting his letter with the words 'My dear fellow Mayneord, it actually very much saddened me to hear that you were in principle against my proposals'. He then listed the main points of the proposal:

1. Transforming ICRP into an organisation that would be able to function through funds set up with no terms.
2. Increasing the number of members who have competence enough to represent the whole of the radiation protection area.
3. Change of the name, but this was less important.
4. An effort to obtain sufficient money to enable efficient and continuous work.

Sievert then mentioned that he had had high hopes of obtaining money from the Ford Foundation since he had found that Dr. Gustavson's view of the proposals was positive, but that he was now afraid of a setback because 'I know what happens when experts have different views'. He added:

> I sometimes have the wrong ideas in my research but when it comes to financial and organisational matters I feel as though I am 'on my own home ground'.

Sievert then appealed to Mayneord to look at the proposals again and consider which changes he would like to suggest. The letter was then concluded with the following proposal:

> It was a pleasure for me to hear the voice of my old friend Mayneord on the telephone, but it would've been even better to have seen you here. Can't you and Binks take a week's break for talks in Tvartorp? I really do think it would be useful since we must solve the organisation problems now. If we don't, it won't be very long before we see a number of different radiation protection organisations springing up like fungi after the rain from the Conference in Geneva.
>
> I feel it's absolutely necessary that you and I agree. As far as I am concerned, I'm willing to change my view regarding each matter that I don't think is crucial to the realisation of the project.

Sievert had revised his previous proposal for an international academy for the 1956 meeting in Geneva and was now proposing an International Organisation of Radiation Research to which ICRP and ICRU could affiliate themselves. According to Sievert, the proposed organisation would consist of between 40 and 60 members in addition to those who were now more than 65 years old. The proposed organisation would have a board with representatives of at least seven different countries and have full-time secretaries beneath a Secretary General. It would collect literature and information on radiation protection laws and information on accidents and publish a journal, the *International Journal of Radiation Protection*. It would also publish and continuously update a book containing 'recommendations, technical data, practical advice, bibliographies and summaries of important articles, as well as other information on protection against ionising radiation'. It would organise world-wide measurements of the natural radiation environment and the change therein owing to radioactive contaminants, support research and arrange symposia on radiation protection problems. Sievert now estimated that creating such an organisation would require between 20 and 30 million dollars.

These were large-scale plans and Sievert was impatient. Before ICRP's meeting in Geneva, he had written to Binks (on 7 February) that if ICRP could quickly establish the proposed organisation with the help of money from the Ford Foundation, it might be possible to convince the UN to allow it to take over UNSCEAR's tasks in the future. He thought the meeting with Dr. Gustavson in Geneva the previous year had given ICRP 'a very good chance which had diminished with each month that had passed'. He continued: 'Maybe I'm wrong regarding the difficulties of agreeing in a short space of time, but I've always found that the most effective thing to do is to strike while the iron's hot. Gustavson really was very interested, along the lines I'd proposed as well'.

It was agreed that Sievert's proposal for work assignments for the intended new organisation would be accepted with the exception of the measurements of the level of radiation in different countries, since this was something that WHO had plans to carry out.* Since the birth of the organisation would depend entirely on whether or not a financier could be found, however, they would temporarily continue to work as before. The question of the most suitable financing was discussed after that. Should they ask the Ford Foundation for a large enough donation to start with or should they apply for annual grants? Some members thought it would be unrealistic to ask for a donation which was so large that the interest would keep the organisation going. Others thought that just one large donation would make the organisation completely independent. The decision was to write a letter which clearly emphasised the importance of the organisation's independence and autonomy.

ICRP's Committee I met on 5 April, still in Geneva, under the chairmanship of Gioacchino Failla. The most important matter was the way in which the dose limit for radiation workers would be formulated. The applicable recommendations from 1954 (published in 1955 in Supplement No. 6 of the *British Journal of Radiology*) stated limits for the weekly dose (actually the exposure to the surface) in different organs: 300 mr (milliröntgen) for the gonads and blood-forming organs, 600 mr for the skin, and a limit of between 300 and 600 mr for other organs, depending on the depth in the body according to a diagram in the report. In the forthcoming recommendations, the dose limits would no longer be stated in milliröntgen but in the new unit rem, or in thousands of rem (millirem). An mr and an mrem were certainly not exactly the same thing but the difference was insubstantial from the practical point of view, despite the fact that in the one case you were referring to the exposure and in the latter case some sort of equivalent 'dose'.

The subject now being discussed was what was actually meant by a limit of '300 mrem per week'. Was it an average and, if so, over which period of time would the average be calculated? Or should the dose not exceed 300 mrem in any one week? In both cases, it would theoretically be possible to allow 300 mrem each week. The Committee did not think it was dangerous, but did they want to recommend it?

The Committee reached agreement temporarily that they did not actually mean *per week* but in *any one* week, but wanted to discuss the matter further with Committee II.

The next question concerned what was meant by 'significant surface' and 'significant volume' for dose calculations. The Chair thought that a square or a cubic centimetre could be a reasonable estimation and the Committee agreed with him.

The following morning, 6 April, a joint meeting was held by ICRP, ICRU and representatives of WHO, which provided Pierre Dorolle and a number of heads of department, including Doctors I. S. Eve and Irvin Lourie. In this context, Sievert concluded the discussion with Eve and Lourie regarding a grant for me for the trip to the USA a few weeks later to study the handling of radioactive waste. According to Taylor's subsequent report, the meeting brought no surprises but did indicate the desire on the part of both organisations to cooperate in the future.

On the afternoon of 6 April, a joint meeting was held by ICRP's Committees I and II with Failla as Chair once again. The issue of the dose limit was then discussed in a new light, probably following corridor discussions with Sievert who was applying a lower weekly dose limit of 100 mr instead of 300 mr in Sweden. He had not gained support for the lower weekly dose when the matter was discussed in Copenhagen in 1953 but the circumstances did now support him. The rumour held that the question of a suitable dose limit was studied as part of the work with the two 'white papers' which were prepared in England and the USA by the Medical Research Council and the National Academy of Sciences with reference to the concern about the radioactive contamination from the atmospheric nuclear weapons tests. It was seen as pretty certain that, bearing in mind the risk of hereditary injuries, the American

* Such a centre was later created by WHO in Paris under the leadership of Dr. *Pierre Pelleri*

Academy of Sciences would propose an annual dose limit of 5 rem, corresponding to 100 mrem per week.

This expectation worried ICRP members. It would not do for a national organisation to propose a lower dose limit before ICRP did. Sievert and others had also maintained that an annual dose limit of 5 rem (currently denoted as 50 millisieverts) would be a consistent follow-up to Hermann Muller's proposal from the Stockholm meeting in 1952.

Failla referred to Committee I's desire to retain the option of a weekly dose of 300 mrem, but also realised the gravity of the desire to limit the accumulated dose over longer periods of time, e.g., to 5 rem over the space of a year. That would limit the dose over even longer periods of time and would thereby also provide better protection against any risk of leukaemia.

Karl Morgan reminded them that the critical period for a risk of hereditary injuries was in the group of those below 30 years of age. It would be possible to maintain the weekly dose limit at 300 mrem but to set requirements regarding the limitation of the dose that was accumulated within different time intervals: less than 20 years a dose of zero, 20–30 years no more than 50 rem and 30–60 years 150 rem, i.e., a total genetically significant dose of 50 rem and a total by the age of 60 of 200 rem.

The Englishman Marley agreed with this but thought that it would be better to express it as no more than 50 rem before the age of 30 and no more than 200 rem before the age of 60. He was not positive towards an additional limit of 5 rem for the annual dose since, in practice, this would mean a requirement of no more than 100 mrem per week.

Failla pointed out that it was necessary to also look at the practical application and the opportunity for the authorities to set requirements. If there were no annual dose limit and an employer allowed the personnel to receive a high dose in the first few years, would you then be able to keep a tag on the doses of different individuals and ensure that they received a corresponding lower dose in subsequent years? He preferred to recommend an annual dose limit and keep it at one third of that which corresponded to the previous weekly dose limit, i.e., to set the new limit to 5 rem per year. As far as he understood, such a limit would not involve any problem for the atomic energy industry. In his opinion, this was not important with regard to the industry but it was with regard to ICRP's opportunities of having its recommendations accepted. Since ICRP had no power to have its recommendations enforced, it would have to get them accepted on a voluntary basis.

Marley thought it would be more worrying for the healthcare services and radiation therapy than for atomic energy, which already had good radiation protection. Professor Failla thought that, since 0.3 rem per week could become 15 rem per year, this meant too high a risk of leukaemia. Failla thought the risk was no longer as theoretical as had previously been believed.

Robert Stone remonstrated that, unlike the others, he could see no scientific reason to lower the dose limit. Morgan's response was that if the Committees did not lower the dose limit now, someone else would soon do so. Taylor agreed and reminded them that there were many geneticists in UNSCEAR who would be able to proffer good arguments.

Bugnard thought there were psychological reasons for not lowering the dose limit; if it were lowered, ICRP could be criticised for previously having had a three times too high limit. Failla's answer was that there were equally good psychological reasons for doing the opposite: if ICRP did not take the initiative to lower the dose limit, someone else would do so and maybe in an unreasonable way.

Morgan thought that lowering the dose limit would reduce any shortened lifetime owing to radiation. Stone countered that if you wanted to eliminate all risks, you ought also to ban construction workers from climbing up scaffolding.

The notes from the meeting show no clear opinion. Failla proposed an annual dose limit of 5 rem whereas Bugnard and Marley wanted a weekly dose limit of 300 mrem and additional limits for accumulated doses up to 30 and 60 years of age. Failla asked if this meant a limit of 200 rem (corresponding to 2 sieverts) for the lifetime dose and Stone said he wanted it noted that he was against any change at this stage.

Finally, Failla asked whether they should introduce a 'genetic dose limit' of 10 rem per generation for the public. Taylor's response was that they should not allow themselves to be steamrollered by the geneticists; neither they nor anyone else would have an answer to the question within the next twenty years.

The next day, 7 April, a joint meeting was held by ICRP and ICRU. It now appeared that the total that could be hoped for from the Ford Foundation was between 100 000 and 200 000 dollars per year for some years to come, so a total of no more than one million dollars, far from the 20–30 million dollars that Sievert had calculated as being needed. Concern was also expressed about what the Society of Radiology (ISR) would say if ICRP were to receive its main support from elsewhere. Taylor later wrote: ‘This again raised the question which had come up at nearly every meeting, namely the suggestion that the commissions should separate themselves from the sponsorship of the International Society of Radiology. As before and since, the idea was turned aside’. Hermann Holthusen was asked by both of the Commissions to negotiate with the ISR for financial support.

Sievert was confirmed as being elected as the new Chair to start on 1 July of the same year (1956) with Failla as Deputy Chair and Walter Binks re-elected as Secretary. The number of members of the Committees was increased. From 1 July 1956, ICRP was made up of the following:

The Main Commission:

Rolf Sievert, Chairm.
G. Failla, Dep. Chairm.
W. Binks, Secr.
L. Bugnard
H. Holthusen
J. C. Jacobsen
R. G. Jaeger
W. V. Mayneord
K. Z. Morgan
L. S. Taylor
E. A. Watkinson

Sir Ernest Rock Carling
(Chairm. Emeritus)

Committee I:

(External radiation)
G. Failla, Chairm.
A. R. Gopal-Ayengar
Gert Bonnier
L. Bugnard
D. G. Catcheside
J. C. Jacobsen
T. Kemp
R. Latarjet
J. F. Loutit
H. J. Muller
Jens Nielsen
Rolf Sievert
R. S. Stone
Shields Warren

Committee II

(Internal radiation)
K. Z. Morgan, Chairm.
Walter Binks
A. M. Brues
W. H. Langham
L. D. Marinelli
W. G. Marley
M. K. Nakaidzumi
G. J. Neary
M. N. Pobedinsky
E. E. Pochin
C. G. Stewart

Committee III:

(x-ray and γ sources)
R. G. Jaeger, Chairm.
E. E. Smith, Dep. Chairm.
Sven Benner
J. Bouchard
C. B. Braestrup
B. Combee
C. Garrett
T. Gauwerky
H. Holthusen
P. Rønne
D. J. Stevens
H. O. Wyckoff
J. Zakovsky
A. Zuppinger

Techn. Secr.: W. Hübner

Committee IV:

(High energy radiation)
H. E. Johns, Chairm.
J. S. Mitchell, Dep. Chairm.
L. H. Gray
F. Hercik
G. Joyet
W. H. Koch
J. S. Laughlin
W. V. Mayneord
C. A. Tobias
M. Tubiana
F. Wachsmann

Committee V:

(Radioactive waste)
C. P. Straub, Chairm.
E. E. Pochin, Dep. Chairm.
H. P. Jammet
A.W. Kenny
W. G. Marley
C. A. Mawson
A. Perussia
Edith Quimby
F. D. Sowby
F. W. Western

Techn. Secr. G. G. Robeck

These lists of people require a few notes of explanation. The list of Committee members' names includes the names of members of the Main Commission; this is no longer the case although, at the time of writing, members of the Main Commission may, if they so desire, participate in the Committees' meetings. The Chairs of two of the Committees were new: H. E. Johns had succeeded Mayneord as Chair of Committee IV, and Conrad Straub had succeeded the deceased Cipriani as Chair of Committee V. The fact that two of the Chairs were not members of the Main Commission ended up leading to practical difficulties.

Dr Eddy's name is missing from the Main Commission; he unfortunately died before the new Commission had arranged to meet. For the first time, Committee members included a Russian (M. N. Pobedinsky of Committee II). The new set-up included three Swedes, as well as Sievert, Gert Bonnier and Sven Benner.

At the start of the meeting in Geneva, the hope was that the result would be fairly straightforward additions to the recommendations in the 1955 *British Journal of Radiology*. However, the changes that were starting to be made meant that considerable paraphrasing was required. Not only that, the modus operandi until now had meant that the Committees had been very independent. The 1955 supplement to the *British Journal of Radiology* stated the most important recommendations (e.g., regarding the dose limit) in Committee reports and not in the report from the Main Commission. At this point they were not sure where they were going. Failla's and Morgan's Committees might have different views and Morgan was known for his stubborn way of sticking to his opinion. Increasing pressure began to fall on the Main Commission to take over the direct responsibility for ICRP's policy and give the Committees instructions as to which policy they ought to follow. This way of working gradually came to fruition, but for a number of years, Karl Morgan's Committee II was a 'state within the state' with a partially non-conforming policy.

The discussions in Geneva in 1956 had demonstrated the problems, but the solutions were not obvious, so a decision was made to hold a meeting with the Main Commission and the Committees on Monday 9 April to summarise what had been concluded. Sir Ernest said that the new Commission with Sievert as Chair would meet sometime in October. After that, the five Committee Chairs reported on what had happened the previous week.

Failla did not think that the decisions from Committee I would be difficult to present. However, bearing in mind the genetic risk, he did think that the increasing presence of sources of radiation would necessitate the recommendation of some form of limitation to the dose for large populations. The Committee had not arrived at any proposal for this.

In the discussion, it was said that the previous recommendations had been unclear as regards any difference between the expressions 'large populations' and 'whole populations'. The 1954 recommendations had stated that 'in the case of the prolonged exposure of a large population, the maximum permissible levels should be reduced by a factor of ten below those accepted for occupational exposures'.

It was now being said that with this recommendation, ICRP had not meant 'the whole population', and this meant that the annual dose for the public could go up to 1.5 rem (15 mSv). Criticism came from geneticists, who multiplied this annual dose by 30 (the number of years for each generation) and deduced an implicit permitted generation dose of 45 rem, considerably higher than what Muller had spoken of in Stockholm in 1952. During the discussion in Geneva, it was said that the intention had never been for each individual person to be permitted to receive 1/10 of the dose limit for workers. It was now thought that the intended meaning was instead the maximum dose that any individual would be permitted to receive. The average dose in the whole population could be expected to be an additional one tenth thereof, that is to say at 1/100 of the dose limit for workers, i.e., at 150 mrem (1.5 mSv). This would mean a generation dose ('genetic dose') of 30 * 0.15 = 4.5 rem.

It was agreed that a statement was needed to clarify this. It should also mention that Committee I did some work on the issue.

The matter of the way in which the dose limit for workers should be expressed was then discussed again, firstly in connection with Failla's report on Committee I's conclusions and immediately thereafter in connection with Karl Morgan's report on Committee II. The first discussion was a lengthy one with a number of different proposals. An acceptable formulation was eventually voted for. The text read:

> In order that the maximum permissible weekly doses be not exceeded* and that the spirit of the general recommendation 'that exposure to radiation be kept at the lowest practicable level in all cases' be adhered to, general experience indicates that the average yearly dose received by those occupationally exposed will not normally exceed 1/3 of the maximum permissible limit. It is felt that it would be prudent to limit accumulated doses to 50 rems by the age of 30, and beyond that age to average doses of 50 rems per decade.

This is an example of the linguistic monstrosities which, when tired due to long discussions, Committees can suddenly reach agreement on. The grammatical and logical shortcomings of the first sentence already existed in the original text. Luckily, as far as I am aware, the text was never published by ICRP. It was a compromise to save the weekly dose limit of 300 mrem and simultaneously try to convince the critics that they actually wanted an average weekly dose limit of 100 mrem, at least for those who are below the age of 30. They had still not determined an annual dose limit.

Karl Morgan explained that Committee II did not think that the text from 1954 needed to be rewritten, although the large tables containing nuclide data and the values for maximum permissible concentrations (MPC) and body burden (q) should be revised. The conditions should be a weekly dose limit of 300 mrem for all organs except for the gonads, for which 100 mrem would be used. With whole body irradiation, the weekly dose limit should also be 100 mrem. This would reduce the risk of leukaemia and a potential lifetime shortening. The limit for bone-seeking nuclides should be based on comparisons with the well-established limit for the body content of radium-226, i.e., 0.1 microgrammes.

Committee II intended to extend the previous tables with a further 70 nuclides. Failla emphasised how important it was for Committees I and II to keep in contact in the future so that they did not develop different policies.

Robert Jaeger reported on Committee III's proposed recommendations for residual radiation through tube protective housings and protective containers for sources of gamma radiation. In off mode, it should not be possible for the average exposure (over different directions) per hour at a distance of one metre from the source of the radiation to exceed 2 milliröntgen, and by the surface of the protective housing or the container it should not exceed 30 milliröntgen. It should not be possible for the maximum doses (in a specific direction) to exceed five times these values.

The Committee intended to expand the recommendations on protection during x-ray crystallography work. Equipment for such purposes ought to be made to be fully protective because in this context you could not rely on any warning from personal dosimetry or blood examinations.

The Swedes put forward a proposal to increase the requirements for protection against x rays from televisions. They wanted a lower limit for the exposure rate close to the device, 0.1 microröntgen per second rather than the valid recommendation of 0.6 microröntgen per second. The Dutchman Oosterkamp from Philips was against a change, saying that it would scare people. The Committee wanted to refer the matter to Committee I which had better biological expertise. During the discussion, Failla pointed out that it was not just the dose rate next to the device which was of interest, but also the dose that the observers could receive at the distance where they were sitting. It was certainly very small, thought Failla, but it might need to be limited in any case, bearing in mind the contribution to the genetic dose. The dose rate ought also to be stated as an average dose per hour rather than in microröntgen per second.

Professor Mayneord reported from Committee IV and felt that there were so many new factors that a completely new report would be needed. The RBE problems required a better level of investigation for the Committee to work on recommendations for work with high-energy radiation and neutrons.

Conrad Straub, who ended up taking over Committee V following the sudden death of Cipriani, also thought that a completely new report was needed here.

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* Regarding ICRP's expression 'Maximum Permissible Dose': that which is permitted is determined by responsible authorities. ICRP's recommendation concerned the maximum which ought to be permitted.

Following these messages from the Committees, Marley discussed the matter of extended holidays for those working in a radiation environment. He referred to the fact that a recent ILO publication recommended that radiation workers ought to work for a shorter period of time, as well as have extended holidays. ICRP had not expressed an opinion on this matter and should perhaps do so, one way or the other. Taylor referred to discussions in the USA where the conclusion had been that, with a 40-hour week and the application of ICRP's recommendations, no further safety measures were necessary. In addition, it was not as though an extended holiday could be seen to provide protection against radiation.

The Chair asked about the situation in the Soviet Union. Pobedinsky's answer was that the importance of the holiday had been studied and it had been found that the workers' health was improved by the holiday. The working period was kept at 40 hours per week by limiting the number of days. The holiday for radiation workers was 36 days a year. Instead of a weekly dose limit, there was a limit for the daily dose, which could not exceed 50 mrem (0.5 mSv).

Rock Carling, Bugnard, Failla, Jaeger, Mayneord, Morgan and Stone met on Tuesday 10 April. ICRU met at the same time, which meant that neither Sievert nor Taylor could be there. Binks had probably already become ill by then and it is unclear as to who took the minutes from the meeting which Lauriston Taylor reproduces in his large compilation of texts (Taylor, 1979).

From these minutes, it is very difficult to gather what was actually decided and the extent to which the decisions made would stand their ground because not all of the Commission had been present. Some of the previous decisions were torn up and some were reformulated. However, none of this is particularly relevant since, following many tribulations, a report was finally published in December 1957 in an attempt to summarise the viewpoints. The main reason for the confusion appears to have been that Walter Binks had become ill, along with the fact that none of the minutes had been processed immediately. In turn, the report that was issued in December 1957 (and to which I will eventually return) soon became outdated due to the appearance of the new, more thought-through recommendations which the Commission approved in 1958 and which were the first to be published in the ICRP series of reports through Pergamon Press.

On 19 June 1956, a troubled Herbert Parker in Hanford wrote a letter to Lauriston Taylor in which he expressed his concern about what he had heard:

> I was horrified to discover in one of the circulating newsletters on atomic energy that L.S. Taylor had recommended that the permissible exposure to radiation be reduced to one-third of its present value. I hope you were misquoted!
>
> If this means a reduction to 0.1 r or 0.1 rem per week, the effects on the atomic energy program could be extremely drastic. In our telephone conversation this week, I thought we had established that no change from the 0.3 r per week was in sight.
>
> Now I find K.Z. Morgan advocating 0.1 rem per week in his committee on internal dose (ICRP). It is most important that we establish that 0.1 rem per week is nothing more than a convenient going rate. With short-lived emitters or those easily eliminated, there is no need at all to keep to this limit in order to meet the total accumulated dose criterion of 50 r up to age 30.
>
> We cannot overemphasize the needless waste of funds that would go to meet a 0.1 r per week limit, compared with the present 0.3 r per week. The move should be strongly resisted, unless of course there should be technical evidence indicating its necessity. Provided these low limits to age 30 rest solely on genetic needs, there can be no such evidence.

In the end, it was decided that Lauriston Taylor would represent ICRP at the 8th International Congress of Radiology in Mexico City in the late summer of 1956. Sievert did not take part in the Congress, despite having taken up the post of Chair of ICRP on 1 July. Lars-Eric Larsson and Sven Benner were there, however. Taylor went to the Congress of Radiology armed with not just the tidings of Cipriani's death, but also the information that Professor Eddy had died. He faced many questions there and, on arriving home, about what had actually been decided in Geneva. The report he was able to give the Congress did not provide sufficient information.

While ICRP met in Geneva, Swedish scientific delegations took trips to the Soviet Union. Arne Hedgran was one of the participants. When I asked about the trip, he answered:

> So, you're asking about my Soviet trip. It was the most dramatic trip I've been on. It was part of a visit at the highest level. It took place in April 1956. First of all, a political group including Erlander, Hedlund, etc. travelled out. A week later, they came home and the scientific group travelled out and we met at the same hotel in Leningrad. The scientific group included Alfvén, Bonnier, Torsten Gustafson, Kaj Siegbahn as well as Nils Göran Sjöstrand and me as substitute. Aina Erlander had a cold in Leningrad but Torsten Gustafson sat on her bed and was given news – not exactly trivial matters either. We started hearing about Khrushchev's speech condemning Stalinism a few months earlier.
>
> I was with Gert Bonnier - a very nice person. He was prepared to help Dubinin*. He also somehow succeeded in arranging a meeting with him. He offered him strains of mice, etc., and this had some effect. Sievert was meant to have taken part in the meeting but was prevented by his international undertakings.

When Rolf Sievert succeeded Sir Ernest Rock Carling as Chair of ICRP in summer 1956, he inherited a chaotic situation. Walter Binks, who was formally re-elected as Secretary, was ill after the meeting in Geneva. The temporary Secretary was Eric Smith, who was Binks' deputy at the English radiation protection institute, called the Radiological Protection Service (the RPS) and established by the Ministry for Health in 1952. Smith had definitely been at the meeting in Geneva but was not in the Main Commission and had therefore not taken part in the important concluding meetings.

Sievert had great respect for Binks and agreed with the arrangement of having a Secretary in England, but he did not know Smith that well. The uncertainty about what had actually been decided in Geneva worried him, and he felt isolated from the decision-making. At the same time, he did have many irons in the fire, particularly the efforts to create an international radiation protection organisation with greater operative resources than ICRP. This also needed points of contact with UNSCEAR and with Sievert's friends Failla and Taylor in New York. To Sievert, the Secretariat in England was something to worry about. Not only did Bink's illness mean that it was inefficient, as far as Sievert was concerned, it was affected in an unclear way by the English ICRP members, i.e., Mayneord and Sir Ernest as well as Binks. Mayneord was an old adversary whom Sievert both esteemed and mistrusted and who amused himself by levying criticism in the form of understatements. The American members with their more abrupt manner were more to Sievert's taste.

After coming home from Mexico and on the way to a meeting in San Francisco, Taylor dictated a letter to his Secretary in Washington for Sir Ernest Rock Carling. The letter expressed his worry about the fact that the Geneva decisions had not been made public, particularly with regard to the dose limits, and Taylor thought that the silence was a threat to ICRP's prestige. Rock Carling's answer was that he had difficulty taking action with Binks ill and Mayneord also on the sick-list. He referred to the fact that Eric Smith was temporarily acting as the appointed Secretary of ICRP.

UNSCEAR was giving signs that the Committee was considering inviting ICRP and ICRU to give advice on the way in which the doses of radiation for patients undergoing x-ray examinations ought to be estimated. Earlier in the year, the comprehensive report by the American Academy of Sciences had focused the public's attention on the fact that the greatest contribution to the 'genetic population dose' over and above that which originates from natural sources of radiation came from the medical x-ray diagnostics. UNSCEAR's Secretariat now needed advice on how this contribution should be estimated in order to be relevant to risk assessments.

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* N.P. Dubinin was a Russian geneticist who had fallen from grace during the period when the Soviet rulers supported the notorious *Trofim Lysenko* (1898–1976), who maintained that acquired characteristics could be inherited, a theory which was scientifically untenable, albeit politically viable. In 1956, Khrushchev dismissed Lysenko from his post as President of the Soviet Academy of Agriculture but was restored to favour in 1958 to finally be forced to leave all positions of power in 1964 when Khrushchev was dismissed.

Sievert and Taylor had agreed that it would be a good idea to hold a joint meeting with ICRP and ICRU in New York at the time of UNSCEAR's 2nd session which was intended to take place in the UN's headquarters from 22 October until 2 November 1956.[*] They asked the new UNSCEAR Secretary Raymond Appleyard if he could help the Commissions to meet in the UN's building on 31 October so that any questions about the expected assignment could be answered quickly. But Appleyard was negative. He wrote the following in a letter to Walter Binks:

> The proposed joint meeting of ICRP and ICRU on October 31st conflicts directly with the last few days of meetings of the Scientific Committee *[i.e., UNSCEAR]*. We have already stated to delegates that these meetings will continue for two weeks, which will take us to Friday, November 2nd or the next day. For the sake of our common membership, I therefore hope you will see your way clear to avoiding such a conflict. I have in mind especially that Sievert, as the delegate who represents the Swedish Government, could hardly wish to be absent during the terminal plenary session of the Scientific Committee.
>
> I have explored the possibility of assisting your joint meeting from here, and regretfully, must give you an emphatic negative. The United Nations has a great many non-Governmental Organizations in consultative status. If we were to open the doors to one, we would have to do the same for all and the position would become intolerable. The position is, of course, quite different from that which pertains to a meeting of experts under the proposed contractual arrangement, since there I can act with the authority of a General Assembly Committee in order to get what it explicitly wants in the most expeditious manner. I am sure you understand that the negative to your joint meeting implies no lack of goodwill towards the International Commission—indeed, the reverse, for I want nothing to prejudice our support of you in the areas where perhaps we really can give it.

Since Dr. Eddy had died, Professor *Carlos Chagas*[†] from Brazil was elected as the new Chair of UNSCEAR. At this meeting, the Committee made a departure from its custom of only reporting and not recommending in that it formulated letters to be published in the medical press and for special radiological journals. It was pointed out that a substantial share of the radiation to which humans are exposed comes from x-ray examinations, and that it was therefore important for all forms of medical irradiation to be limited to those which really were important to diagnosis and treatment.

Despite Appleyard's warnings, the joint meeting of ICRP and ICRU took place on 31 October 1956, but in Failla's library at the Columbia University Medical Centre in New York. The following were present:

From ICRP:		From ICRU:
Rolf Sievert, Chairm.	Hermann Holthusen	Lauriston Taylor, Chairm.
G. Failla, Deputy Chairm.	K.Z. Morgan	Harold Wyckoff, Secr.
Eric Smith, Temp. Secr.	Robert Stone	André Allisy
Louis Bugnard	E. Watkinson	Felix Leborgne (Uruguay)

Dr. Pochin, who was UNSCEAR's British representative this time because Professor Mayneord was ill, had also been invited as a substitute for Mayneord and to account for UNSCEAR's plans.

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[*] It may be worth mentioning that UNSCEAR's meeting coincided with the major international crisis which comprised the uprising in Hungary (on 23 October) and the Suez crisis (Israel attacked Egypt on 29 October with the support of the UK and France). The Hungarian uprising was crushed by the Russians on 4 November and the USA and the Soviet Union forced the UK and France into a ceasefire on 6 November. However, in the report from the meeting, the Swedish delegation wrote: 'The tense foreign policy position, which arose during the ongoing session, and the forthcoming American Presidential election [President Eisenhower was re-elected] did not have a noticeable effect on the Committee's work'.

[†] Many people were incorrect in thinking that Chagas was the person who had given his name to Chagas Disease, an infectious disease which is transferred by bloodsucking bugs and which affects a good ten million people per year in South America. However, the disease is named after an older Chagas.

Sievert recounted the discussions with the Ford Foundation concerning possible economic support for a larger international organisation and said that he expected an opinion soon. Due to deliberations with the Ford Foundation and the work in UNSCEAR, he was unable to take part in the continued meeting and handed the Chairmanship over to Taylor.

Dr. Leborgne was concerned about the worry which had become the consequence of the essay recently published by Shields Warren in the *Journal of the American Medical Association*. Warren had studied the life expectancy of doctors of different categories and thought he had found that radiologists had a shorter life expectancy than other doctors. Leborgne thought that this would make it more difficult to interest doctors in becoming radiologists. He thought it was important for ICRP and UNSCEAR to investigate this in greater depth (see also Chapter 12).

The first day of the meeting was taken up by discussions about the matters which had been dealt with at ICRP's meeting in Geneva. Failla emphasised that the meeting was now scarcely competent to make decisions and that, until further notice, ICRP's recommendations were as they had been stated in Geneva. Whatever it was possible to conclude in the discussion now had to be given to those who were not present for approval or only be considered to constitute an interpretation of the Geneva recommendations.

It had been agreed that the risk of hereditary injuries meant that the accumulated gonadal dose in radiological work up to the age of 30 ought to be kept below 50 rem (i.e., 500 mSv in current units). Karl Morgan pointed out that this was all well and good as a principle, but that a limitation to 100 mrem per week was needed for construction and planning. The applicable limit value for the weekly dose was 300 mrem (for each individual week). Should they continue to allow the limit to apply to weeks or years? An average of 100 mrem per week would give an annual dose of 5 rem. Was this to become ICRP's new dose limit?

There were also discussions as to the approach to doses of radiation after the age of 30. Would higher doses be acceptable then? The following interpretation was agreed:

> *Basic principle*: A total of at most 50 rem up to the age of 30 and thereafter no more than 50 rem per decade;
> *Operational*: An average of at most 5 rem per year, but no more than 10 rem in one year or 3 rem per 3-month period;
> *Weekly limit*: At most 300 mrem in a week.

The next question was the limitation of the dose of radiation to the whole of the population. Both the British Medical Research Council and the American Academy of Sciences (NAS) had recommended a limitation of the average gonadal dose for the public up to the age of 30, which meant a maximum addition of 8 rem (MRC) or 10 rem (NAS) in addition to the doses of radiation from natural sources of radiation but including the doses of radiation from medical examinations and treatments, i.e., a total of 12 or 14 rem because 4 rem were anticipated from the natural sources of radiation. The American advisory National Committee on Radiation Protection and Measurements (of which Taylor was Chair) had proposed a total of 13 rem, including all sources of radiation. The members of ICRP in Failla's library did not want to set a limit for the dose of radiation from medical uses; they thought that the dose from other sources ought to be kept lower than a total of 4 rem in addition to the dose from the natural sources, which could be expected to keep the total 30-year dose below 11 rem. All the groups who discussed the matter had thus arrived at approximately the same result.

The meeting continued on 3 November 1956, although not with exactly the same participants, i.e., without Sievert but also without Allisy and Pochin. UNSCEAR had now given them the task and 10 000 dollars to be able to implement it. UNSCEAR had requested a report for 1 September 1957, which did not give much time for an assignment of the size concerned. With its first report, which should be ready in 1958, UNSCEAR wanted a scientific Appendix containing details of the radiation doses to patients who had been exposed to medical examination or treatment. Above all, the gonadal doses from x-ray examinations should be addressed. UNSCEAR's Appendix should also be structured so that it would be possible to derive genetically relevant doses (see later on) and perform risk assessments.

It was obvious that the collection of data would be a task largely for UNSCEAR, but the work group from ICRP and ICRU was expected to give instructions concerning which information was needed and how it would be collected. This, too, would involve a great deal of work.

Eric Smith was startled as to the burden this would involve for ICRP's as yet practically speaking non-existent Secretariat. Being Secretary had been an honorary task for some of the Commission's members and now most recently for Binks. The situation was the same for ICRU, where Wyckoff was Secretary. Due to the increase in the workload, ICRU had proposed that, in addition to the Secretaries the Commissions would have *technical secretaries* to do most of the work. Smith now wondered how ICRP intended to solve the problem. According to Taylor (Taylor, 1979), Smith said:

> Many of the difficulties of ICRP were due to the lack of a paid organization to carry on the extensive secretarial and editorial work. This is a special hardship in view of the more complicated reports being developed. With the additional work being proposed for the Commission, consideration should be given to providing the secretariat with some assistance in maintaining a technical staff.

Taylor himself continues his account of the meeting:

> At the moment, the rules do not limit the term of office of the Chairmen of the Commissions. There was *[during the meeting]* some feeling that the chairmanship should be limited to one term. With a change in chairmanship, the continuity of the work would be in the hands of the technical secretary. However, if the financial negotiations are successful, a permanent secretariat would be established which would have responsibility for the continuity of the records, and the geographic location of the chairman and secretary would not be of concern.

Taylor clearly had ICRU's proposal in mind, i.e., an honorary secretary from among the members plus a technical secretary who carried out the work.

The meeting took place on 3 November and continued on 4 November. Significant time was spent discussing the composition of a work group (Joint Study Group) for the UNSCEAR assignment and what the best arrangements would be for the work. It was proposed that the group's first meeting take place in Geneva on 23–27 April 1957.

It was suggested that the UNSCEAR study should be led by a group consisting of the Chair, the Deputy Chair and the Secretary (in the case of ICRP, Binks) for the two Commissions as well as André Allisy, Greg Marley, Karl Morgan, Eric Smith and Ernest Watkinson, with Smith as Secretary of the study group. Assistance and advice would also be sought from Hermann Holthusen, Bo Lindell, Lars Lorentzon and David Sowby.

The assignment from UNSCEAR affected the painstaking Walter Binks badly and probably made him panic in the face of all the work that beckoned. On 14 January 1957, Sir Ernest wrote and told Sievert that Binks was not well and probably should step down as Secretary of ICRP. Sir Ernest thought that if Binks' illness meant that a membership gap had to be filled by an Englishman, Dr. Pochin would probably be the best choice. However, the new Secretary ought to be Swedish, he wrote, in order to be in a better position to help Sievert. And this is when the first proposal was made to have a Secretary who was not a member of the Commission.

On 16 January, Binks wrote to Sievert and resigned from his position as Secretary. His doctors had said that he could no longer do the work that he had previously been capable of and that he could not cope with drawing up new ICRP recommendations.

On 21 January, Binks wrote again to Sievert and said that he had written to Appleyard and asked about the 10 000 dollars which had been promised to the study group to enable the planning for the latter's meeting in Geneva in April to continue. Binks said that, if Sievert so desired, Eric Smith was willing to come to Geneva to 'take on the tasks that would have fallen to me had I been in a position to travel to Geneva as Secretary of ICRP'.

On 24 January, Sievert answered Binks' letter, regretting his decision but wondering whether or not the Congress of Radiology's Executive Committee had to formally approve changes to the Secretaryship. Sievert added that Lindell was temporarily dealing with the correspondence.

Binks' response to this on 14 February was that the Executive Committee's rules said that the Commission itself could fill vacancies that arose during the period, and that this might also apply to the Secretary. It was OK for Lindell to be handling the correspondence, but this did not offload the heaviest workload, that of coordinating the Committees and ensuring that their reports tallied. Binks wrote 'The only solution is that I step down altogether for someone else to deal with the heavy work'. He had not yet received the reports from Committees II, III, IV and V. In the end, Binks asked Sievert to discuss the matter with the Commission.

On 26 February 1957, Sievert wrote to Binks stating that 'Lindell is the only person here in Sweden who is capable of taking on the work', but that a small country like Sweden should not have two members in the Commission. He therefore suggested a temporary solution until 1959. A draft of a circular was added to Sievert's letter and was also sent to Failla and Taylor for comments. I took Sievert's flattery with several pinches of salt; I realised that it was not just his way of expressing appreciation; it was also mainly because Sievert wanted a reliable Secretary nearby. There were several reasons why Sievert wanted to fortify the contacts with America, and the circumstances were now playing into his hands.

On 4 March, Failla's response was that he accepted the circular and the proposal for me to be Temporary Secretary, and on 14 March Binks stated that he had consulted Sir Ernest and Professor Mayneord and that Sievert's proposal was accepted as far as the British were concerned. Sievert issued the circular on 22 March.

The Commission's response was positive and it accepted me as 'Temporary Secretary'. However, Morgan emphasised that the work would not be easy. He asked: 'Will Dr. Bo Lindell be willing to sacrifice many months of hard work with preparing the final draft of the ICRP report?' And the final thing he wrote was:

> To summarise, I would be happy to see Dr. Bo Lindell nominated to serve for the remainder of Dr. Binks' period provided that 1) Dr. Binks believes he is going to resign from his post and 2) Bo Lindell is willing to take on this very difficult editorial task.

While Sievert was concerned about the ICRP Secretariat's work capacity, Harold Wyckoff issued a circular which provided information about ICRU's work information and the Chair of the Committee. Radiologist Martin Lindgren from Lund was a member of the ICRU sub-committee III on dosimetry with Gray as Chair. A Finn, now living in Sweden, on the ICRU's Committees was the Chief Physician at Roslagstull Hospital, Carl Wegelius. Professor of Medical Radiology in Åbo. At the time, the ICRU's Main Commission also included a Swede, Rolf Sievert. There were also four members who were also members of ICRP; their names are in bold in the following list of the ICRU members from 1956–1959:

L. S. Taylor, Chair.
L. H. Gray, Deputy Chairm.
H. O. Wyckoff, Secretary
A. Allisy
R. H. Chamberlain
F. Ellis
G. Failla
H. Holthusen
H. E. Johns
F. E. Leborgne
W. J. Oosterkamp
B. Rajewsky
R. M. Sievert

My own initial indirect contact with UNSCEAR was not until spring 1957. Sievert's conflict with his supervisors had intensified in that Matts Helde had given a separate statement in the Radiation Protection

Committee's report on the organisation of the Institute of Radiophysics. I had not formed an opinion of the conflict. Sievert was certainly acting arbitrarily like an old-fashioned patriarch, but I thought that the complaints against him were petty and concerned trifling matters. The idea of a Sievert who did not tolerate criticism did not match my idea of him; my impression of him was that he liked being contradicted if he found the criticism intelligent and above board. He seemed to value *my* work although I often contradicted him. On occasions when he might sound cross with me there was always an explanation that he did not want me to be accused of being shown favour, although I am sure that this was in fact the case.

When Sievert saw that I might perhaps be accepted as Temporary Secretary of ICRP, he asked me to travel to Switzerland with Lars Lorentzon to take part in ICRP and ICRU work group's meeting. The meeting was to be hosted by WHO in Geneva, where Sievert had already been for talks with higher WHO officials concerning the options of support for ICRP. Lorentzon and I went by train through Germany, a pleasant trip during which he gave me lessons in German wine and beer customs.

As well as us and Sievert, Lauriston Taylor, Harold Wyckoff and Karl Morgan from the USA, Harold Gray and Eric Smith from England, David Sowby from Canada and Hermann Holthusen from (the former) West Germany also took part in the meeting. Since Holthusen was able to stay for only half a day, it became a very Anglo-Saxon meeting. All participants lived at the Eden Hotel, an old and not particularly comfortable hotel for which the British had developed a liking for some unknown reason. And there were British as well. One day I hurried up the stairs one floor too far and rushed into what I thought was my own room. The door was unlocked and in the middle of the floor stood a half-naked gentleman performing gymnastic movements. When I rushed in he did not bat an eyelid, instead saying in a friendly tone: 'Would you like to join me?' That was the way the Eden Hotel was.

Outside the hotel was a small park with a café. This was where I met Karl Morgan and Eric Smith on the first day over a glass of beer, as well as the latter's assistant, Maureen Walton. David Sowby, who would be Secretary of ICRP five years later, was also a new acquaintance. I was impressed by the effortless way in which he could tell stories to the other people in the gathering, and in a situation where, at his age, not yet having reached the age of thirty, I would not even have dared to open my mouth.

We ate dinner together at *Plat d'Argent* up in the old part of town. Karl Morgan, who was late, arrived by taxi and opened the car door so carelessly that it was knocked off by a passing car and an argument developed between the taxi driver and Morgan. This was not the only thing that would etch the image of K. Z. Morgan into my mind. I still remember how, on a freestanding blackboard in the WHO conference room, he tried to derive a formula to calculate the genetically significant dose. I have a photograph of the blackboard and Morgan's formula. Unfortunately, it was incorrect and remained incorrect in the report which was published by the work group later that same year.

Let us look at the concept of 'genetically significant radiation dose' (GSD) in more detail. Geneticists who study hereditary changes following irradiation have often made their observations on fruit flies or mice and have used these to draw conclusions as regards what can happen to humans. The geneticists have been interested in equilibrium situations where people have exposed populations to an extra dose of radiation generation after generation. The doses of radiation which interest them are obviously doses of radiation to reproductive individuals.

On the other hand, from the radiation protection point of view, we are usually dealing with human populations in which all ages are represented and where sometimes one part, sometimes another part of the body can receive doses of radiation from different sources of radiation. When it comes to hereditary harmful effects, within reason it is only the dose to the sex organs, the gonads, which is relevant. So, in order to estimate these risks, we would like to know which gonadal dose different individuals have received. I have already discussed older dose concepts and dose units. In the 1950s, people wanted to estimate the dose for radiation protection purposes in the form of a 'dose equivalent' and state it in the unit 'rem'. If you multiply the number of irradiated individuals (N) by their average gonadal dose, you obtain a product that used to be called the 'population dose' and which was expressed in man•rem.

However, a population dose for the gonads is a nonsensical concept for a geneticist because it means that we have also taken into account gonadal doses to individuals who are too old to be able to expect to have children. We therefore have to find some way of completely discounting the doses of radiation for people who are too old. This can be done by multiplying their doses of radiation by zero. But there are also 'middle-aged' individuals who are only half as likely to have children compared with the very youngest. The calculation ought then to cut their doses of radiation by half, i.e., they should be multiplied by 0.5. The doses of radiation to the very youngest ought to be fully taken into account, i.e., multiplied by 1.0. In doing so, we can calculate a corrected population dose by weighting each individual gonadal dose with a number which states the individual's relative expectation of having children.

The genetically significant dose is defined as '*the gonadal dose which, if received by each member of the population, is expected to cause the same total hereditary injury within the population as the gonadal doses which the various individuals have received in reality*'.

A genetically significant dose from x-ray examinations and from work with radiation had already been estimated for England and Wales by *Sidney Osborn* (1918–) and Eric Smith in 1956. Unbeknown to ICRP/ICRU group at the time, the Japanese physicist *Eizo Tajima* had defined a genetically significant dose at the UNSCEAR Secretariat. The expression which Karl Morgan wrote on the blackboard largely meant that the number of children who could be anticipated from a man and a woman together was the sum of each of their expectations. In the way that the formula was written, the genetically significant dose from natural sources of radiation would then be 200 mrem per year, even though the gonadal doses were only 100 mrem per year.

The meeting in Geneva made a number of important decisions. A classification of x-ray examinations was agreed on by 22 groups, which was characterised mainly by which part of the body was being examined. This classification, with minor modifications, would later be used in UNSCEAR's reports. It was also recommended that people differentiate between five types of x-ray examination: fluorographic examinations, ordinary x-ray examinations, fluoroscopies, tomographies and cineradiographies (cineröntgen). Information on gonadal doses (male and female) should be registered for infants (under two years of age) and children between the ages of 2 and 4 years, 5 and 9 years, 10 and 14 years and 15 and 19 years, adolescents between the ages of 20 and 24 years and 25 and 29 years, as well as thereafter for each ten-year age period, i.e., 30–39 years, 40–49 years, etc.

An important decision made by the group was to advise against a total registering of all doses from all x-ray examinations for the purpose of estimating the genetically significant dose. Such a registration would lead to results which were less reliable than estimates made by experts on the basis of limited studies. The registration would also constitute an unwarranted load for the healthcare system.

The report from ICRP's and ICRU's Joint Study Group was published in the journal called *Physics in Medicine and Biology* in 1957.

UNSCEAR met for the third time from 8–18 April 1957, this time in Geneva. Professor Chagas explained that he was not willing to continue as Chair. Dr. Z. Bacq from Belgium was therefore elected as the new Chair and Dr. Watkinson as Deputy Chair. The Swedish delegation still consisted of Sievert, Caspersson, Aler and Nelson, but because radiation genetics was expected to constitute an important part of the programme, Sievert had summoned Professors of Genetic Gert Bonnier and Åke Gustafsson as extra advisers. At this session, the Committee agreed the way in which the chapters of its first report to be published in 1958 would be divided up.

On 16 May, Sievert was able to state in a letter to Walter Binks that the majority had now responded to his circular and accepted me as Temporary Secretary. He wrote that he had reckoned I would help Binks with the new recommendations, but that whilst in Geneva he had gained the impression that Eric Smith was interested in this. That would be good, thought Sievert, because the recommendations will probably be published in England. 'But I think the work will be comprehensive enough for both of the "temporary" secretaries, and I would be grateful for your opinions on how the work ought to be organised and on the best time for Lindell's visit to London. I hope you can take charge of the work because Mr. Smith said that you were now feeling better'.

Sievert proposed that the work be divided so that those who were in Stockholm took care of the correspondence and questions about the future organisation, while those in London worked with the UNSCEAR assignment and the new recommendations in connection with which Lindell and Smith could assist under Binks' leadership.

However, Sievert had totally underestimated the impending work and overestimated Binks' health and willingness to continue. Whereas in June Sievert had been able to tell Binks that everyone now supported the proposal for me to be Temporary Secretary, Binks' reply on 8 July to Sievert's proposal for the allocation of work was as follows:

> I asked to be able to step down as Secretary of the Commission, and am now of the opinion that the Commission's approval of Dr. Lindell as Temporary Secretary shows that my application to step down has been approved. As such, the work cannot be allocated between Stockholm and London as you proposed, except for with regard to the report to the UN's Scientific Committee which Mr. Smith has already undertaken. I propose that Lindell visit me as soon as possible to
>
> 1 take over all ICRP actions,
> 2. discuss the procedure he may use to start preparing the new ICRP recommendations, and
> 3. look at my experience with regard to the way in which the next ICRP meeting can be organised.
>
> Mr. Smith has offered to help with (2), but we think the responsibility for drawing up the new report ought to be that of Dr. Lindell.

Sievert responded immediately, expressing regret that Binks' health did not permit him to participate in the continued work. He agreed with the view of the Secretaryship and proposed that I come to London for 2–3 days between 15 and 25 August. He also wrote:

> With regard to the year of the next ICRP meeting, I think it would be very unwise to hold it later than early spring 1958. We must have our new recommendations published before the UN's Scientific Committee has issued its report, which will probably be in July 1958, or else I'm afraid we will lose face.

While Sievert was pushing through his proposal for me as Temporary Secretary of ICRP, on 6 May, i.e., his birthday, he had received a letter from Raymond Appleyard at UNSCEAR which began as follows:

> My dear Professor Sievert,
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> Once again I must turn to you while I am having difficulties. Tajima finishes here at the start of July and we are in desperate need of a physicist to replace him. The ideal person would be an expert in both dosimetry and fallout, but because such a person probably cannot be obtained, one or the other will have to do, but preferably the first. As you know, I was thinking of a candidate from another country but have yet to hear anything particularly good about his technical ability. Any sort of replacement in the middle of writing the report is bad enough, but I think it is particularly important to have someone who is really good at physics this time. Is there any possibility that you can provide a Swedish candidate for a period of probably one year but absolutely no less than eight months from 1 July? The salary was $ 12 000 gross, $ 9 250 net (tax-free), plus a number of contributions, such as for a married man with two children, takes it up to the gross figure. That has now increased. I can obtain and send information to the person you suggest as a candidate if he really is interested in coming. […]

Sievert immediately suggested me for the job. Arranging a one-year job for me as Scientific Secretary at UNSCEAR's Secretariat in New York was completely in line with his objective – to improve contact with New York. The fact that my moving to New York would logically conflict with his motive to allow me to replace Binks did not worry Sievert in the slightest, but did subsequently lead Lauriston Taylor to write that he was chagrined to find that the Secretary of ICRP was diverging and had no direct access to Sievert's resources in Stockholm.

So, in autumn 1957 I found that I was both Temporary Secretary of ICRP and Scientific Secretary of UNSCEAR's Secretariat. In September, I visited Binks to take over ICRP's archive and was given good advice on the Secretaryship. At the same time, Sievert wrote to the Chairmen of the ICRP Committees and emphasised the importance of their reports being finished in good time. I began to comprehend the scope of the work that was expected. Sievert also wrote a thank-you letter to Eric Smith in which he praised the latter for his extremely good work as Secretary of ICRP's and ICRU's Joint Study Group for the UNSCEAR project.

12. FROM NEW YORK'S HORIZON

And so I undertook the journey to New York on 17 September 1957 with my wife and six year-old daughter on the Swedish American Line's *Gripsholm*. We had ordered a one-way tourist class cabin for 3 368 Swedish kronor, but the UN's personnel office insisted that we travel first class (for 5 830 kronor). We did so with a certain amount of trepidation because I had been advised to take along a smoking jacket for dinners, but the misgivings were unfounded. I wrote to Sievert from the boat the day before we arrived: 'We have had fog throughout the journey until today when it cleared up. The rich widows and "sausage millionaires" did not indulge in elegant dressing and I find that I need not have brought my dinner jacket with me'. Having said that, we ascertained that we had had a more enjoyable time on the tourist class boat journeys of 1956.

The job I was starting was on the P-5 Step 3 salary scale, which meant approximately one thousand dollars a month, a staggering amount to us, especially as there were also showered with completely unexpected funds in the first few days, in the form of resettlement allowance and child benefits. We moved into the Tudor Hotel on 42nd street for the first few days before finding someplace to live, a hotel where, looking at the drawings hung showing each floor, you could gather that the dimensions of the small rooms barely exceeded the thickness of the walls. After having spent time hunting for accommodation over half of Long Island, we finally found an apartment for 175 dollars a month on the upper floor of a villa at 170 Church Street in White Plains instead, a central place around 40 km north of the UN building. The lower floor was where the building's owner lived, Miss Helen Porter, an authoritative Canadian schoolteacher who did extra work in the evenings teaching children with reading difficulties and teaching English to immigrants. From the building, it was a twenty-minute walk to White Plains railway station followed by a three-quarters-of-an-hour train journey to Grand Central Station as well as a further ten-minute walk to the UN building. We found that a completely ordinary primary school was situated slightly higher up on Church Street, so we would not need to worry about the traffic posing a risk to our daughter, Karin, when she was to start her schooling.

UNSCEAR's Secretariat was on the 34th floor of the 38-floor UN building next to East River in Manhattan. The building is so narrow that the rooms available to UNSCEAR were on both sides, with a view over either East River and Brooklyn or Manhattan. My first room looked out over East River, but I gradually made my way along to a room on the other side with two large windows overlooking a wonderful view of Manhattan. At the Secretariat there were five Scientific Secretaries with the Canadian Ray Appleyard as the boss. The young Argentinian Dan Beninson was there, whom I had shown around in Stockholm in 1956, as were the Belgian biologist *Maurice Errera* and the American pathologist *Hermann Lisco*. In the room next to mine was Professor *Vladimir Obuchov*, a man who was fifty-five years old and who spoke only Russian. My first work contact was the Canadian medical physicist *Harold Batho*, although he would be returning home immediately to the British Columbia Cancer Institute in Vancouver.

In turn, the Scientific Secretaries had secretaries and the total male domination at the scientific level was equalled by a total female dominance where the office work was concerned. Four young ladies were directed by the slightly older office manager, Mary Bohan, the same Mary Bohan who had been *Samuel Goudsmit's* (1902–1978) secretary in the ALSOS group in Germany.* To some extent, the choice of employees reflected the ongoing cold war. Raymond Appleyard had a brother who was said to hold a

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* See 'The Sword of Damocles'

high position within the British intelligence service. Dan Beninson may or may not have had intelligence assignments, but nobody had a greater capacity to 'hoover' up information.

Dan was a real polyhistor who had been educated in medicine, biology, physics and psychology, and who was highly language-orientated and interested in most things. Those who spoke to him soon found themselves telling him just about everything there was to tell.

Dan was also a master of chess and spent a great deal of time at the Manhattan Chess Club. Once the people there had discovered his skill, they dragged him around various chess premises in Manhattan. In New York, as in most of the large cities all over the world, new recruits to a chess premises, a club or a café, are easy victims of chess sharks who lure them into thinking they are more of a dab hand than they actually are. The whole thing ends with a proposal to play for money, which the new recruit usually loses. The chess shark is usually so skilled that he can count on beating all new recruits; the real masters are well-known. But Dan was not yet well-known in Manhattan and this affected one chess shark after another.

Meeting up with Dan again started a life-long friendship and a fruitful cooperation in radiation protection matters. We held the same view of fundamental policy matters, which led to provocative comments about the Beninson–Lindell mafia at the various international meetings. David Sowby has described our cooperation in the following words (Sowby, 2001):

> From the start Bo and Dan became a sort of symbiotic team, rather like the lichen* with which we were to become so familiar because of its ability to absorb *[radioactive]* fallout.

Professor *Leonid Ilyin*, who would later become part of the Soviet UNSCEAR delegation and go on to become the Soviet representative, describes the Beninson– Lindell factor as follows (Ilyin, 1995):

> Beninson is a very convivial person who is always prepared to get involved in a scientific discussion, but he cannot tolerate inadequate competence. A good diplomat who skilfully leads the expert meetings is quick to pick up the leading idea behind the discussion and in the end proposes conclusions with which it is impossible to disagree. […] Bo Lindell was noted for his very serious method of analysing the scientific information discussed during the committee meetings and for his in-depth review of proposed recommendations. Only after doing so did he formulate his own view of the matter.

I ended up sharing a Secretary with Professor Obuchov. She was a blonde lady of my own age, Nina Nekrassoff, born in Cyprus (or possibly Malta) of Russian parents. Her father was General Nekrassoff, who had once been a well-known officer in the Tsar's army. The family had been forced to flee following the revolution. Nina was then educated in Paris and spoke fluent English, French and Russian and Portuguese as well now because she was courted by a Portuguese UN official.

Batho conveyed to me what Eizo Tajima had previously done. The short period of employment, one or two years, posed continuity problems. Tajima had passed the problems to Batho and they were now making their way over to me. My first task was to use the basis of the material which Tajima and Batho had left behind to draw up the Appendix of the doses of radiation for patients from medical use, primarily x-ray examinations. The basic material I had also included the report from the ICRP-ICRU group, which I had actually helped to draw up.

I found that Tajima's, Batho's and Karl Morgan's definitions of the annual genetically significant dose did not tally. As I described in the previous chapter, you must weight the doses of radiation to the genitals for each individual, taking into account his or her likelihood of having children in the future. The weighting factor is obviously zero for an eighty year-old lady and very close to zero for an eighty year-old man. But what is it for a five year-old child? To fulfil the requirement from the definition of

* 'A complex organism in which fungi coexist with algae'. [Sowby's footnote]

genetically significant dose, the weighting factor ought to be proportional to the future number of children anticipated by the individual which, for an average young individual, is approximately two in order for the size of the population to remain constant (*approximately* two because not all children survive to reproductive age).

In order to obtain an average value of the genetically significant dose for a group of people or a whole population, the sum of all individuals' gonadal doses once they have been weighted for the expected number of children must be divided by *twice* the anticipated number of children for the whole of the population. This subsequent factor of two comes into the equation because we have assumed that each very young individual expects to have approximately two children (and older individuals a gradually smaller number). Because we had described the situation so that both the mother and the father each anticipated having two children, i.e., anticipated having this number together, we should not believe that the family would have four children. We must therefore divide the sum of both of the parents' expectation by two. Alternatively, we could have worked on the basis that each individual anticipates approximately one child and the parents therefore together anticipate approximately two, but this was not what was initially assumed. In his formula, Karl Morgan had forgotten to divide by the extra factor of two. His formula would have resulted in the annual genetically significant dose from a natural radiation dose of 100 mrem (1 mSv) being 200 mrem, which conflicts with the definition of the genetically significant dose. Tajima, however, had calculated correctly.

Calculating the genetically significant dose is arithmetically easy but difficult to embrace if you are mathematically gifted. It took me a long time to understand despite its simplicity - I am not particularly mathematically inclined.

UNSCEAR had requested information on doses of radiation to the genitals from different types of x-ray examination according to a classification which ICRP and ICRU work group had proposed. It was my task to process this information and present it in a uniform way so that the total annual genetically significant dose could be calculated. It was a frustrating job because the reports that were being received were not uniformly arranged. Sometimes information was lacking and sometimes there were obvious errors. In order to perform my calculations, the only thing I had access to was a large, unwieldy, manual calculator which made me long for an Odhner, Addo or Facit (Swedish manual calculators). For many calculations I had to use my slide rule that I had brought with me. Pocket calculators did not yet exist and computers were found only at large universities and IBM.

Another tool which I lacked to start with was a typewriter. I'd had my first typewriter when I was ten and was used to using a typewriter, but using only my index fingers. However, this did not make a difference because I did not type from a manuscript like a clerk, but thought up my own words to type. That meant that I typed just as quickly as the office girls. But the UN had no typewriters for the Scientific Secretaries. This created problems for my letters to Sievert, which I described in a letter to his secretary, *Mrs. Marianne Wirén-Blomgren*:

> […] In the future, do not be surprised if I occasionally type and answer some letters in English. It is not because I have forgotten my Swedish or want to show off my American [English], but for the simpler reason that I do not finish work until between six and seven in the evenings and. owing to a long train journey to White Plains, where we live, do not get home until around eight. We then eat dinner and there is not much time left for writing letters. The point is that I have greater opportunities to write letters in the UN building, but I cannot type there (since it would shock five female secretaries in the rooms outside). Nor can I dictate a letter in Swedish, because the secretaries' language skills are limited to English, French, Russian and Portuguese. And so the option open to me is English, hence English letters. […]

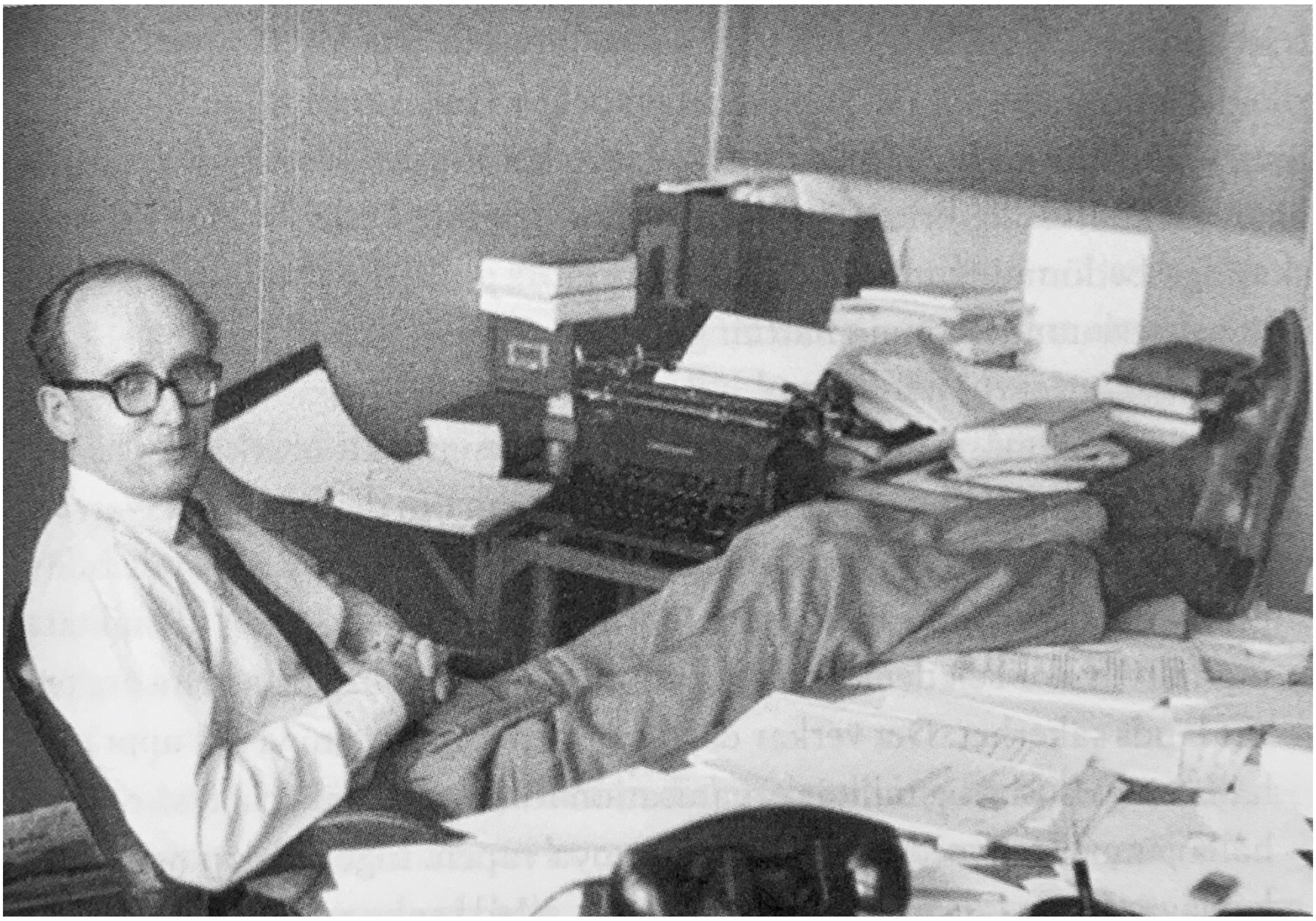
However, not being able to type was difficult for me and my labouring with pencils took its toll on my fingers. Whether or not this was the cause I do not know, but I ended up with Dupuytren's contracture, a thickening of the connective tissue in the palm of your hand which means that several fingers become bent and it becomes difficult to write. I then insisted on having my own typewriter and ended up right in the UN's bureaucracy. Office girls have typewriters - scientific secretaries do not have typewriters; such a thing has never been heard of and there is no ordering procedure to facilitate such a heresy! I stood my ground and was supported by the legendary Undersecretary and Nobel Prize winner *Ralph Bunche* (1904–1971), who said ‘We must fight bureaucracy!’ After many ifs and buts, I was finally given a typewriter (not an electric one in those times, of course). When I began to type, I felt like a monkey in a cage. Outside my open door office girls were shocked into coming from the near and far corners of the UN building to check the truth of the rumour that there was a *scientific* secretary – and a *man* at that – using a typewriter! It took Nina a while to dare to admit to outsiders that she was my secretary; it was something she was probably upset about.

My task as Secretary of ICRP meant that I had already rung Professor Failla on one of the first few days in New York to agree a time when I could come and talk to him. However, it so happened that I managed to see Failla before we met as we had agreed. Herman Lisco asked if I wanted to go with him and listen to a presentation of a statement from the Atomic Energy Commission’s Advisory Scientific Committee for Biology and Medicine about the risks from the radioactive fallout from the bomb tests. The signatories included Dr. Shields Warren, UNSCEAR’s American representative, but also Failla who, to my surprise, was Chair of the Committee. Lisco told me that the statement had stirred up considerable criticism because the Committee had not stopped at scientific assessments but had also made political statements. The following was said in the Committee’s concluding summary:

> Since it must be assumed that some harm will result from fallout radiation, the question naturally arises as to whether this is justified by the benefit, even if it be well within recommended limits. In this country a large fraction of the annual budget is for military expenditures – which in a democracy gives a measure of the citizens’ concern about the safety of their country. It seems obvious, therefore, that if we wish to maintain a first class military organization for the safety of the country, we must at least keep abreast of new weapons developments. No such developments can be carried out successfully without tests. […] Therefore, in terms of national security, necessary tests of nuclear weapons are justified. […]
>
> The question arises in the minds of many thoughtful persons whether the number and power of bombs exploded in the tests are being kept at the minimum consistent with scientific and military requirements. In view of the adverse repercussions caused by the testing of nuclear weapons, the Committee recommends that tests be held to a minimum consistent with scientific and military requirements and that appropriate steps be taken to correct the present status of confusion on the part of the public.

This statement from the AEC’s Advisory Committee aroused a great deal of fuss and was also discussed within the UNSCEAR Secretariat. The atmosphere there is indicated in a letter that I wrote to Sievert at the end of October 1957:

> […] I have just read the U.S. AEC’s Advisory Committee’s statement on fallout, signed by Shields Warren and Failla, among others. First of all, their dose estimates deviate from that of the UN Secretariat in that they fail to quote measurements which have given high values. Secondly, they conclude that the injuries from the fallout (which they have calculated to include 200 deaths from leukaemia per year in the USA) are worthwhile accepting with reference to ‘the security of the nation’, which is a daring conclusion by a Committee which is thought should provide purely scientific assessments. The statement has been strongly criticised in the press here. If this is to be the American delegation’s view when the UN’s Committee [i.e., UNSCEAR] meets, there will be some hard discussions. […]



Bo Lindell as Scientific Secretary in his work room at UNSCEAR's Secretariat in the UN building in Manhattan in 1958. Photo: Nina Nekrassoff.

Later, I was able to read the minutes from a hearing which had been arranged in June before a Committee on Radiation, set up by the Joint Committee on Atomic Energy.* It contained the following exchanges with Shields Warren:

> Dr. WARREN. I would like to leave very briefly the discussion of research and make one further comment. That is this: The ultimate decisions with regard to weapons testing and with regard to the whole development of atomic energy will have to be made, as they have been made in the past, by you and other duly constituted representatives of our people. I believe that the advances in science within the next few years provided research is adequately supported and facilitated will permit obtaining much more conclusive data than now exist as to the feasibility of continued weapons testing. The concern of the world is for disarmament and the elimination of war, of course. I firmly believe as a physician that it is inexcusable for us to jeopardise our own safety and that of the rest of the free world in order to eliminate a risk of as low an order of magnitude as is constituted by any reasonable programme of atomic weapons testing.

* See my reference USCo 1957, pp. 1416–1419.

Senator ANDERSON.[*] Do you think that the proposal made by Dr. Langham[†], which was an overall control of the total tonnage of any fission products going into the atmosphere would jeopardise our production of weapons?

Dr. WARREN. I am not at all an expert in this field, Mr. Anderson. I would not have any opinion. I would hope that it might be feasible to work out some programme of this type.

Senator ANDERSON. Would you read again the last paragraph?

I firmly believe as a physician that it is inexcusable for us to jeopardise our own safety and that of the rest of the free world in order to eliminate a risk of as low an order of magnitude as is constituted by any reasonable programme of atomic weapon testing.

You there set yourself up like an expert in the field. I am not trying to say it is improper. You testified what you would do.

Dr. WARREN. Yes.

Senator ANDERSON. Having established that, do you think Dr. Langham's proposal that an amount of 10 megatonnes of fission production going into the atmosphere each year which we are now doing *[a misunderstanding which is commented on below]* is about the safe limit?

Dr. WARREN. I feel we ought to be safe in having that much. I would hesitate to say that is an absolute upper limit. I would think that is a reasonable amount. I would not be worried by a programme at that level.

Senator ANDERSON. If you have not made studies in the field yourself, you recognise that the Los Alamos and the Livermore Laboratories have.

Dr. WARREN. Yes. They are most competent.

Senator ANDERSON. If they feel that is a top limit, does that suggest to you that is something we might look to as a proper guide or not?

Dr. WARREN. I would think that this might be very sound indeed. From my own knowledge from the medical standpoint, as I said, I would not be at all worried by a programme at this level.

Senator ANDERSON. Almost every time when somebody comments, they talk about limitation on testing as if it meant the elimination of all progress and all testing of every kind. It is like saying to man he should be careful in the amount of protein he takes into his system. But a doctor will say if you do not take any protein at all, many things will happen to you. Somehow we do not get much comment on the suggestion of limitation. It is always said that we will abolish it all. This was not the proposal of Los Alamos and certainly was not my own.

Senator JACKSON.[‡] Dr. Warren, what this really boils down to is that we have two risks. One is the risk to the free world if we are not prepared to deal with an enemy that might well bring total atomic hydrogen catastrophe to all free nations. On the other hand, continued testing do present a danger of an undetermined nature to people. We do not have enough scientific data for scientists to speak scientifically, whether

[*] Senator Clinton P. Anderson (New Mexico) was Deputy Chair of the Congress' Joint Committee on Atomic Energy.

[†] Dr. Wright Langham was the head of the biomedical research at Los Alamos and a well-known expert on the toxicology of plutonium.

[‡] Senator Henry M. Jackson (Washington State) was a member of the Congress' Joint Committee on Atomic Energy.

they are doctors or pure scientists. There are these two threats. Maybe between the two some kind of reasonable balance can be achieved. Don't you think that is a reasonable approach?

Dr. WARREN. Yes. I think that is a very reasonable approach. That is what I had in mind when I spoke of any reasonable programme of atomic weapons testing.

The statement from Wright Langham and Shields Warren agreeing that nuclear explosions corresponding to 10 million tonnes of TNT each year would not create a worrying problem led Dan Beninson and me to start reconciliation calculations. What would actually happen if the bomb tests were to continue at an undiminished rate? It was a question that I subsequently tried to answer by introducing the concept of 'dose commitment', which will be explained later on in this Chapter.

Senator Anderson's use of the word 'tonnage' gave the impression that the politicians actually thought that there were tonnes of radioactive substances which were spread in the atmosphere. However, with the RAND report's conversion factor, explosions corresponding to 10 megatonnes of TNT would produce only 10 kg of strontium-90 and considerably lesser quantities of the shorter-lived substances. The total mass of fission products was therefore very small. On the other hand, these fission products represented substantial radioactivity.

Dan Beninson had an enormous appetite for the subjects under discussion. As soon as he found out that I was Secretary of ICRP, he began bombarding me with Spanish inquisition-style questions. On which basis had ICRP chosen its dose limit? If it was not obvious that there were threshold values for dangerous doses of radiation, the choice had to reflect a view of which risk could be acceptable. How had they arrived at such a risk?

I was between a rock and a hard place. Everything that Dan said was right. ICRP ought to have discussed these things but had not done so. The dose limit was a remnant from the time when people really thought about the threshold values. Experience clearly indicated that you could be exposed to doses of radiation up to the dose limit every year for a lifetime without having any injuries caused by the loss of cells. But what was the case with leukaemia? And hereditary injuries? The only thing that was known was that the risk was very small - but was it small enough?

I said there was a small risk. No-one knows whether it was small enough. But even if the dose limits were random, like drawing tickets out of a hat, it would be very advantageous to have the same dose limit throughout the whole world through ICRP. Imagine the confusion if all countries were to use different dose limits!

Dan smiled contentedly. He knew he had shaken me, and shaking people mentally was one of the things that satisfied him the most. Just you wait, I threatened. ICRP may decide to elect you. Then you'll be the person who has to defend the dose limits!

The work at the UNSCEAR Secretariat was hard, partly because we were driven by curiosity and partly because Ray Appleyard set substantial requirements. All Scientific Secretaries met regularly in the conference room on the 34th floor and gave accounts of the work progress. This guaranteed uniformity of the whole report and maximum use of new ideas. All work was aimed at a big meeting of the Committee in February 1958 to draw up a report for the UN's General Assembly in June. Drafts of different parts of the report were sent out in advance to the Member States' delegations.

There was a tangible difference in the scientific training between the biologists Errera (who had written a biological book with Arne Forssberg) and Lisco on the one hand, and Beninson and me on the other. The two biologists were fact collectors and observers but did not appear to be interested in models and explanations. They also showed an instinctive contempt for mathematics; Lisco reared when faced with the very simplest formula. Beninson and I sought explanations and enjoyed creating mathematical models at which the two biologists snorted. Their view seems to have been that no biological event can be described using mathematics, while we believed that the mathematical models could give valuable ideas. And when I say mathematical model I do not mean advanced mathematics – you need not use a sledgehammer to crack a nut.

The previous British delegate had been Professor Mayneord, but we now heard that he was to be replaced by a doctor who was unknown to us, E. E. Pochin, a name which we were initially unsure how to pronounce (it was pronounced 'paw chin'). Dr. Pochin returned our draft with plenty of corrections and comments. Who is this Dr. Pochin, we wondered with slightly resentment, who is forcing so many points of view on us? We found out soon enough.

Because our work was scientific rather than bureaucratic, our workbench was overloaded with specialist literature, reports, reference books, calculations, diagrams and manuscripts, a glorious mish-mash. This mish-mash was the thing that happened to increase our prestige in the UN building. The General Secretary *Dag Hammarskjöld* (1905–1961) had said that one day he would go on a tour and visit all parts of the UN Secretariat. Everyone had tidied up their workbenches for that day except for the UNSCEAR Secretariat which, for some reason, had never received the message about Hammarskjöld's visit so the latter therefore came as a total surprise. Ray Appleyard was deeply embarrassed about the awkward situation where everything was in a complete muddle. But Hammarskjöld commended us. 'It's the first time,' he said, 'that I've come to a department where work is actually ongoing!'

When the expressions for calculating the genetically significant dose had been derived and the different national reports had been processed in this regard, we began to discuss the way in which the doses of radiation to other organs should be calculated. Bearing in mind that, according to the Court-Brown and Doll reports,* leukaemia was thought to be the dominant cancer risk, the obvious thing to do was to calculate the dose to the blood-forming bone marrow. It was not an easy task because the active bone marrow is distributed over different parts of the skeleton and an evaluation of each type of x-ray examination was needed to be able to calculate the doses of radiation in these parts. Then there was the question of whether there was any point calculating a mean value, a mean marrow dose, for the whole of the active bone marrow. And finally, was there any point going on to take a mean value of this mean value throughout the population, a *per caput* mean marrow dose?

The term *per caput* is worth a special comment. Most people write *per capita* (i.e., 'for each individual of a group of people') rather than 'for each individual'. And so did I until a WHO editor corrected me and gave what I considered to be a credible explanation. 'Per capita' actually means 'according to the number of individuals' and was used by the Romans to calculate income tax – the greater the number of individuals, the greater the income tax. The term had become so embedded that when you were looking for an expression which usually referred to 'per individual', you wrongly used the same expression, *per capita*, rather than the correct *per caput*, i.e., 'per head'.

At the time of the discussions regarding the bone marrow dose, I began to study the illness statistics for different countries to see whether the total leukaemia risk had changed as time passed by. I found the following information in WHO's statistics for 1952:

Country	No. of cases per million	Annual increase (%)
Sweden	62	4.1
USA	62	4.1
England	46	5.0
France	45	9.5 (!)
Italy	37	5.0

The prevalence of leukaemia in France increased twice as quickly as in the other countries. At the same time, I found that lung x rays of young Frenchmen were obligatory and that 19 million youths per year were exposed to x rays through fluoroscopy examinations, i.e., where the doctor stands looking at a fluorescent screen, sometimes possibly for quite some time. The dose to the bone marrow could be expected to be 2–5 times the natural dose, so if there were no threshold value for the radiation dose that could cause leukaemia, the prevalence of leukaemia in France ought to have increased in latter years,

* See Chapter 14.

just as the statistics showed. But it was not possible to scientifically substantiate that it was the lung x rays which specifically increased the risk of leukaemia because in epidemiological studies, you have to make comparisons with a normal population that has not been exposed to extra radiation. How would you find a normal population which was the same as the French population in all respects apart from irradiation?

Rolf Sievert visits Dag Hammarskjöld at UNSCEAR's 5th session in New York in June 1958. Bo Lindell, who was at the Secretariat then, looks on. Photo: The United Nations.

The observation still meant that we felt vindicated in our plans to calculate bone marrow doses.

We had some support from an editorial in the *Science* journal (17 May 1957), where the conclusion that had been drawn in an article by *E. B. Lewis*, Professor of Biology at the California Institute of Technology, in same issue had been accepted. Lewis thought that available data indicated that the dose-response relationship* for leukaemia was linear with no threshold value for the radiation dose that can cause leukaemia. A linear dose–response relationship had long been assumed where hereditary injuries were concerned, but this was where the first 'LNT assumption' (linear non-threshold) discussions also began for somatic injuries such as leukaemia.

Some scientific papers aroused particular interest while our work was ongoing, although not until 1958. Failla wrote about ageing and cancer in the New York Academy of Sciences' documents. *R. Seltzer* and *P. E. Sartwell* wrote in the *Journal of the American Medical Association* about the connection

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* A dose–response relationship states the relationship between a dose and the likelihood of injury as a consequence of the dose. This is different from the dose–effect relationship which states the relationship between a dose and the level of injury.

between irradiation and the life expectancy of doctors, mainly because Shields Warren had maintained two years earlier in the same journal that radiologists had a shorter life expectancy than other doctors. It was not possible to confirm Warren's result; however, it looked as though radiologists did run a greater risk of leukaemia. However, the radiologists for whom information was available had worked at a time when radiation protection was not as good and the doses of radiation were probably really high. *Alice Stewart* (1906–2002) wrote once again about cancer in children and the dose to foetuses when x-raying pregnant ladies, the first indication of the risks of cancer from low doses of radiation.

Autumn 1957 was more eventful than we at the UNSCEAR Secretariat were fully aware of. On 29 September, a tank containing highly active reprocessing waste exploded in Kyshtym close to the city of Chelyabinsk in the Urals. This accident caused the catastrophic radioactive contamination of large areas of land, but this was not disclosed by the Russians until 1990 under Mikhail Gorbachev's Glasnost.*

On 4 October, the Soviet Union launched the world's first artificial satellite, Sputnik 1, into space. On 3 November it was followed by Sputnik 2 with the 'space dog' Laika who died in the satellite before the latter burned up upon re-entering the atmosphere. The Soviet Union's successes did nothing to reduce the intensity of the 'cold war'.

On 7–10 October, the Windscale accident occurred when one of the British plutonium-producing, graphite-moderated reactors caught fire and spread volatile radioactive substances into the surroundings. The presence of radioactive iodine in the air close to the ground was evident throughout Europe. However, no information about this (which you could read about in the daily press) reached the UNSCEAR Secretariat either. The superpowers regarded one another mistrustfully and the accepted procedure was strictly adhered to: the only things the Secretariat could write about was information that had either been published in scientific journals or which had been given to the Committee through the official channel of the Member States' delegations. There was therefore no official information about Windscale in the report, which was completed in 1958. The closest you got was a paragraph (38) on page 11 of the report (UNSCEAR, 1958):

> Radioactive contamination of man's environment occurs as a result of nuclear explosions and may also arise from radioactive waste disposal and accidents involving dispersion of radioactivity. At the present time the radiation doses from these last two sources are negligible, but in the future they might become appreciable.

Things also happened with regard to the 'cobalt gun' that Rune Walstam and I had designed and which was marketed by Elema-Schönander. One of these had been sold to *Dr. Erich Uhlmann* at the tumour clinic at the Michael Reese Hospital in Chicago. It caused problems with the AEC's bureaucracy. The requirements set by the Commission for a 'cobalt gun' were drawn up using kilocurie devices as standard and quite simply could not be fulfilled by a small head and neck unit. We pointed out that the device gave considerably better protection than the 'radium guns' for which no requirements were set because they fell outside the AEC's area of responsibility. When that bureaucracy was cleared up, up popped the next problem. Our cobalt gun contained uranium as radiation shielding material because it is more effective than lead. That meant that Elema-Schönander exported uranium – a very suspect material from the Americans' point of view – to the USA!

Another remote partner for our cobalt gun was *Dr. Donald Paterson* at the Christian Medical College & Hospital in Vellore in India. Usage problems arose there as a consequence of the high air humidity.

Parallel to the work on UNSCEAR's first report I had my job as Secretary of ICRP. For Binks, who was a member of ICRP's Main Commission himself, the position of Secretary had been an honorary, unpaid task. For me, as nothing higher than Secretary, it was purely a task but was still unpaid. The idea that the assignment could be given as an honour was alien to both me and Sievert. Hardships aside, I simply saw it as a privilege. The work had to be dealt with mainly in the evenings at 170 Church Street, White Plains, which was the ICRP Secretariat's address for a year. My only tool was my travelling typewriter which I had brought with me from Sweden.

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* See Chapter 13.

Lauriston Taylor attended my first meeting with Failla after arriving in New York – he had travelled up from Washington D.C. We made a list of the problems. Problem number one was that there was no clear picture of what had really been decided in Geneva in spring 1956, despite Taylor having sent a report to the Congress of Radiology in Mexico City later that same year. Problem number two was how to do the work in order to produce new ICRP recommendations to replace those that had been published in 1955 in Supplement No. 6 of the *British Journal of Radiology*. Problem number 3 was how such a report would now be published.

ICRP's 1955 publication had consisted of an introduction followed by reports from the four Committees that existed at the time. The introduction was of little substance apart from a list of definitions. The most essential sections were the reports from Failla's Committee I on protection against external radiation and Karl Morgan's Committee II on protection against internal radiation (at that time and until 1962, the Committees were designated by Roman numerals). It was initially thought that the new report would have a similar structure.

It was now 1957, but because there was no authorised report containing the decisions from 1956, other international organisations had no reference material apart from the 1955 recommendations, which were based mainly on decisions made at the Congress of Radiology in Copenhagen in 1953. The world was still unaware of the fairly comprehensive policy changes that had actually been determined by ICRP in 1956. This led me to write the following to Sievert on 16 October:

> […] the second important point is the idea that suddenly struck me and which I have discussed with Failla today. I have observed that the UN Committee [UNSCEAR] cannot refer to any information on the Maximum Permissible Dose other than that which is given in the old recommendations (Supplement 6 of the British Journal). Nothing else has been published. ICRP's report to the Mexico Congress certainly does mention the new dose limitation of 50 rem up until the age of 30, but only as an abstract from Committee I's report and with no note stating that the Commission has accepted the concept. Also, it does not exactly seem possible to publish the completely new edition of the recommendations before the UN Committee meets in January–February 1958.
>
> So, at the time of the UN Committee's meeting, no information from ICRP to which reference can be made regarding the MPD* will have been published except for that which is stated in Supplement 6 of the British Journal. This is extremely unfortunate and will do little for the Commission's standing, at such a critical stage as well.
>
> As said, I had a spontaneous idea as to how to solve this problem. The new recommendations cannot and should not be forced through more quickly than we already have already planned. Publishing some of them in advance, i.e., the chapter on basic philosophy (Failla is very positive towards the idea of introducing such a chapter separately before the different Committees' special recommendations) may be difficult or will at any rate take time because this chapter requires careful preparation and reflection on the part of the author and will also no doubt give rise to discussions and proposed changes. So my proposal remains: ICRP sends all significant radiological journals a letter, its wording as short as possible, saying:
>
> 1) The MPD values are those that are mainly quoted at the moment.
> 2) As a rule, the source is ICRP's recommendations from 1953 as published in Supplement 6 of the British Journal.
> 3) This source is no longer relevant because the Commission made essential changes to the fundamental principles at an ordinary general meeting in Geneva in 1956.
> 4) The old text is quoted.
> 5) The changes agreed in 1956 are stated as regards the most important points.
> 6) It is emphasised that the Commission is currently drawing up new recommendations but that the work with these has taken considerably longer than estimated, and that they cannot be expected to be published until spring 1958.

* Maximum Permissible Dose.

> 7) In the meantime, the Commission would therefore like to bring to your attention the changes that were made in 1956.
>
> I take it you see the significance and the consequences of this action? By sending the UN Committee this letter at the same time as sending it to the journals, we can work around the official procedure of contacting the UN Committee: what it (in whichever way) will receive is a general, published document which will be printed in the scientific press and which can therefore be freely referred to. The real publication will therefore take place on the day when the letter is issued by ICRP, saving us the time which we would otherwise have lost waiting for it to be printed.
>
> ICRP will avoid criticism in terms of no longer being up to date or silence as regards an important matter.
>
> […]
>
> Failla is completely and enthusiastically in favour of this solution. It gives us breathing space and the possibility of tackling the 1958 recommendations on an impartial basis. I have promised Failla that I will draft a letter. I will show it to him as soon as he has time to look, which will be on 26 October. We will work on the letter jointly and send you the result. If you agree and accept this solution, you can send the Commission a circular straight away and have an answer in mid-November, whereupon the best case scenario will be that the result can be published before 1 December.
>
> I am eager to hear your points of view: thumbs up or down?
>
> Your affectionate friend, B. L.

The fact that there was a certain amount of conflict between the British and American members is illustrated by what I wrote in my next letter to Sievert on 18 October:

> […] While I still had Taylor on the line, I referred to my conversation with Failla as well as the fact that I consulted you about my proposal for an 'intermediate publication' of the 1956 recommendations. Taylor said that this was what he had intended to suggest because he thought it was absolutely necessary that something be done immediately, and that you and he, who had been members of ICRP/U from the very start, were particularly concerned about the situation and that it was a blow that the Geneva meeting's most essential results were not published immediately. He said that this was partly due to the British group.
>
> If that is the case, do you think Mayneord and Binks now have something against publishing the 1956 decisions as unanimous ones from the Commission? Bearing in mind that the 1958 recommendations are up for discussion with regard to the principal matters, I do not think they will have any objection; at least that is the impression I got while talking to Binks and Smith.
>
> And in any case, if nothing comes out before the UN Committee's meeting, ICRP is no longer any more than an historical curiosity. Please write and give me your opinion as soon as possible. If I have not heard from you, I might end up spoiling your plans by acting at my own discretion simply because I do not know all the underlying matters which might also change my own opinion. I am currently in a position where I must take initiative and make rapid decisions as to whether my presence here is worthwhile.
>
> […]

On 21 October, Sievert's response was that he fully agreed with my proposal but that the statement must be carefully formulated so as not to anticipate the discussions that could be expected within the Commission with regard to the continued policy. On 1 November, I sent him a proposed statement consisting of a text that I had written and which had then been modified and abbreviated by Failla. This was published as planned, with minor changes (in *Acta Radiologica*, among others, as early as 1957). A couple of important policy changes from 1956 took into account the risk of hereditary injuries to the whole population as well as the possibility of only *statistically*-demonstrable injuries (the primary reference was made to the shortening of someone's life expectancy). The respective texts read as follows:

For the entire population

> The use of nuclear reactors for power production involves waste disposal and dispersion of radioactive material that may affect large sections of the population. The rapidly expanding use of radioactive materials in science and industry subjects more and more people to exposure to radiation. Therefore, genetic damage assumes greater importance. Designers of nuclear power plants and others concerned with the peaceful application of atomic energy, cannot plan for the future in the present state of uncertainty as to what the genetic problem may mean in terms of a permissible level for the whole population. Realizing the importance and urgency of the matter, and cognizant of its responsibility to the public, the Commission has decided to accelerate its study of the problem in order to be able to recommend in the near future a maximum permissible 'genetic dose' applicable to the whole population. […]

and:

Statistically detectable effects

> The definitions of permissible dose and permissible weekly dose are based on possible bodily injury manifestable in the lifetime of an individual *[this sounds strange; at what other time could an individual be injured?]*. Since any such injury to a particular person that might result from exposure at present permissible limits would be very slight, and in view of large biological variations that always exist, certain types of injury cannot be detected in a single individual. This is particularly true in the case of a possible shortening of the life span. Therefore, it becomes necessary to consider injuries that become significant only when large groups are examined statistically.

With these statements, ICRP extended its area of interest to cover not only deterministic radiation injuries to those who worked with sources of radiation but also injuries to the whole population which were hereditary and less remarkable.

On 9 November 1957, a concert and dance were arranged in the UN building. I was surprised to find that Marrit was invited onto the floor by none other than Sigvard Eklund who, with ill-disguised pride, recounted that he had been given the honourable assignment of Secretary General of the second Atoms for Peace Conference which was to be held in Geneva on 1–12 September 1958. This was Eklund's definitive step into international atomic energy politics. He would go on (in 1961) to succeed Sterling Cole as Director General of the International Atomic Energy Agency (IAEA).

At home in Stockholm, Sievert followed the work on UNSCEAR's first major report with great interest. His interest was primarily in what was referred to as 'Chapter H' during the work stage, but which in the final report was called 'Chapter VII': Summary and Conclusions. I constantly kept him informed of how the work was progressing. Sievert was very critical, which is shown by one of my letters to him in October 1957:

> […] Secondly, Appleyard showed me the letter today, the one you wrote to him with regard to Chapter H. He says he is surprised that you are incensed by the Secretariat's version of this Chapter. I declare myself impartial in this matter; I know nothing of the chronology behind the creation of each version, but I do know that Errera and Beninson here have gone to great trouble as regards the formulation of the Secretariat's script. I have, without being asked by Appleyard, who has not discussed this with me, read both versions.[*] As a reader who does not know the background but who does know about the information that the Committee has at its disposal, I must say that I much prefer the Secretariat's letter, which is a logical follow-on to the previous chapter. The Geneva

[*] The second version had been written by 'group H', a work group consisting of UNSCEAR members who met in Geneva for that purpose.

> group's letter is a well-worded description of the contents of the previous chapter, but actually contains no conclusion or interpretation of this content.
>
> I assume this is intentional, and I am guessing that from your point of view it is preferable to have a 'feeble' statement that has been unanimously approved rather than an essentially correct chapter followed by various reservations and remonstrating footnotes. I am also guessing that you consider a vague final chapter to be justified, bearing in mind the inadequacies of our current knowledge. I would be more inclined to agree with you if the chapter that you are criticising made statements in along the lines of radiation not being dangerous, but as things stand now, concrete examples of unfortunate consequences are being illustrated which, with his current knowledge, mankind cannot prove to be unbelievable.
>
> A vague interpretation (in the report) of the presented material leaves the field free for different theories to exactly the same extent as they are currently carrying on about in the UN's political discussions on disarmament and the danger of the atomic bomb. […]

But Sievert did not agree with me. In his response letter, he wrote:

> […] With regard to Chapter H, to my mind the last version, worked out by the Secretariat, is definitely reprehensible. In this chapter there is no reason to go into the genetic issues and provide numbers which the Swedish genetic specialists, Bonnier and Lüning, do not even want in the genetic section. It is scarcely going to inspire confidence if the Committee provides information, which must still be seen as well and truly half-baked, to be used to draw conclusions. I have been in contact with Pochin and indirectly heard from Bacq* that they are both just as displeased as I am. The summary which group H wrote in Geneva definitely needs to be reworked but, if you read it carefully, you will find that it leaves room for numerous doubts as to whether injuries due to the atomic bomb tests are already possible now. I cannot find a more positive statement in this respect in the Secretariat's version, and I also find it extremely scant, not to mention lacking in content. I therefore have great difficulty believing that you really did compare the two versions. Is there by some chance an additional version that I have not seen? I do not exactly think our statement from Geneva was feeble, but I would prefer it if it could be strengthened in several respects, and I think that the fallout examinations of the past few months lend support to a considerably stronger statement, particularly with regard to the risks of mixed fission products. I would be grateful if you would emphasise the opinion, which is extremely likely to be correct, that accumulation in the skeleton over a short period leads to a much greater local concentration than accumulation over a number of years. Here, it is certainly a matter of concentration differences to the power of 10. During my discussions with Mayneord, I also got the idea that he is by no means looking at the matter of Strontium-90 all that seriously. My instinct is definitely telling me that there have been major exaggerations here.

In my view, the conclusions ought to establish the following points of view in particular:

1) It is known that some radioactive isotopes are concentrated in some lower plants and animals and in some organs when it comes to higher animals.
2) It is known that the uptake of radioactive isotopes in the body through inhalation and with water and food is very rapid (within the space of hours to days).
3) t is known that during the period immediately following atomic bomb explosions, mixed fission products are distributed extremely unevenly over the large areas bordering the test areas.
4) It is known that small quantities of radiation can also cause biological consequences.
5) Due to the delayed and cumulative nature of the consequences of radiation and because the symptoms that they cause are no different from those that

* The Belgian UNSCEAR representative, Professor Z. M. Bacq, was now Chair of the Committee.

accompany other diseases, it is and will be extremely difficult to determine whether or not radiation injuries are increasing in frequency.

All of these circumstances call for the greatest of caution, even if there is currently no possibility of proving whether or not the atomic bomb tests presently lead to harmful effects on humans. […]

My reaction to this was a short section in a longer letter of 30 October concerning other matters:

> […] What is more, it was snowing here last Sunday yet the heat of summer has returned today. I have considerably more to do for the UN Committee than I was anticipating so there will not be much time for sleep. When I have had time to think about what you wrote about Chapter H, I will get back to you and dispute it or agree with you; I still do not know which it will be. Just one thing: what you wrote about unreliable genetic numbers is not a criticism of Chapter H but of the source, i.e., Chapter G, which I have not got around to reading.
>
> So, I have finished my speculations by most undiplomatically putting you in a bad mood. I will begin my next letter with my stubborn views and end with an amusing story. […]

In 1955, a draft of statutes for an International Atomic Energy Agency (IAEA) had been formally discussed by a number of countries and in August, the Soviet Union agreed to participate in the discussions. The UN's General Assembly appealed for the draft to be dealt with rapidly and a Twelve-Power Working Group met in Washington in February 1956. On 27 April, the group was able to present an agreed draft. In October 1956 it hosted a conference in the UN building in New York with 81 nations taking part, which signed IAEA's statutes on 27 October. The decision needed 18 ratifications, a condition which was fulfilled on 29 July 1957, so that was when IAEA was formed. A preparatory committee led by the Swiss *Paul Jolles* began the work to find premises and personnel.

The decision was to allow Vienna to become the headquarters of the new organisation. At the time, Vienna was a badly-wounded city that was still suffering after the war. *David Fischer*, one of IAEA's later higher officials, writes (Fischer, 1997):

> … a city that still bore the scars of war and of its ten year occupation by the four Allied powers (France, the USSR, the United Kingdom and the USA). It was said that in 1945 Hitler had ordered a last stand in Vienna against the advancing Red Army. Many buildings along the Danube Canal, the last barrier before the heart of the city, were in ruins. Allied air raids had brought down the roofs of St. Stephan's Cathedral and of the Opera, but one of the first acts of the Austrian Government after the war was to restore both buildings to their pre-war splendour. Elsewhere, vacant lots showed where heavily damaged buildings had been demolished. Rubble still blocked parts of the city's main street (Kärntnerstrasse). Unlike New York and Geneva, untouched by the war, where all municipal facilities were fully functional, Vienna was just emerging from its tribulations. Except for its extensive but slow and noisy tram car network, communications were poor. Most buildings were badly heated and dimly lit. Many Viennese were still poor and shabby, motorcars were few and far between, electric goods and other 'luxuries' even scarcer. Austria, and particularly its eastern parts, had been isolated by war and occupation, few Viennese had travelled abroad for business or pleasure since 1939 and there was a sense of intellectual isolation. There was also some resentment against the new colony of rich foreigners, enjoying their duty-free commissary and extensive diplomatic privileges, relatively few of whom could speak German; a colony that was seen by some Viennese as a successor to the Allied occupation.

Of the possibilities that were offered for premises for the new organisation, Paul Jolles decided on the old Grand Hotel, which had stood empty since being used by the Red Army during the occupation. The Grand Hotel was a large building, traditional in style, which lay in an advantageous central location by Kärntner Ring, the broad ring road which runs around Vienna's city centre.

Some of the members of IAEA's Board of Governors were people who had taken part in the preliminary negotiations regarding the formation of the organisation. The most influential was the Frenchman Bertrand Goldschmidt, who remained on the Board until 1970. Other Board members who made their mark early on were *Vasilij Emelianov* of the Soviet Union and the well-known Indian physicist *Homi Bhabha* (1909–1966).

On 1–23 October 1957, the IAEA General Conference was held in Hofburg (as it was many years later), and the election of a Director General was on the agenda. Despite Soviet opposition, the American Senator Sterling Cole was voted in, who was also Chair of the Congress' Joint Committee on Atomic Energy. Cole visited the UNSCEAR Secretariat on his way to Vienna to start his job, and Appleyard summoned me to his office to greet the first head of IAEA. My impression of Cole was that he was almost a caricature of an American politician - jovial, enthusiastic about his tasks and, as far as I could see, slightly naive. David Fischer describes Cole in the following quote (Fischer, 1997):

> Ralph Bunche, the well-known and highly regarded Under Secretary General of the United Nations, who represented the UN at IAEA on a number of occasions, remarked that the Cold War raged more violently in the IAEA Board than in the UN itself.
>
> One reason was the US decision to impose an American Director General on IAEA despite Soviet objections, and Soviet concern that IAEA would be run as an instrument of US policy. Cole's own idiosyncrasies did not make his task any easier. Given the authority he had possessed as Chairman of the Joint Committee of the US Congress on Atomic Energy, it was perhaps natural that he should regard himself as a leader rather than a servant of the IAEA Member States. He had little direct experience in administration or diplomacy, he was impatient of protocol and diplomatic conventions, a trait that did not always endear him to the ambassadors with whom he had to deal, and he sometimes had difficulty in selecting the right issues on which to make a stand. He was not popular with economy-minded Western European delegations, who were annoyed by US insistence that he should receive a salary and perquisites second only to those of the Secretary General of the United Nations and were alarmed by his penchant for launching, or trying to launch, what they regarded as costly projects that had little to do with the mandate of IAEA. The heads or representatives of European nuclear energy agencies also held against him his ignorance of nuclear science.

However, Fischer also found characteristics to commend. In no respect was Cole a tool for Washington. He criticised American measures to act outside IAEA and, in doing so, to weaken the position of IAEA. He invited Oppenheimer to visit IAEA in spite of the fact that Oppenheimer was viewed as a security risk in his home country. And he cemented the status of IAEA. However, his own status was initially compromised by Paul Jolles becoming Deputy Director General and being responsible for the administration. Because Jolles had a better idea of how the organisation worked, he was in a stronger position whenever the two happened to disagree.

The first headquarters of IAEA in the old Grand Hotel on Kärntner Ring in Vienna. Photo: IAEA.

IAEA joined WHO to become ICRP's most important international cooperation partner within the radiation protection field. There were others, however. In summer 1957, *Abbas Ammar*, Assistant Director General of the ILO, had invited ICRP to send an observer to a meeting of experts to be held by the ILO in Geneva on 25 November – 11 December 1957 to revise its Model Code of Safety Regulations, as well as to prepare three codes of practice for radiation protection. They would also generally discuss the ILO's work within the radiation protection field. Sievert asked Professor Bugnard to represent ICRP. This was the start of a meaningful cooperation between the ILO and ICRP.

Sievert worked uninterrupted with his organisation proposals for the international radiation protection organisation, or academy as he thought was needed, as a parent organisation for ICRP and ICRU. However, in his last draft following discussions with Mayneord he had become slightly more restrained, which led me, who had been intoxicated by his previous plans, to encourage him a little recklessly in a letter of 22 November 1957:

> With regard to [the organisation's proposed permanent Secretariat], you and Mayneord have finally found a reasonable solution. However, I do wonder whether you

might have been thinking on too small a scale. With the current position, I think it is at last possible to force through something larger than just a library. I'm thinking of something more like a complete research institute that accommodates radiophysics, radiobiology, genetics, radiochemistry, population statistics and law. If lines start to become clear where organisation is concerned, ICRP should be able to obtain private funds (the Ford Foundation) and the UN Committee from places such as the World Bank. Don't worry, these big financiers have heeded and realised the possibility of the General Meeting's decision. Remember the need for an international fallout monitoring service. It may also be necessary to link a research institute to the permanent Secretariat in order to attract your competent people and to facilitate checks and a certain amount of relevant target-orientated research in addition to that which may be proposed for scientific institutions around the world. […]

How such an initiative will be brought to life is something that I am having more difficulty seeing. The matter is a sensitive one and requires early co-planning by ICRP/U and the UN Committee. Other bodies, particularly IAEA, will no doubt oppose it.

I said something about Appleyard in the same letter:

To touch on Appleyard once again, I do wonder whether his manner leads people to misjudge him. I have now seen him deal with several cases where he has had a specific view from the start (and makes no secret of it!), but has then abandoned it because he realised that the other party's argument was justified. I have seen few people who are as willing to recognise that they were wrong from the start – that is no ordinary characteristic. So, he has a good sense of fair play but is irritated when he suspects that someone is going behind his back, which I have seen happen in recent weeks when various second-rate politicians have pussyfooted around here in connection with the Czech proposal.*

Sievert responded to my letter of 26 November with:

It is funny you should mention the matter of a research institute. I have been involved in this matter on numerous occasions but have yet to think the time is right since there may be some risk of some people and organisations perceiving such a proposal as slightly irritating.

He added with some resignation:

Finally, thank you for your letters. As you may realise, I have a whole load of work here as regards the future organisation of the institute which, if you do not want to think that people are lying, looks fairly sensible. \

To complicate the matter even further I have a few other irons in the fire. Assistance from anyone who knows something is not available within our field, at least not from anyone I would use. It is a case of having to manage as best I can, but the difficulty comes in fitting everything in and fighting over so many factors.

On New Year's Eve in 1957, I wrote Sievert a long letter proposing measures to prepare the ICRP meeting that was planned for March. I said that Taylor was now proposing that the meeting be held at Failla's institution at Columbia University Medical Centre on West 168th Street. You might think the location would not mean all that much - it was still in Manhattan. However, the distance to the UN building on 43rd Street was 125 blocks, a not inconsiderable journey for Bugnard, Sievert and Watkinson if they were suddenly needed by UNSCEAR. The advantage of meeting on Failla's premises was the access to copiers.

* There was a proposal from Czechoslovakia that UNSCEAR's next meeting should be held in Prague; this did not occur, however.

The intention had been to hold the meeting with ICRU but this proved to be impossible. On the other hand, tICRU would be represented by its functionaries, who were Taylor (Chairman), Gray (Deputy Chair) and Wyckoff (Secretary). I wrote:

> It is now important to get out as much material as possible for ICRP to cogitate over before the meeting. I have thought of the following documents to start with:
>
> 1. Circular (ICRP/58/?) containing:
>
> (a) Confirmation of the time and place.
>
> (b) Preliminary programme.
>
> (c) A statement that the Chairmen of the Committees have been asked to provide the Secretariat with their reports in good time before the meeting to allow for advance circulation, as well as that during the meeting, the Chairman of the Committee will present his reports in such a form that they can be published directly after any approval.
>
> Point (c) is obviously included so that the Chairmen of the Committees will see that the Main Commission is expecting them to clarify something. I am particularly pessimistic in this regard, but I think we have to officially pretend that there is no doubt that all Committee reports will be ready in time. If we appear to doubt this, no-one will bother doing anything 'because the others will not be ready in time in any case'.
>
> […]
>
> 6. Various working papers. It would be nice to get something out about the basic philosophy. I have also met many who have criticised ICRP for failing to adequately report the uncertainty behind the MPL values [MPL = Maximum Permissible Level]. From one place I have received a proposal to introduce an historical chapter which gives an account of the development of the MPL values from the old days and reports the justification for each change that has taken place so far, as well as also clearly showing the justification for the last choice.
>
> (With regard to the last point, Failla agreed but was afraid that the clearly-reported justifications would be fairly scant – in my opinion, all the more reason to actually bring them to light.)
>
> […]
>
> The Committee reports are in the hands of the Good Lord yet the devil's tail is wrapped around them. With regard to the introductory chapter which I think ought to include (if necessary, divided into several chapters) an historical overview, a report of the justification for current levels and a common, clear line for the following special recommendations, I hope that Failla will write a working paper proposing uniform recommendations. The section reporting the justification for the levels he chooses can hopefully be worked out during the actual meeting. However, if Failla has no time for more – who better than you to write an overview of the historical development and the previous justifications? I am convinced that it is important.
>
> The Committee reports make pretty dismal reading and are a perfect example of how feeble ICRP is without a permanent Secretariat. The current position is as follows:
>
> Committee I — <u>Failla's</u> report has remained unchanged since Geneva 1956. There is no change to be expected here other than that which was proposed in New York in 1956 and which partly comes under the introductory chapter about the principles.
>
> Committee II — <u>Morgan</u> writes to me on 13 December: With regard to Committee material that we have sent to Dr. Sievert, all this seems very preliminary and as yet awaits the approval of the Committee members. As soon as we have made progress in arriving at a more final manuscript, I will send it to you.
> Not exactly promising, is it?
>
> Committee III — <u>Jaeger</u> hasn't responded to either of my letters of 11 September and 17 December in which I requested information on the current status of the report and pointed out the importance of having everything ready before the March meeting. Both Smith and Wyckoff have told me that we cannot expect a report from Jaeger, but that it will

probably be Smith who has to write it. Braestrup and Wyckoff, who are members of the Committee, have not received any documents from Jaeger.
Even worse.

Committee IV Johns knows nothing about the report and refers to Mayneord. The latter has not answered my letter of 10 December in which I ask about the report and say that it must be circulated before the March meeting. Committee members like John Laughlin know nothing about the Committee's work and have not received any documents.
!!!

Committee V Straub has circulated parts of his report, but not in any condition for them to be published in the recommendations. I have told him this in a letter, to which I have had no response.

It was obvious that the reports from the five Committees would not be finished in time to be included in the new ICRP recommendations. We had been thinking of a comprehensive volume containing all of the collated material. There were now only two possibilities. I formulated the first in my letter to Sievert from New Year's Eve:

> Because it is extremely important for the recommendations to come out for the summer, I cannot see any way forward other than to take extraordinary measures. The ICRP/ICRU's study group spent (or at least received a grant for) 10 000 dollars. Is it not the case that its essential recommendations are just as valuable? Would it be impossible to get a grant for the purpose – from Ford, the UN or elsewhere?
>
> Set up a temporary Secretariat with two typists and three Scientific Secretaries. They ought to be able to produce a perfect edit if they can concentrate on the task full time for a couple of months. In that time, two girls will have time to type approximately 1 000 complicated (layout) pages or letters, which is my estimation of what will be needed with all transcriptions and all correspondence. The Scientific Secretaries don't have to be genii to be able to process the material if they have access to minutes of previous discussions and can ask ICRP members through personal contacts or correspondence. Three Scientific Secretaries for two months of editing of 150 pages represents an average of 1 page per person per day, which I think is a maximum for responsible work and corresponds to the performance of Binks and his people, although now during normal working hours. Try and get such a group for Failla, Binks or Stockholm where suitable premises can be arranged; Failla may be the most suitable and I have checked with him that he can allow space.
>
> I think we have to accept the above estimate of the scope of the work and draw the appropriate conclusions or else there will be a mish-mash of recommendations and ICRP will die a death to the tune of the UN Committee's entry march. Will you think this over until we meet?

Sievert received my letter just before he was about to take the train to Gothenburg and the boat to America to get to UNSCEAR's big meeting which began at the end of January. He rang me and agreed with most of what I had written. He had come up with some money for editorial work and had sent Lorentzon to Germany to help Eric Smith to try and draw up a report from Committee III together with Jaeger. But the rest of ICRP's work would languish until March; it was now the turn of UNSCEAR to receive Sievert's full attention.

UNSCEAR's big meeting began on Monday 27 January 1958 and continued throughout February. The work took all the hours of the day and the weekends as well, and we Scientific Secretaries spent many evenings sitting together with chain-smoker Chair Professor Bacq, until way after midnight. There was no time for me to travel the 40 kilometres to White Plains; I was forced to sleep at the nearby Tudor Hotel for a few hours late at night for several weeks.

Appleyard had taken care in choosing the meeting premises. The conference rooms for the different types of UN meetings there were in a special, low building next to the Secretariat's 38-storey building. There were premises for the General Assembly and the Security Council, as well as a number of smaller conference premises. We could have had a roomier hall, but Appleyard wanted to prevent the delegates from starting to 'give speeches'. What was needed was more informal discussions, and for that they needed as small a room as possible. The following fifteen national representatives were at this important work meeting (in the order in which they sat around the table):

Argentina: Captain Dr. Constantino Nuñes
Australia: Mr D. J. Stevens
Belgium: Prof. Zenon Bacq
Brazil: Prof. Carlos Chagas
Canada: Dr. E. A. Watkinson
Czechoslovakia: Prof. Ferdinand Hercik
Egypt: Dr. A. Halawani
France: Prof. Louis Bugnard
India: Dr. V. R. Khanolkar
Japan: Dr. Masao Tsuzuki
Mexico: Dr. Manuel Martínez Báez
Sweden: Prof. Rolf Sievert
The Soviet Union: Prof. A. V. Lebedinsky
The United Kingdom: Dr. E. E. Pochin
USA: Dr. Shields Warren

Appleyard was very envious of the Secretariat's appearance. The Scientific Secretaries did not get to take off their jackets and sit in their shirt sleeves in the meeting premises. The delegates can do what they like, said Appleyard, but the Secretariat must keep up appearances.

In addition to Sievert, the Swedish delegation included Torbjörn Caspersson as the representative's substitute, plus Bo Aler and Arne Nelson, and Professor Gert Bonnier as consultant. The American delegation was enormous. As well as Shields Warren, who was representative with Argonne biologist *Austin Brues* and Merril Eisenbud as substitute, there were no fewer than twenty advisers, some of whom were well-known scientists such as Failla, Harley and Marinelli or well-known representatives of the AEC such as Charles Dunham and Forrest Western. However, the majority were unknown to us.

The Soviet delegation consisted of five people. It was led by biologist A. V. Lebedinsky – whom Sievert nicknamed 'Lebby' – with physicist *K. K. Aglintsev* as substitute and biologists *A. M. Kuzin* and *N. A. Krajevskij* as advisers. The fifth man (all of the delegates were men) were given the title of 'Secretary' in the list of participants.

The British delegation was few in number. As representative, Dr. Pochin had support only from Greg Marley as substitute. The Argentinian delegation now included Dan Beninson as the representative's substitute. He had left the UNSCEAR Secretariat and been replaced by the Norwegian *Arve Kjelberg*. The French delegation stated no substitute, but Henri Jammet was one of three advisers.

Appleyard was careful to remind the Committee that it was not entitled to steer the work of the Scientific Secretaries. The Secretariat was not the Secretariat of the delegates but part of the big UN Secretariat under Dag Hammarskjöld. The Committee could accept or reject the proposed texts that came from the Secretariat, and it could also formulate its own texts because the final report was that of the Committee and not of the Secretariat. But the Secretariat was free to put forward its own proposals and its own analyses. With reference to Hammarskjöld, Appleyard insisted that the Secretariat was an independent body, the equivalent of the Member States.

Being Scientific Secretary and defending a Secretariat document and convincing the delegates that they ought to accept it as their own was a new experience. I had thought I was shy and reticent, but I was now forced to defend what I had written and stand up to criticism. It was a time from which I learnt something. Apart from the short plenary sessions, the Committee worked in mainly two work groups, one biological and one physical. None of the delegations' experts were mincing their words at the work group meetings and it was necessary for the Scientific Secretaries to embark on discussions that could sometimes become really heated. The simultaneous interpreters had an important task but the impressions you got from someone who was speaking were often formed more by the way in which the interpreter expressed himself rather than what he was actually saying. Sometimes the interpreters competed to be the first to spontaneously get an agreed and completely irrelevant word such as ‘umbrella stand’ into his version.

A costly episode occurred when a physicist from the Brazilian delegation took to the floor and spoke in a never-ending flow of words. No interpretation into English was heard through the headsets and one delegate after the other looked over towards the interpreters’ booths and pointed to their silent headsets. Then the voice of the interpreter was finally heard, saying ‘The man hasn’t actually said anything yet!’

The shadow of the cold war fell over the first meetings. The delegations from the Soviet Union and the USA included delegates who never expressed an opinion and who were not known to anyone as scientists. Rightly or wrongly we talked about political commissars. David Sowby recalls the situation (Sowby, 2001):

> Inevitably, the committee divided into camps. There was what the USA called the ‘friendly Western’ countries, consisting of themselves, the UK, Australia, and Canada. Another camp comprised the USSR and its minions – Czechoslovakia and, to a lesser extent, the UAR. On the Sunday afternoons before those early meetings we happy band of ‘friends’, each with a political adviser hovering around, were summoned to the US delegation office. There, we were virtually given our marching orders by the American delegate, Dr Shields Warren. Shields was a well-known pathologist from Boston. Sitting at the table, he resembled a rather sleepy old lizard, but he was an astute operator, and had the confidence of the US State Department. Our delegation was under instructions from the Canadian Department of External Affairs, which in matters to do with nuclear testing then took its line from the State Department. Later, however, Canada took an independent line on this.

The first UNSCEAR report took shape during the long February meeting and then mostly editorial work remained to complete the report. This formed the basis for the comprehensive UNSCEAR reports which summarise what has been known since 1958 about human exposure to ionising radiation and radioactive substances as well as the knowledge of the biological effects of radiation on humans. The 1958 report ended up covering 230 size A4 pages, but some later reports have been close to 800 pages. The UNSCEAR reports constitute a unique source of knowledge on the human radiation environment and radiation risks, and the Committee has garnered considerable respect and esteem for its scientific achievement.

You might wonder why the UN’s General Assembly has a Scientific Committee and whether or not UNSCEAR’s scientific work could not be done equally well by an International Committee under UNESCO or WHO, for example. In so wondering, you forget that, no matter how important it is, the reporting, which was the objective of the activity in the first few years, has become a by-product in recent years. The most important thing about UNSCEAR has been the fact that it *existed*. It represented an established forum for scientifically assessing a sudden threatening situation with comprehensive radiation risks such as a reactor disaster or a nuclear war. At UNSCEAR you can analyse the scientific consensus and separate this from political discussions to give the politicians an agreed platform from which debates can be led. Suddenly setting up a credible group of scientists in a crisis situation would be difficult. With UNSCEAR, such a group is already established, which is of great value.

During UNSCEAR’s long February 1958 meeting, the delegations competed to outdo one another when it came to receptions and cocktail parties. These gave the opportunity to discuss sensitive matters in detail or to create compromises as regards formulations that were difficult to accept. However, the distrust was also tangible here during the cold war. The period of 1956–1961, i.e., between the 1956 Suez crisis and the Hungarian Uprising and the 1962 Cuba crisis, was a period of mistrust. My American friends said that, after having visited the Soviet Union’s vodka party, the CIA asked them to report what had been said each time.

A rare interlude concerned Colonel K in the Soviet UN delegation. One day, Dan Beninson asked me for help. He had an Argentinian colleague who was about to return home but who had first told Dan about Colonel K. ‘I think he’s trying to get me to be a spy,’ the Argentinian had said, ‘and he’s invited me to lunch and made strange statements. I'm going home now and may never see how this would have turned out. Can't you take charge of him?'

Dan, with his enormous curiosity, was unable to resist temptation, but because he did not want to be incriminated, he asked me to go along as a 'chaperone' to his meetings with Colonel K. The Colonel was a powerful young man with a child-like appearance and child-like assurance. We pulled his leg a fair bit when he attempted to defend the state of the Soviet Union. One day he asked if we liked vodka and our cautious answer was yes. 'Then you have vodka!' declared the Colonel cheerfully in his rudimentary English.

After that, nothing happened for a while, that is until UNSCEAR met in February. A cocktail party was held at the Soviet Embassy for UNSCEAR and its Secretariat. There I met Colonel K again, who explained that he had not forgotten his promise. The following conversation took place.

> 'Vodka's here,' he proclaimed proudly.
> 'Excellent!' was my reply.
> 'We must arrange the handover,' said Colonel K.
> 'That's easy,' said I, 'I can take the vodka with me when I go.' Colonel K shook his head.
> 'You can't do that,' he said, 'not here, not this evening.'
> 'You can give it to me at the UN tomorrow then,' I suggested.
> 'In your room?'
> I nodded.
> 'But you might not be there. Room might be empty!'
> 'If room's empty,' said I, 'you put bottle on desk.'
> 'Bottle on table?!!'
> 'Yes, why not?'
> 'But a bottle's a bottle! We must arrange!!!'
> I was now starting to become tired of the good Colonel.
> 'Of course a bottle's a bottle. A bottle has to be a bottle because if bottle were not bottle, vodka would run all over the table!'

I never did receive any vodka. The Colonel had chosen to 'arrange' the handover, presumably in some way which would have made me feel guilty and thus ripe for continued persuasion, on a bench in Central Park or something. He had not counted on the immunity that is offered by a good conscience.

Colonel K was not the only person who acted suspiciously. The UN building was literally smouldering with intelligence activities, and the USA was inflamed by the fear of Communism. Nina Nekrassoff, my very language-orientated secretary, wanted to be a simultaneous interpreter, bearing in mind the good pay, but found that it was not a matter of simply having good language skills. Once in the 1960s when I returned to New York as a member of the Swedish UNSCEAR delegation, she approached me in tears. 'The Russians here say I'll never be approved as an interpreter if I don't give them any information,' said Nina. 'They say I must remember that I am actually Russian and must be loyal to my fatherland. The State that drove my father to exile! And when I get home the telephone rings – 'This is the CIA office at lower Manhattan' – and the Americans already know what has been said and want me to do as the Russians say but then tell the CIA everything! My telephone is bugged and unsavoury types ask my little girl questions while she's playing outside. I'm so ashamed!'

I explained that *she* was not the one who should be ashamed. 'Kick them on the shin and spit in their face next time! Report it all to the Secretary General! Shout and swear, but *don't feel ashamed*!' But Nina cried a while longer and said that she did not want to quarrel.

The incident with Colonel K and the vodka may be seen as a comical episode, but indicates serious problems - both the political extortion situations that can arise within international organisations, plus the alcohol culture which can lead to tragedies. Alcohol has played a significant role in the international community. At UNSCEAR's meetings over the years, many of the delegations have arranged receptions with alcohol and snacks. Ironically enough, in some cases during the early years, the receptions that were held by the Soviet Union and Czechoslovakia were the most luxurious where food, drink and beautiful salons were concerned. The Swedish delegation was also given entertainment money to arrange receptions which pertained to those who were more popular. As the years went by, this entertainment was generally reduced, not so much for alcohol policy reasons but due to a much more limited economy. The dangers that hospitality with alcohol leads to for people who are predisposed to alcoholism include the temptations that lure them on flights where alcohol is easily available. Unfortunately, I have seen colleagues lose their lives due to the widely available and tempting abundance of alcohol. Luckily, neither I nor my Swedish colleagues who worked internationally had a tendency towards alcoholism, but this was probably down to luck.

When someone is exposed to all this generosity with alcohol at the international meetings, that someone can choose one of two paths. The first is to abstain completely from all alcohol and ask for a soft drink instead. This is an honourable attitude to have but it can easily lead to isolation. The equivalent action where tobacco is concerned is easier; when I was offered cigarettes, my standard answer was 'Thank you but I haven't started smoking yet!' No offence is taken to this joke, but the teetotaller is often seen as someone who does not properly fit into society.

The second path involves accepting the alcohol but having enough discipline to limit your consumption to the amount that you can actually tolerate. If you do not know what you can tolerate or you know that you cannot tolerate it, alcohol-free drinks are the only solution. If on the other hand you can tolerate it and succeed in keeping your drinking to a moderate level – which is a debatable gamble – the reward is that you are accepted, which can be very helpful in terms of international cooperation. It is rare that important decisions are made and agreements are reached around the meeting table – they are made in private. You can deny that this is the case, but remonstrations would be needed to try and change the situation.

You must also be aware that the hospitable host who presses you to take alcohol likes to see colleagues who are so elated that they tell you things that you would not otherwise hear about. The Russians in particular, whose alcohol habits have never known any bounds, used to expose their guests to floods of vodka. To protect ourselves, my Swedish colleagues and I used to avoid going to the Russian receptions without having eaten anything, and we sometimes drank thick cream beforehand in the hope that it would help us.

After the big February meeting, the work at the UNSCEAR Secretariat calmed down and I was able to return to concentrating on the preparations for the meeting of ICRP's Main Commission and the ICRU functionaries at Failla's premises in March. A preparatory meeting had taken place at the start of February between the functionaries of the two Commissions. We had then drawn up draft articles of association for an umbrella organisation that would tentatively be called the International Radiation Protection Institute. This was just one of the many proposals for an international radiation protection organisation worked out by Sievert over the next few years.

ICRP's March 1958 meeting took place in the library at Failla's institution. The dominant players were Sievert, Taylor, Failla and Mayneord. Karl Morgan also participated in the discussion. Bob Stone sat slumped down in a chair and looked as though he was asleep for most of the time, but woke up without fail when it came to the important decisions. At the time, I was not aware of Stone's effective inputs into radiation protection during the Manhattan project. Watkinson was also mainly silent, not joining in the discussion until it came to finding a compromise between conflicting desires. He then often found a content-free formulation that sounded good and could be accepted by everyone; I learned new, diplomatic words such as 'appropriate'. I do not recall any contribution to the discussion from Bugnard, Holthusen or Jaeger, although all of them were there.

The future of both of the Commissions was also a burning question at the 1956 meeting. In spite of Sievert's grandiose plans for a new international organisation, the arrangement with the International Congress of Radiology (ICR) as the parent organisation was questionable. Appleyard had previously mentioned the possibility of a connection to UNSCEAR. I had discussed the matter with Failla, and in a letter to Sievert on 26 November 1957 I had written:

> […] Returning to the big question regarding the forthcoming external organisation, it is clear that Failla, like all others, senses that the time is now right for something to be done, but that he, like others, is not sure what.

> We discussed the parenthood problem and I mentioned the possibility that Appleyard and I wrote to you about. In principle, Failla had no objections. He obviously wanted to think about other possible 'fathers', but he completely agreed that the ICR was unsuitable for the reasons we previously discussed. He mentioned IAEA but agreed with me that it was possible to use the same argument here as against the ICR. However, he did think that WHO was a serious possibility.
>
> One of Failla's main reasons for wanting to ask Bunche was that he was unsure as to whether the UN Committee really would continue to exist when the fallout problem became less relevant. When Appleyard and I discussed the possibility earlier, Appleyard's point of view was that both reactor waste and radiological work would always mean that sufficient problems remained to guarantee the continued existence of the Committee, as well as the fact that, in the worst case scenario, it was better to have a father who dies later on than never to have had one at all
>
> (considering the economic matters, he clearly counts the ICR more as a mother than a father).

And still the question could not be settled. It was believed that Sievert would continue his efforts to find economic support for an independent parent organisation and that it was important to continue informal negotiations with the UN concerning the cooperation with UNSCEAR. The UN's plans, which were still not clear, were worrying. Sievert would later word his concern about ICRP's future in a letter to Taylor on 3 May as follows:

> Frankly speaking, I always feel concerned when we speak of the world-wide recognition of ICRP and ICRU and of our leading position.
>
> If we continue on the present scale of our work I am sure that we will soon lose our reputation because we have not sufficiently realized the new order of importance of our task. Do you really think that ICRP with its limitation in specialists and means can take the responsibility of establishing [dose limits] affecting the entire atomic energy work? I am convinced that this will, within a few years, be impossible if we are not closely linked to a powerful safety organization working on a very broad basis.

So, Sievert was worried that, as a consequence of a lack of resources, ICRP would not be able to live up to the requirements that would be set by the Commission, and that other international partners would take over. In another letter to Taylor (of 17 April) he would end up writing: 'I worry very much about how our Commissions will be able to compete with an organization set up by the UN including many of the specialists in our Commissions and Committees and having the advantage of being able to get substantial support from the UN'.

The other important matter discussed in Failla's library was how ICRP would manage to publish its new recommendations before the Commission was trampled on by others. In my letter to Sievert from New Year's Eve I had proposed an acute, special appointment of three Scientific Secretaries for two months to complete the recommendations. The Commission modified the proposal and set up an Editorial Committee that could be convened for intensive work for a few weeks, tentatively at the Marine Biology Laboratory at Woods Hole, Cape Cod, where Failla used to spend his summers. The Editorial Committee would have Failla as Chair, me as Secretary and otherwise consist of Elda Anderson, Harald Rossi and David Sowby.

The Commission also discussed a number of important principal matters in the new recommendations. The fact that the intention was now also to state a dose limit for the public led to the question of whether doses of radiation from natural sources of radiation should be included in the limit. A decision was made not to recommend this. Failla wrote the following in his basis for discussion (Taylor, 1979):

> There are populated regions in which the background level of exposure is considerably higher than indicated above [3-4 rems in 30 years, i.e., about 1 millisievert per year]. If permissible limits recommended by ICRP included background radiation the contributions from man-made sources would have to be correspondingly lower. The present state of knowledge does not warrant this restriction. However, ICRP should

> point out that omission of background radiation does not imply that its effects are negligible. When more knowledge accumulates, individual countries or regions can make suitable adjustments.

Failla's statement is interesting. The idea that the knowledge situation did not require a limitation of the sum of the doses from the background radiation and artificial sources of radiation – because the sum would be low enough – is natural if you consider deterministic injuries for which the dose is required to exceed a certain threshold value. The comment that the harmful effects of background radiation might not be negligible assumes that Failla also reckoned there could be harmful stochastic effects without a threshold value for the dose.

It was also agreed that nor would doses of radiation to patients be included in the dose limit. Failla wrote: 'ICRP must assume that medical exposure is necessary and that it is not within its competence to restrict it'.

The ordinary discussion then continued, saying that – bearing in mind the risks of hereditary injuries – it was primarily important to limit the dose to younger people. A limitation of the weekly dose to 300 mrem as before would mean an annual dose of 15 rem (150 mSv), which was now thought to be unacceptable for younger people. Failla forwarded the American proposal to have the accumulated dose (D), expressed in rem, limited by the formula $D_{max} = 5 * (N – 18)$, where N is the age expressed in years. This would ensure that the average annual dose would not exceed 5 rem (50 mSv), even if an annual dose of 15 rem could be accepted for individual years.

Failla forwarded the Commission's points of view on his proposal to his editorial group, which met for two weeks in New York in May and drew up a proposal which was sent out to members for comments.

At the UNSCEAR Secretariat, I was primarily wrestling with two problems. My actual assignment was to compile information on the doses of radiation to patients mainly from x-ray examinations. In order to facilitate comparisons between different countries, we had asked to receive these details in a uniform format which, not unexpectedly, did not occur. I was therefore obliged to request supplementary information, which was time-consuming work. The information from Sweden was an exception because it consisted mainly of Lars-Eric Larsson's examination results. He knew what was needed.

Lars-Eric defended his thesis in May with special permission following special efforts on the part of Sievert and Professor of Physics Erik Hulthén because he did not have a licentiate degree. I had written him a few letters in the spring; he had made the same mistake as Karl Morgan previously and used a formula which yielded a genetically significant radiation dose which was a factor two too high, and I could not use his results for the UNSCEAR report without recalculating them. Lars-Eric referred to the fact that both K. G. Lüning and Gert Bonnier had proved him right. And his calculations certainly were correct, it was just that he had chosen a definition which meant that the annual genetically significant dose from the background radiation's 1 mrem instead became 2 mrem, which was unfortunate.

My second problem was beyond the scope of my actual assignment, but when Dan Beninson had left the Secretariat and before Arve Kjelberg had found his feet, Appleyard asked me to also look at the presentation of the dosimetry where the radioactive fallout from nuclear weapons testing was concerned.

This is where the situation became complicated because a number of periods of time had been superimposed. Each year's injection of long-lived radioactive substances in the upper atmosphere leads to a fallout on the ground over the space of many years. The fallout that takes place during a given year may remain for many years and contaminate crops. The resulting contamination of foods therefore leads to an equally prolonged uptake of long-lived radioactive substances in the human body. Bone-seeking radioactive nuclides such as strontium-90 remain in the skeleton for a long time and again lead to the protracted irradiation of bone tissue and bone marrow.

The Committee attempted to calculate the annual number of cases of leukaemia if nuclear weapons testing were to continue for as long as there was an equilibrium situation (i.e., the number of radioactive atoms supplied was equal to the number of those disappearing through inaccessibility or radioactive decay). It also attempted to calculate the number of cases of leukaemia as a consequence of the tests that had already been carried out. The estimated maximum dose of radiation during a lifetime (70 years) was used as a measure of the risk.

Some of the superpowers' experts first compared the low annual radiation dose that had been measured from the nuclear weapons testing and the significantly more substantial annual dose from natural sources of radiation. However, everyone was aware that continued tests would gradually increase the annual dose. Where was this leading us?

Suppose that an emission of radioactive substances into the atmosphere causes the fallout of radioactive substances to the ground, making the first year's radiation dose 100 units. Then suppose that some of the radioactive substances are still there next year and that year will lead to a dose of 70 units. In the third year, some of the radioactive substances are still left and then, let us say, give a dose of 40 units. For the fourth year, we can assume for this example that the dose will be 10 units. And finally, for the following year, that the dose will be negligible.

Let us then assume that equally radioactive substances are released in the second year. The 70 units from the first year's emission then have a dose of 100 units added to them. To the 40 units which in the third year are left over from the first year's emission you add 70 units from the second year's emission and a further 100 units if there is also an equal-sized emission during the third year. If the emission continues to the same extent, year after year, the following doses will be received:

First year	100	= 100 units
Second year	70 + 100	= 170 units
Third year	40 + 70 + 100	= 210 units
Fourth year	10 + 40 + 70 + 100	= 220 units
Fifth year	0 + 10 + 40 + 70 + 100	= 220 units
Sixth year	0 + 0 + 10 + 40 + 70 + 100	= 220 units
The same amount for all subsequent years if the emission continues		= 220 units

As of the fourth year, the dose each year will be 220 units and will consist of 100 units from same year's emission, 70 units from the previous year's emission, etc., i.e., 100 + 70 + 40 + 10 = 220. But the sum of the annual doses caused by one single year's emission is also 100 + 70 + 40 + 10 = 220.

It is the sum of the annual doses of radiation after one year's emission that is called the dose commitment from the emission. This English name was coined by Appleyard. We see from the numerical example that the dose commitment from one year's emission is equal to the maximum future annual dose if the emission continues to an unchanged extent year after year. Limiting the annual dose commitment (220 units in the example) rather than the annual dose (which was 100 units in the first year but which then gradually increased) means that you can keep the maximum future annual dose under control right from the start.

As the example shows, this is a very simple connection, but in 1958 it was difficult to get people to understand the dose commitment. The 220 units in the example need not be a dose that any existing person receives. It was difficult to understand dose commitments for time periods of hundreds of years; you added together annual doses of radiation to completely different people or, if this is how you preferred to look at it, to a fictitious person who never aged and who would be alive for eternity. But the dose commitment does not profess to be an actual dose of radiation – it is a mathematical aid to estimate future average doses of radiation.

Using the principle for the dose commitment would make our calculation of the maximum annual dose much easier in the future if nuclear weapons tests were to continue unchanged. However, we had difficulties convincing experts outside the Secretariat that there was no need to go through the more involved procedure to calculate a future 70-year dose. Nor did we have access to any computers. In the end, we received a special grant to engage IBM to perform a calculation. We then found ourselves in new difficulties. The IBM computer experts did not understand our problem and we did not understand them. After IBM had sent one team after another for discussions with us, they finally succeeded in establishing a group who understood us and it was possible to perform the calculations.

It was not long now until UNSCEAR was to meet again in June. We prepared a work document on the dose commitment but realised that it would be impossible for an unprepared Committee to discuss this. The Member States were therefore invited to send experts to a special meeting at the start of June to discuss the work document before the Committee's meeting. The meeting took place on 3–7 June in

New York. The expert group consisted of experts from Argentina, Australia, Canada, France, India, Japan, the United Kingdom, Sweden and the USA.

In the work document I had also developed a method of calculating the dose commitment from strontium-90 in the skeleton. The expert group thought that the document would probably have affected the style of the relevant section of the UNSCEAR report had it been available to the Committee earlier. The experts thought that the document indicated possibilities of better consequence calculations and that it clarified the difficulties that the Committee would have to avoid in order to be able to perform its simplified calculations.

At the same time as UNSCEAR's expert group, on 2–6 June to be more specific, the functionaries of ICRP, ICRU and UNSCEAR met in the UN building at Sievert's initiative. These experts included Bacq, Failla, Gray, Sievert, Taylor and Watkinson (who was Deputy Chair of UNSCEAR at the time). They discussed a number of forms of organisation and cooperation, most of which were affiliated to UNSCEAR in some way. The problems are summarised rather well by comments made by the American Atomic Energy Commission (AEC) when it was questioned by Failla or Taylor regarding points of view (Taylor, 1979):

> ICRP-ICRU has never had the assurance of financial support. The participation of member scientists has been at their own expense. This has limited activities of the organizations and now, with the increasing use of radiation, this limitation should be relaxed. ICRP-ICRU might gain in stability and stature if attached directly to the UN with their operating funds, which would provide primarily for salaries, provided out of the UN budget. Since this would raise many difficult problems, such as whether the scientists should serve as individuals or as governmental representatives, we would prefer to look to the Department of State for guidance on this point. Possibly IAEA could be a source of funds. It seems to us that ICRP-ICRU could also acquire a more firm financial base by receiving grants from private foundations or from governments. If this approach is feasible, it may be the most suitable method. Particular care must be taken to guard against a loss in the independence of ICRP-ICRU and above all to insulate these commissions from political influences.

During the period of 9–13 June 1958, UNSCEAR met to finally approve the report arranged by the Secretariat since the February meeting. Sowby has given a lively account of this meeting (Sowby, 2001):

> One of the traditions of UNSCEAR was - and still is – that, during its working sessions, politics is subservient to science. Of course, being a creature of the UN, there are political overtones, but the committee relegates them to short plenary meetings at the beginning and end of a session. However, there was a notable exception at the committee's final meeting in the summer of 1958, at the point of adopting its first, innovative, report. At the last moment the USSR delegation introduced a paragraph that they wanted included in the report's scientific conclusions. They proposed that the committee should recommend that all nuclear testing should cease. Most of the other delegations were against this procedure, as they felt that such a recommendation was a political matter that should be left to the General Assembly of the UN.
>
> There then ensued an enormously long plenary, at which there was a marathon argument, mainly conducted for and against by the delegates of the USSR, Professor Lebedinsky, and of the United Kingdom, Dr Bill Pochin. Everyone else sat back to enjoy the battle. It was like the Men's Final at Wimbledon – one of the contestants would bring forward a devastating point, and we thought: 'that's got him, he'll never be able to counter that'. But he did, and came back with a vicious return. And so it went on, hour after hour. Finally, the chairman, Professor Zenon Bacq, suggested an adjournment, during which senior delegates would meet in the secretary's office to see whether a text could be agreed. The Canadian delegate was included in this gathering, and he subsequently reported to the rest of us that things became very heated up there. At one point Bill Pochin had a row with Rolf Sievert, in the course of which Pochin kicked a hole in the wall. Later that afternoon, Sievert, the real gentleman that he was, went out and bought a bunch of roses for Pochin.

Sowby goes on:

> During the informal discussions the Indian delegate, Dr Khanolkar, proposed a compromise resolution, to the effect that if testing stopped then *[the radioactive]* fallout would decrease. This self-evident statement seemed to satisfy the by-now weary delegates, who were anxious to go home. The rest of us were sitting around in the committee room waiting for the Great Ones to come up with a solution, when in came the delegates. The word went round: 'they've agreed a text'. Unfortunately, however, no one thought to inform the Argentinian delegate, who hadn't been party to the discussion upstairs, and who would start the voting, which was done in alphabetical order. He thought that the committee was still voting on the original USSR resolution, and when the chairman called out: 'Argentina' he replied: 'no'. A gasp went round the table; we all wondered what was happening. The next to vote was Australia; they also voted 'no'. Then came Belgium; the delegate thought there must have been a change of plan which nobody had told him about. So he played safe and abstained. The next to panic was Canada, who also abstained. Then complete chaos set in; some voted 'yes', someone 'no', and some abstained. While the pandemonium went round the table like a demented 'Mexican wave', I noticed the USSR delegation at the very end of the line, laughing their heads off. [...]

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The result of the pandemonium was that the first UNSCEAR report's summarising Chapter contained three versions of paragraph 54: The Secretariat's, the Soviet Union's and India's.

With regard to the work document on the dose commitment, a separate paragraph (131) was devoted to it, which read:

> A document (A/AC.82/INF.3) entitled: "An approach to a general method of computing doses and effects from fall-out" was prepared by the Secretariat of the United Nations in collaboration with a group of experts of the Committee, as a working paper. It was completed just before the Committee's last session (9-14 June, 1958). The Committee has not had sufficient time to study and eventually to accept this work which was considered to be of substantial scientific interest; it has decided to make this paper available because it will be useful to scientists engaged in calculations of gonad or bone marrow doses and their biological effects.

The Committee was also kind enough to suggest me as the person to have the main responsibility of publishing the document in the scientific literature, and it later appeared in *Health Physics*.*

The dose commitment as soon accepted as a concept and used in future reports by UNSCEAR. It also gradually penetrated the American bureaucracy. The head of the AEC's technical analysis section within the Department for Biology and Medicine, Dr. *Hal Hollister*, wrote to me about the paper in 1964:

> […] I think we completely agree on the content. I can detect no errors or general discussions to disagree with. You should know that I have long been impressed by the scope and quality of the work that UNSCEAR has carried out or stimulated concerning the interpretation of data on the effects of radiation exposure and on the transportation of radioactive substances in the environment.
>
> One of the reasons that I went through the calculations in your paper was that I thought that the very fact that it existed seemed to have largely been disregarded in the United States. I am the first to regret this and thought that a small letter with appropriate circulation could remedy the situation. In parentheses, I think that, owing to the requirements from the Federal Radiation Council, it has actually been remedied.

On the last day of the meeting, Sievert was elected as Chair of UNSCEAR for the following two-year period. This was a departure from the previous procedure where the person who had been Deputy Chair (in this case Watkinson) became Chair for the next period. The deviation took place quite properly, bearing in mind the cooperation discussions between ICRP and UNSCEAR for which Sievert had taken the initiative.

On 14 July I travelled with Marrit and Karin to Woods Hole on the southern tip of Cape Cod where Failla and his young wife usually spent the summer at the well-known marine biology laboratory. This is where ICRP's editorial group was now also to meet under Failla's leadership. The UN Secretariat had formally hired me out to ICRP for the group's meeting. Unfortunately, Harald Rossi was prevented from attending, but Elda Anderson and David Sowby were already there.

I had become acquainted with Elda Anderson when she was in charge of WHO's radiation protection course in Stockholm, and we had then also visited her in Oak Ridge in 1956 when we were driving around the USA. I had first met David in Geneva in April 1957 and then at UNSCEAR's meeting, where he was part of the Canadian delegation.

Failla had reserved rooms for us at the only hotel in the vicinity, the Breakwater Hotel, a big, old hotel with some dignity and mainly full of pensioners, primarily widows who had stayed there with their husbands at some stage of the hotel's former time of splendour and had now returned to surround themselves with nostalgia. Our work meetings took place at the Marine Biology Laboratory where Patricia Failla was doing research at the same time.

Every evening before dinner, Marrit and I sat down with Elda Anderson in a small pavilion outside the hotel for a cocktail before eating. We sometimes played cards. Elda, who was 58 at the time, was dressed in shorts, which the older hotel guests did not like. Even less popular was the fact that we allowed Karin to sit with us in the pavilion. We became well acquainted with Elda Anderson because we socialised outside work. She was a very loyal friend with great experience of radiation protection work and successful radiation protection teaching which many hundreds of dedicated students all over the world could vouch for.

Our task was to write a draft of ICRP's new recommendations based mainly on what had been agreed in Geneva two years before. Some questions still remained, however. Failla was concerned that the dose limit which, logically speaking, ought to apply to doses of radiation that were not a consequence of work with sources of radiation or medical examination or treatment, appeared to be fast approaching the doses of radiation which originated from natural sources of radiation. What would the practical consequences of that be?

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* Vol. 2 (1960), pp. 341–365.

We also had long discussions about the way in which the genetically relevant dose ought to be calculated. Initially, Failla thought you should add up the doses of radiation to the genitals over a 30-year period, i.e., approximately one generation. I maintained that, by definition, the quantity of the 'annual genetically significant radiation dose' already took into account the length of a generation and that there was no reason to state it for any period of time other than a year. David Sowby has described the discussions between Failla and me in Woods Hole (Lindell, 1984):

> In August most of the members of the group gathered in Wood's hole, Massachusetts, to hammer out the final version; during the hammering, Failla and BL engaged in vehement discussions that, to the rest of the group, resembled a composite of a medieval disputation, chess, tennis and wrestling. Eventually the text was agreed, and Failla wrote a prefatory review, which actually comprised about one-third of the entire text of the recommendations. The review was a masterly summary of the current state of knowledge and thinking about radiation protection at the end of the 1950s, and it formed the basis of radiation practice for the next decade. Those 22 paragraphs changed the direction of radiation practice, and led the way to subsequent modifications that were perhaps too daringly innovative for 1958.

'It's said that Karl Morgan is so stubborn that an Act of God is needed to get him to change his opinion, but I'm afraid that wouldn't be enough in your case!' said Failla, despairing about my stubbornness.

In Cape Cod. Parts of Professor Failla's editorial group for ICRP at the meeting in Woods Hole in summer 1958. The picture shows David Sowby, Elda Anderson and Gino Failla. Marrit and Karin Lindell are standing with their backs to the camera. Photo: Bo Lindell.

Our task was to try and compile a report like the one that had been published in 1955 as a supplement to the *British Journal of Radiology*, i.e., a document which contained both the Commission's recommendations and the reports from the Committees. However, we soon found that this would be impossible. After the editorial group's meeting, I wrote to Sievert:

> I have returned to New York today after three weeks with Failla. We have worked hard, maybe a bit hard for Failla, to put together a report, but it looks really worrying.
>
> We must be clear on two things: that it is necessary for ICRP to get its recommendations out in September, and that it is necessary for the recommendations to be, if not perfect, at least mainly correct.
>
> I previously thought it would be beneficial for ICRP to have all Committee reports in one volume because it would be a fairly impressive work. After having read the reports at our meeting, I have now changed my mind. My view is that, rather than increasing the value of the recommendations, Jaeger's and Straub's reports published in their current form would torpedo them. This is nothing that can be improved on or saved through quick, radical action – I doubt that anything other than long-term work within each Committee can improve on the reports. Jaeger is excused in that he has been under the maximum pressure; I can find no good excuse for Straub. Elda Anderson and Sowby were of the same view; Failla has not had time to study the reports in detail.
>
> After having discussed this fact, we agreed that in the current situation it would not be very wise to report this as our view to the Commission in its entirety because many members might then think nothing of publishing something before each Committee report was tip-top. This is something that ICRP cannot afford at the moment – something must be said, and that which is said must be well considered.

In Woods Hole, we were now thinking of two publications instead. In the first we were meant to have had the Commission's recommendations and the reports from Committee I and Committee II, i.e., the fundamental principles. The second report was to consist of the reports from Committees III, IV and V, i.e., recommendations for practical applications. However, quickly being able to include the comprehensive report from Karl Morgan's Committee II proved impossible. Then there were problems with Morgan's stubbornness as regards implementing a policy that did not comply fully with that of the Commission. No separate report from Failla's Committee I was needed in the end; the substance was incorporated into the Commission's recommendations.

When the group's work was finished, we dropped Elda Anderson off at the nearest airport for her return journey to Oak Ridge. She hugged us with tears in her eyes and we did not understand at the time why she was so overcome to be saying goodbye. We did not know that she had been aware for a few years that she had leukaemia. She did not reckon she would meet us again, and indeed she did not.

Back in New York, I wrote to Sievert about the proposed recommendations which were the result of the Editorial Committee's work:

> As you might expect, we will have difficulty getting the new proposal approved. Failla suggests that I travel to Geneva to meet whichever members of ICRP may happen to be there, and take with me a ballot paper and convince them to write on it. If we do not take drastic measures, we will never get anything out and ICRP will fizzle out. On the other hand, if we are too hasty in getting something out, it may be so compromising that ICRP will go out with a bang. The question is, which death is preferable – a fizzle or a bang?

The pressure of the work at the UNSCEAR Secretariat had now eased - UNSCEAR's first big report was on course to be printed. It was published on 10 August and mentioned in *Svenska Dagbladet* the next day. It was now time to leave. We sold our Chevrolet to Harald Rossi, who in turn sold his old Buick, called 'Bouncy', to an acquaintance for the unusual price of a bottle of whiskey (Buick was the name of the American whiskey) a month for as long as the car was running.

ICRP now received undivided attention from me and Sievert. The urgent task was to get the new recommendations approved so that they could be published in the autumn. For this I was prepared to follow Failla's suggestion to travel to the second Atoms for Peace Conference in Geneva in September, which would probably be attended by half the ICRP's members. But the British members were now raising objections. Failla wrote to Taylor on 1 September (Taylor, 1979):

> I am enclosing copies of correspondence with the British members of ICRP. As you will see, if you do not already know it, they suggest a meeting of the full commission 'before any public pronouncements on principles are made and before any report is published'. I suppose what they have in mind is a meeting in Munich next year.* I do not know just what is behind this move, but whatever it is, it is not good for ICRP. I think you, Sievert and Lindell will have to act fast and firmly in Geneva to prevent this. […]
>
> The reports of committees are not in very good shape. Morgan has not sent a final report. I think he is having trouble getting his committee to approve certain pet ideas of his. I heard rumors to this effect in Burlington. However, I think he will have a good report in the end. Probably it will be best to publish now the reports of the Commission and Committee II, for which there is an urgent demand, and the others can be published after the Munich meeting. I asked Sievert to see whether this plan would be acceptable to WHO who provided the funds. The Pergamon Press has offered to publish the whole thing for something like $3.00 a volume. They would like it to be a supplement to '*Health Physics*' but they may not insist on this.

The question of who would publish ICRP's recommendations in the future had been discussed for some time. Sievert did not welcome the idea of it being published as a supplement to a journal as had happened the previous time with Supplement No. 6 to the *British Journal of Radiology*. He thought this detracted from their international character. He had been in contact with Almqvist & Wiksell's publishers in Uppsala himself, and for a time it looked as though the most advantageous thing would be to print the recommendations there. However, an eccentric gentleman suddenly appeared on the scene: Captain *Robert Maxwell* (1923–1991), owner of Pergamon Press which he had founded himself in 1949. Maxwell, who was originally Czech with the name Jan Ludvik Hoch, fought with the British during the Second World War and held onto his title of 'Captain' for a long time. He was a powerful man in body and behaviour, and he was extremely stubborn. When he proposed to Failla that Pergamon Press should print ICRP's recommendations, Failla was very hesitant at first; the dominant Maxwell epitomised the insufferable salesperson, but Failla eventually yielded, perhaps due to tiredness but also because Maxwell was offering good terms.

And that was how Pergamon Press, with its headquarters in Oxford, ended up publishing ICRP's recommendations. The volume that was relevant in 1958 was not given an edition number, but is often referred to as ICRP Publication 1. As of the second edition, which was designated ICRP Publication 2, ICRP's publications had consecutive edition numbers, as they still do. Robert Maxwell was strangely passionate about ICRP, whose publications must have been a very negligible part of his big publishing empire. He took a remarkable personal interest in the Commission's movements and reports. Unfortunately, his life ended in a tragedy with his death under mysterious circumstances and a fraud scheme which meant that his company was put into administration. However, ICRP's publications are still issued with Pergamon's logotype by Elsevier Science, who bought the rights to this emblem.

On 22 August 1958, the Lindell family left New York to travel home on the *M/S Gripsholm*, which arrived in Gothenburg on 30 August. On 2 September I flew to Geneva where ICRP's Committee III and the seven members of the Main Commission happened to be at the time of the Atoms for Peace Conference. All hotels were fully booked and, like many others, I was forced to stay in Lausanne.

An international meeting concerning the detection of nuclear weapons testing had taken place earlier and had ended on 21 August. Those taking part in the meeting were from Canada, France, Poland, Romania, the United Kingdom, Czechoslovakia and the USA. The participants agreed that it would be

* The 9th International Congress of Radiology was to be held in Munich in 1959 and ICRP was expected to meet there at the time of the Congress.

possible to use measurements to detect whether a test ban treaty had not been followed. On the very next day, President Eisenhower explained that the United States was prepared to discuss a treaty to stop nuclear weapons testing. This led to a temporary test stoppage which was respected by the USA and the Soviet Union until 1961, while France carried out its first nuclear weapons testing in Algeria in February 1960.

The next meeting of interest in Geneva in autumn 1958 took place at Sievert's initiative. He had sent an invitation to a dozen international organisations which were interested in radiation protection matters, and eight had agreed to send representatives. These eight were the FAO, ICRP, ICRU, ILO, UNESCO, UNSCEAR and WHO.* Sterling Cole, Director General of IAEA, had said that IAEA had a lot of work within its Health and Safety Division so it was unfortunately unable to send a representative, but that he was personally prepared to discuss the cooperation between IAEA, ICRP and UNSCEAR sometime during the conference.

The meeting took place on WHO's premises in *Palais des Nations*. The participants were welcomed by Deputy Director General Pierre Dorolle. The representative of each organisation recounted his organisation's activity within the radiation protection field. They agreed to keep the cooperation completely informal for the moment but to exchange information.

The second Atoms for Peace Conference took place on 1–12 September 1958. The following was written in the special journal that was issued during the conference (Atomic Markets):

> If those who attended the first Geneva conference on Peaceful Uses of Atomic Energy in 1955 thought that was a monster affair, they will find at Geneva-II that, even seeing, it is difficult to believe. As a matter of fact, U.N. officials believe this year's meeting, certainly the biggest ever held under U.N. auspices, may well be the biggest formal international conference ever held.

On the final day, 12 September, Sir John Cockcroft held a lecture which summarised what had happened during the conference. He pointed out that in the three years that had passed since the first conference, three 'major' nuclear power plants had been commissioned at Calder Hall, Shippingport 'and recently, Siberia'. In referring to the latter-mentioned, it is not clear whether he meant the first Soviet nuclear power plant which was started in Obninsk, 100 km south-west of Moscow, in 1954.

Cockcroft guessed that by 1975, the majority of new power plants would be nuclear power plants. He was also optimistic when it came to running a ship with a reactor, a possibility which had already been realised with the American submarine *Nautilus*. He recalled that the *NS Savannah* was being built as a reactor-driven ship for both passengers and freight and that reactor-driven ice-breakers in the Soviet Union would open up the Northern Sea Route.

Where the possibility of using fusion processes for power reactors was concerned, Cockcroft reckoned that this would not happen for at least twenty years. As regards the risks of handling radioactive substances, Cockcroft referred to UNSCEAR's recently-published report and to ICRP's activity and thought that this gave good grounds for effective radiation protection. He was also optimistic when it came to the risk of reactor accidents. He said that the experience gained thus far had given us 'considerable confidence in safe operations in the future, and we can probably, in due course, count on situating plants in more densely-populated areas'.

During the conference, ICRP's Committee III met under the Chairmanship of Eric Smith because Jaeger was unable to attend. I was at this meeting and remember that Eric, whom I did not yet know that well, viewed my participation with suspicion. He was not a member of the Main Commission and might have seen me as a representative who was monitoring matters on behalf of the authorities.

The seven members of the Main Commission who were present in Geneva met the Emeritus member (not entitled to vote) Sir Ernest Rock Carling on 7–9 September to discuss and, hopefully, approve the proposed recommendations. Binks, Mayneord and Sir Ernest objected to the idea of giving examples of

* FAO = Food and Agriculture Organisation of the United Nations (the UN's specialist agency); ILO = International Labour Organisation; UNESCO = United Nations Educational, Scientific and Cultural Organisation.

how the genetic dose could be distributed between different types of irradiation. I clearly had good arguments with me from Failla because, after the meeting, I wrote to him:

> I do not think any changes to the text are necessary when it comes to 'genetic dose' provided we are thinking of the total dose, but we may need to limit or exclude the detailed discussion about the distribution of the dose from the preface. Because I succeeded in getting the British group to accept the distribution as a concept and also to include the numbers as an example (albeit printed in smaller print), I think it would be unwise to print too much in this part of the preface and risk the discussion starting up again

The seven members who were entitled to vote approved the report with a number of minor changes. The date, 9 September 1958, is the one on the published report, but none of the members were actually convinced as yet. So, on 18 September, I sent the Commission a circular and told them what had happened and which changes had been made, and also enclosed ballot papers for the members who had not attended Geneva.

In the continued editorial work, Failla made further changes that were needed for the sake of logic and context. Therefore, on 3 December, he issued a new circular in which he reported these changes, simultaneously saying that the manuscript had now gone to Pergamon Press and that any further changes could therefore only be made during the proofreading. He reminded everyone that he had written in the preface that 'the final draft was approved by all members', and he hoped that this would not be a disappointment (and it was not). Advance copies of the text were sent to a number of international organisations for information purposes, organisations which had eagerly waited to hear what the Commission had to say. ICRP was back in the mix once more.

In December, Failla still thought that Karl Morgan's report from Committee II could be included in Publication 1, but it proved to be impossible. Publication 1, which was finally published in early 1959, thus contained only the Main Commission's recommendations. The Committee reports were later published as Publications 2–5, which corresponded to the reports from Committees II–V.

In late autumn 1958, Sievert continued to outline different forms of organisation to safeguard the future of ICRP and ICRU. He also approached the Rockefeller Foundation and the Ford Foundation in the hope of economic support for ICRP and was met by cautious benevolence. However, the Rockefeller Foundation approved a grant of 8 000 dollars which could be used to facilitate ICRP's planned meeting in Munich in connection with the International Congress of Radiology in 1959. However, the large contribution would hopefully come from the Ford Foundation.

With regard to the Ford Foundation, Taylor takes some of the credit in his big historical account (Taylor, 1979). In 1959, as Chair of ICRU, he went to visit Dr. Paul Pearson (whose name Taylor incorrectly spelled as Pierson) at the Ford Foundation to discuss a grant for the International Bureau of Weights and Measures (BIPM) in Sèvres outside Paris. ICRU had proposed that the BIPM should establish a standard laboratory for radiation measurements. Taylor wanted to take the opportunity of also discussing with Pearson the grant application he had previously submitted for support for ICRU, and at the same time also remind him of the equivalent application that Sievert had submitted for ICRP. Taylor tells us what happened, referring to himself in the third person:

> When Taylor stepped off the elevator into the office area for the Ford Foundation in New York he walked toward what appeared to be a receptionist's desk. When he regained consciousness, his face was bloody and he found himself on the way downstairs to an infirmary. It seemed this office had heavy, glass-wall floor-to-ceiling partitions and they had just removed a protective palm from in front of one of those frames when Taylor, thinking it was the door, tried to walk through it. The principal result of this was a bloodied nose and several stitches therein – so when he finally got to Pierson's office, after the infirmary trip, he announced that 'the Ford Foundation had better work up an amount of $476,500 or Taylor would sue them for damages.' In any case, that was the amount of money that was being discussed for the two Commissions and the BIMP. Shortly after that meeting, agreement was reached that these three grants would be made.

According to Taylor, this happened in 1959, but as late as 15 July 1960, Pearson wrote to Sievert and thanked him for his proposed support and for the supplementary material that the Ford Foundation had received. Pearson concluded the letter with:

> As I intimated when I was in Stockholm, it will take several weeks for any formal decision to be made with regard to ICRP's proposal. You can rest assured that the proposal will be treated appropriately in the meantime.

However, in a letter to Dag Hammarskjöld of 27 August 1960, Sievert wrote: 'By all accounts, ICRP now appears to have its finances arranged for a 5-year period (annual budget $60 000–70 000), mainly through the Ford Foundation, and a decision hereon is expected in September - October'. Sievert then proposed to situate UNSCEAR's Secretariat in Stockholm to strengthen the cooperation with ICRP.

Hammarskjöld's answer to this on 27 August 1960 was that it was not formally possible to situate UNSCEAR's Secretariat in Stockholm and that the Committee did not have its 'own' Secretariat:

> The members of the group working for UNSCEAR are members of the UN's Secretariat and exist no more as a separate unit than does the Committee itself. They must be kept close to the Committee, which is based at the UN's headquarters, although it does temporarily meet in the other places. It is also administratively impractical and uneconomic to allow small segments of the Secretariat to work elsewhere as separate units. For both principal and practical reasons, the Secretariat that is made available to UNSCEAR must therefore continue to be stationed at the UN's headquarters.

Sievert had to be satisfied with this decision. He certainly continued to outline new forms of organisation, but no longer with any great conviction. He had saved ICRP as an independent body, although the annual budget had to remain at 70 000 dollars rather than the million he had actually been thinking of. It was now a matter of strengthening the Commission's position as an independent organisation together with ICRU.

The uncertainty regarding UNSCEAR's future ceased on 16 December 1958. This was when the UN's General Assembly adopted a resolution which guaranteed the continued activity of the Committee. The General Assembly observed 'with satisfaction' the unanimously-adopted report from the Committee and decided to set the Committee the task of 'continuing its useful work'. The General Assembly also asked UNSCEAR to cooperate with other organisations to avoid overlapping efforts. Finally, everyone concerned was invited to supply the Committee with information and to carry out studies whose results could be useful to the continued work.

It had not been clear whether UNSCEAR would be a one-off phenomenon with an assignment that was finished once it had submitted its report to the General Assembly. However, it had now been confirmed that UNSCEAR was there to stay. UNSCEAR and ICRP would enjoy good cooperation for the rest of the 20th century (and way beyond that perhaps?) and constitute the generally-respected sources of information on radiation levels, the effects of radiation, and radiation protection.

13. THREATS OF ACCIDENTS AND DISASTERS

IN THE BOOK called *Kärnkraften, människan och säkerheten* [‘Nuclear Power, Mankind and his Safety’] (Lindell, 1972) the authors ask themselves how an ‘accident’ should be defined:

Mishap, accident, disaster - what were the defining factors? There has been much confusion in the debate when the word ‘accident’ has been used indiscriminately to cover all three of these concepts. Some might speculate about the cause and think that an aversion to exaggerations, i.e., a ‘disaster’, together with a desire to be fair and not to detract from what an accident is by calling it a ‘mishap’ is typically Swedish.

In the following we will attempt to clarify matters and use the following definitions:

mishap = an undesirable and unexpected event, at least not expected at the precise time it occurs; it may have led to slight equipment damage and greater risks, but not to any serious personal injury

accident = an undesirable and unexpected event that leads to considerable equipment damage or serious personal injuries and possibly also death

disaster = a surprising event that leads to very substantial equipment damage or to personal injuries to a large number of people and possibly many deaths.

Since this was written, some scientific journals have started to avoid the word ‘accident’. This is because we do not want to make the reader believe that an accident is always random and therefore more or less unavoidable and cannot be stopped by preventative measures. There is actually an addressable cause of accidents in most cases, such as carelessness, disregarding instructions, inadequate design, etc. So you then think that in order to clarify this further you ought to specify the undesired event, examples being the derailment of a train, a collision, a fire, etc. Analyses ought also to differentiate between the event (mishap, accident, disaster) whose causes should be investigated and its consequences (as described in the above attempt at a definition).

In ‘Pandora’s Box’ I recounted events where exposure to ionising radiation or radioactive substances had disastrous consequences. These included the early use of x rays before people were aware of the risks to the personnel. These also include the injuries that affected the luminous paint workers who licked brushes dipped in luminous paint containing radium. The Hiroshima and Nagasaki bombings also led to disasters in which the radiation certainly was not dominant yet still had disastrous consequences.

Until now, the two reactor accidents that have occurred in the world and that are of the scope which means that they can be designated as disasters occurred outside the period of time to which this book refers. The one that occurred in the Three Mile Island nuclear power plant outside Harrisburg in March 1979 was a disaster with regard to the equipment and energy policy consequences, although no human being came to any harm. The Chernobyl accident in April 1986 was a disaster in all respects. Yet no reactor disasters occurred before 1979, although there were a number of larger or smaller reactor accidents. However, the accident that occurred in a high-level radioactive waste warehouse in Kyshtym close to the city of Chelyabinsk in the Urals in 1957 must also be designated a disaster. I will reveal more about this as we go on.

You might think that a number of severe accidents could have been anticipated during the Manhattan Project in the 1940s because people were handling such large quantities of radioactive substances and commissioning the first nuclear reactors while still having had no great experience of the risks’ but there was diligent safety thinking and there were surprisingly few injuries to those involved. Two accidents with fatal outcomes during the experimental work were described in ‘The Sword of Damocles’. An additional number of accidents, two of which ended in death, occurred during nuclear physics experiment in the 1950s. All of these accidents were criticality accidents, i.e., a consequence of a surprising, rapid chain reaction in fissile uranium or plutonium because the conditions for neutron multiplication were changed by mistake or due to lack of knowledge. One of the deaths that occurred in a criticality accident in Los Alamos inspired the Dexter Masters novel *The Accident* (Masters, 1955).

The Vinča Accident, 1958

The best-publicised criticality accident was the Vinča accident in (the former) Yugoslavia on 15 October 1958. It occurred at a nuclear physics research institute in Vinča outside Belgrade in Yugoslavia. The institute had already been created in 1948, but the first substantial source of radiation and Yugoslavia's first nuclear reactor was a zero energy reactor for research purposes, called RB. The Reactor was commissioned in May 1958. The accident which led to six people, engineers and students, being exposed to doses of radiation of between 2 and 4.5 gray occurred just six months later. Expert help was sought from Paris and Dr. Henri Jammet came from there to take over the medical responsibility for those who had been irradiated. Jammet decided on a bone marrow transplant for those who had received the maximum doses. Despite this (or possibly as a consequence of this), one student from Belgrade University, *Života Vranic*, died after one month at a hospital in Paris. The cause of the accident is thought to have been inadequate radiation protection arrangements.

Vranic's death led to extensive discussions on the suitability of bone marrow transplants following a radiation accident. This intervention is justified only within a narrow dose area. At lower doses, the bone marrow transplant is unnecessary and downright dangerous. At higher doses it is not sufficient. Because the dose of radiation is rarely homogenous at the time of an accident, in practice it is difficult to determine whether there are conditions for successful treatment. And the dose calculated deep down in the affected person's body is often not calculated either – instead, an unclearly-defined 'dose' is used at the point of application. As in the Vinča case, the dose can be forty per cent higher than the dose deep down so it is then misleading.

Sievert's measurement stations

The possibility of reactor accidents had already been discussed after it had become known that nuclear reactors existed and could actually be built. There was no escaping the fact that the unprecedented activity of the radioactive substances in a reactor led to uneasy feelings. Radiation physicists were used to looking at a few grammes of radium with great respect; at the start of the 1950s, the radiation sources of the two 'radium guns' at *Radiumhemmet* were three and five grammes of radium respectively, and radiation protection presented a number of practical problems. The fuel in a nuclear reactor became so radioactive over time that the activity corresponded to tens of *tonnes* of radium even after several years.

Sievert's small institution was well aware of the risks. In the year before I was employed by Sievert I had already written a poem which reflected the apprehensions that might be felt by the physicists who were not yet familiar with the problems – having said that, knowledge does usually dispel unease. However, in spite of everything, the expected appearance of nuclear reactors in Sweden and elsewhere was not the biggest problem. The USA was no longer the only country to have nuclear weapons. The Soviet Union had carried out its first test explosion on 29 August 1949. The prospect of a nuclear war was terrifying. And even if Sweden were not involved, fallout from radioactive substances was to be expected.

Sievert was very interested in the natural background radiation, i.e., the radiation from naturally-occurring radioactive substances in the human body and external radiation in the form of gamma radiation from the ground and cosmic radiation from the sun and outer space. At the end of the 1940s he wanted to examine the long-term variations in external radiation. He designed field device for this purpose in the form of pressure ionisation chambers supplied with recording devices. His assistant Gunnar Eklund had to travel and take the readings from instruments that had been deployed in different parts of the country, primarily Norrland, taking into account both the cosmic radiation and the impact of the thickness of the snow on the gamma radiation from the ground. Faced with the possibility of radioactive fallout from nuclear weapons explosions and possibly also from reactor accidents, these deployed measurement instruments became even more important.

In September 1949, Sievert requested an extra contribution of 30 000 Swedish kronor from the Atomic Committee to erect six heated, heat-insulated safety cages for his measurement stations but was turned down. Sievert complained in a letter to the Chair of the Atomic Committee, County Governor Malte Jacobsson, and wrote that ‘Given the latest developments, I see no possibility of fulfilling the investigation since my own finances no longer permit me to make such a large personal contribution’. The latter was a modest reminder that Sievert himself had paid for a great deal of his research. He concluded the letter with: ‘If the Atomic Committee cannot be expected to change its decision, I would be grateful for an immediate response so that I can cut down as far as possible on the costs that I have to pay’.

Some time passed before Sievert got his device cages, but four measurement stations deployed from Skåne to Lapland were in operation as of 1950 and another two a few years later. Since autumn 1950, Sievert had also collected dust from the air close to the surface of the ground. The FOA began to measure the beta activity (the activity of electron-emitting nuclides) in the precipitation in April 1953. At the end of the 1950s, Bengt Håkansson took over the responsibility for the measurement stations and it was possible to build a network of technically-improved stations throughout the country.

To begin with, the measurement stations were kept very secret. At Sievert’s institute, Gunnar Eklund was jokingly referred to as ‘Secret Eklund’. In his letter to Malte Jacobsson, Sievert wrote:

> In conversation with Cabinet Minister Mossberg* and through the latter’s negotiation with Superintendent Thulin†, we have been very firmly advised that the device should be assembled so that no-one in the surroundings knows the purpose it serves and so that some monitoring is possible. My proposal to state the investigation as comprising magnetic examinations of specific disturbances from the electric trains owing to said disturbances having caused interesting effects elsewhere was considered extremely appropriate. I have therefore organised the work through a representative who specialises in magnetic measurements, which will facilitate the appropriate erection and care of the device from the secrecy point of view.

The NRX accident in 1952

The first sensational reactor accident occurred in 1952. It affected the Canadian NRX reactor. The reactor had been commissioned in 1947. Thanks to its very high neutron flux density rate‡ it was the world’s most important producer of induced radioactive substances such as cobalt-60.

NRX’ moderator was heavy water and the fuel element consisted of encapsulated metallic uranium. The following quote from the description by Sven Löfveberg and me of the accident in *Kärnkraften, Människan och Säkerheten* (Lindell, 1972) summarises the course of events:

> valves in the basement which meant that the control rods in the reactor changed position and warning lights came on in the control room.
>
> The operating engineer then left the control panel and went down to the basement to see what had happened. He found the cause of the warning signal and closed the valves. When he telephoned up to the control room, the warning lights had gone out, which indicated that the control rods had returned to their previous position.
>
> This was not the case, however – for some reason they had become stuck in an extended position, which made it risky to run the reactor. Because the engineer did not know this, over the telephone he asked his colleague in the control room to press a few buttons which would restore normal working conditions. In doing so, he happened to state the wrong number of buttons, which led his colleague to press a button which withdrew additional control rods; this would not have been dangerous had the first one

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* *Eije Mossberg* (1908–1997), Cabinet Minister and Head of the Ministry of the Interior 1947–1951.

† *Georg Thulin* (1902–1990), State Police Superintendent from 1937–1963 and Under-Governor later on.

‡ See Chapter 4 of ‘Pandora’s Box’ for the quantity flux density rate.

not got stuck. As the situation was now, the nuclear reaction was increased more than had been intended.

After a while, the colleague realised that the reactor was beginning to lose control and pressed a control button which, in emergency situations, would make all control rods shoot out into the reactor and close it. For some reason, all rods but one now jammed and the output continued to rise.

Drastic measures were now required and a valve was opened which released the heavy water into a pool below the reactor. At the same time, the measurement instrument began to register radioactivity in the air and the building had to be rapidly evacuated with the exception of the control room, where the personnel donned gas masks. The temperature had managed to climb so high that part of the fuel element had melted. The uranium metal came into contact with the water vapour and there was a chemical reaction. Hydrogen gas was released and created oxyhydrogen gas explosions. The reactor broke down completely.

Luckily, no human being received hazardously high doses of radiation, but it was some time before they could start tidying up after the accident. Approximately 4 000 cubic metres of water containing 10 000 curies of radioactive substances, approx. 1 000 curies of which were strontium-90, had to be gradually pumped up from the basement. The reactor had to be dismantled, decontaminated and rebuilt. This took place surprisingly quickly and operations were able to resume as early as 1953.

The salvage work was not easy. To prevent any one person from receiving too high a dose of radiation, the tasks were distributed among many and employees who did not normally get involved in radiation work were called in. This reduced the maximum doses – the maximum is said to have been 16 rad, corresponding to 160 millisieverts – but not the collective dose, which has been estimated at approx. 20 man-sieverts.

One of those who took part in the salvage work was the person who went on to be American President, *Jimmy Carter* (1924–) who was a young naval lieutenant at the time. The American Navy sent a group of officers there who were being trained for the first two nuclear submarines. The influence of the accident may well have laid the foundations for Carter's negative opinion of nuclear power.

The cooling water that was pumped out was diverted to the ditch which had been hastily dug within the area in which radioactive waste was normally stored. The ground has great capacity to retain contaminants, but it has been said that approx. 1 millicurie (approx. 40 million becquerels) per day reached the Ottawa River in 1956. That is a big number expressed in becquerels, but it can be compared with the quantity of strontium-90 which was conveyed to the river in 1963 as a consequence of the big Soviet nuclear weapons tests in 1961 and 1962. At that time (in 1963), approx. 0.1 of a millicurie fell directly over the river every day, and more than 1 000 millicuries over its collection area.

The measurement stations in operation

In 1954, Sievert had gained sufficient experience from his measurement stations to be certain that their significance went far beyond his research activity. On 4 April 1954 he wrote a report in five numbered editions, the first four of which he sent to the Prime Minister, the Minister for Foreign Affairs, the Supreme Commander of the Swedish Armed Forces and the Atomic Committee in which he gave an account of his measurement stations and how he had observed seriously-elevated levels of radiation in 1951–1953. At the time of writing the report, he had not yet seen any effects from the Bravo nuclear weapon test on 1 March 1954. Sievert pointed out that in 1951 and 1952, he had found neptunium-239 when taking his measurements (identified through the half-life of 2.3 days), which indicated the use of a uranium tamper[*]. He had no longer noticed any neptunium after the 1952–1953 winter period.

[*] See 'The Sword of Damocles' for information on the word 'tamper' from the French 'tamponner' and English 'tamp' (stop up), a device to reflect a back neutrons and thereby increase the effect of the bomb.

Sievert stated that he had 'most certainly' registered the Russian test (Joe-4) in August 1953, as well as the tests in October 1951 (Joe-2 and Joe-3), and he also believed there were Russian tests in August and November 1952 (he made a mistake here - it was the American Mike which was detonated in November 1952). According to Sievert's report, the explosion that took place in August 1953 (Joe-4) increased the level of gamma radiation at one of his measurement stations to 1/3 above the normal one.

Parker and Healy's impact calculation in 1955

At the UN's big Atoms for Peace Conference in Geneva in 1955, Herbert Parker and *J. W. Healy* held a joint address on the significance of the impacts on the surroundings if a large reactor accident were to occur. The speakers were working on the basis of calculations based on a hundred per cent distribution of all of the radioactive substances in the reactor core and then calculated the consequences of smaller releases. These consequences would obviously depend on the assumptions as to how the release had occurred, e.g., at different heights above the ground, different wind speeds, the duration of the release, etc. The authors made the following comments:

> In any of these models, it may be assumed that a portion of the primary escaping fission products will be retained in the reactor building structure. Only the fraction that escapes into the atmosphere generates an environmental hazard. What this release coefficient may be is best computed locally for each case. […]
>
> In the limit, for a unit with a protective envelope of assured integrity, the release coefficient is zero, the equivalent power level is zero and environmental hazard does not occur. This is the real expected situation; however these data permit potential damage to be assessed for such pessimistic assumptions as a 1% or 10% leak from such a structure.

The authors found that the economic loss from a reactor accident with a hundred per cent release in the American Midwest agricultural areas would be around one billion dollars (costs to purchase contaminated land and compensation for destroyed crops). They were anticipating that several hundred people in the nearby surroundings would die of acute radiation injuries. Death as a consequence of cancer was not yet an expectation.

WASH-740 (1957)

The conceivable consequences stated by Parker and Healy were fairly - although not particularly - startling – the type of accident that would cause the disastrous consequences was thought to be completely unrealistic. The second, often quoted, consequence description came from the American Atomic Energy Commission (AEC) in 1957 and is usually referred to as *WASH-740*. In this report, it was assumed that an accident had occurred in a power-producing reactor with a thermal output of 500 MW*. The area around the reactor was assumed to have barricades up to a distance of 650 metres. The reactor was assumed to be situated approximately 50 km from a city with a million inhabitants. A million people were also assumed to be living closer than that, although the majority thereof at a distance of more than 20–30 km; however, within a distance of up to 10 km, it was assumed that the population consisted of 30 000 people.

It was also assumed that 450 rad (4.5 sieverts of gamma radiation) was a lethal dose, but that doses of radiation lower than 25 rad (corresponding to 250 mSv) would neither cause any injury nor lead to any expense. Considering what we know today, this was a mighty underestimation of the risks. It was also assumed that, in the worst case scenario, 50 % of the core content would be spread in the direction of the wind from the reactor.

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* In a nuclear power plant, an electrical output of only 30–40 % of the heat output is extracted. The electric output from the power reactor which was assumed in WASH-740 would therefore have been no more than 200 MW. For comparison purposes, it is worth mentioning that the units in the Swedish nuclear power plants produce electric outputs of between 500 and 1 000 MW.

With said assumptions, in the worst-case scenario a release of only the volatile nuclides in the reactor (primarily iodine, bromine, xenon and krypton) would cause death up to a distance of 1.5 km. If on the other hand 50 % of the core content were distributed, death could occur up to 2.5 km. If the accident occurred at night and the fission products were not hot enough to climb to high altitudes, it was thought that more than 3 000 people could be killed.

According to WASH-740, in the worst-case scenario, an evacuation would concern half a million people. In this case, the interference with agriculture and its products would concern a surface area of 400 000 km^2 – equivalent to the whole of Sweden – and the costs could be 4 billion dollars (bear in mind that this refers to the value of money in 1957!).

WASH-740 attracted more attention than the Parker and Healy lecture, but the majority were still convinced that a disaster involving such a substantial release of radioactive substances was beyond the realms of possibility. However, the report ended up being quoted frequently in forthcoming nuclear power debates. It is also interesting to note that the AEC, which was not known for being particularly open, released the report freely.

Core damage in the Idaho reactor in 1956

In 1956, core damage had occurred in a reactor at the National Reactor Testing Station in Idaho although no person was injured in this accident. Worse things happened in the accident that affected the SL-1 reactor in Idaho in 1961. I will come back to this accident.

Mayak (Chelyabinsk-40)

The decision as to where to situate the first Soviet plutonium production reactor (corresponding to the Americans' Hanford reactors) had been made by a General who had become enamoured with a picturesque area in the western Urals south of the city of Yekaterinburg (called Sverdlovsk at the time) with its million inhabitants.* The facility was erected in an area that was called after a small town, Kyshtym, but was named after its nearest city and a postcode according to the Soviet tradition, and was therefore called Chelyabinsk-40. Chelyabinsk, which is nowadays also a city with a million inhabitants, is situated approximately 200 km south of Yekaterinburg.

In addition to the facility for plutonium production, a weapons development facility, Chelyabinsk-70, was also established in 1955. A large industrial complex for the production of plutonium grew rapidly at Chelyabinsk-40, which was eventually called Mayak. Seven production reactors were planned. The first was commissioned on 8 June 1948. Another three reactors were ready for operation in 1950–1952. These four reactors were all graphite-moderated and cooled directly, i.e., without any heat exchanger, using water from Lake Kyzyltash by which the Mayak complex was erected.

In addition to the plutonium-producing reactors, the Chelyabinsk-40 facility consisted of a reprocessing plant for the separation of plutonium ('Installation B' – the first reactor was 'A') as well as a facility for producing metallic plutonium ('Installation V' because 'V' is the third letter in the Russian alphabet).

In the first few years, when the matter in hand was to rapidly achieve the aim of producing the first atomic bomb, the doses of radiation to the personnel were very high. There was certainly a prescribed dose limit of 100 millirems (1 mSv) *per day* corresponding to 300 mSv per year (ICRP now recommends a maximum average of 20 mSv per year), but it was not possible to respect the limit. In 1949, the *average dose* among the employees by the reactor was 940 mSv and by the reprocessing plant 1130 mSv.

The discharge to the Techa River, 1950–1951

Given that protection for the personnel was so inadequate, it is hardly surprising that even the protection for the surrounding environment was very poor. The operation of Mayak caused substantial

* This has been described in greater detail in 'The Sword of Damocles'.

discharges to Kyzyltash and thereby to the Techa River, which is the outlet of the lake. The dominant discharges occurred in March 1950 to September 1951 and were substantially reduced after 1951. The Techa River is approx. 20 metres wide on average of and is between half a metre and one metre deep in the summer; the water flow varies between 2 and 10 cubic metres per second. Much of the radioactive substances that were discharged sedimented on the riverbed. Dams were built early on to delay the flow to the nearest village of Metlino, although the latter's 1242 inhabitants were moved away in 1951. In 1950 there were 40 villages along the river before it flowed out into the bigger River Iset. The majority of these villages, which had a total of 28 000 inhabitants altogether, were evacuated after 1951.

From the dose point of view, the totally dominant nuclide in the discharge was strontium-90. It has been estimated that the majority of those living along the river received doses to their bone marrow of between 0.1 and 1 sievert from strontium, but calculations have also shown that the doses could amount to several sieverts. It has been stated that between 1 and 10 petabecquerels (1 PBq = 10^{15} Bq) of strontium-90 and caesium-137 were released into the river system.

When the people in charge understood the scope of the contamination, one department of the Institute of Biophysics in Moscow was moved to Chelyabinsk to examine the health of the population along the Techa River (the department is now called the Urals Research Centre for Radiation Medicine). The respected doctor Angelina Guskova, who was later for a long time part of the Soviet delegation to UNSCEAR, coined the concept 'chronic radiation syndrome' for specific injuries to the blood-forming organs. In the first few years, the presence of such symptoms was thought to be a military secret which the patients should not know about. Their records contained only coded notes which only the doctors could interpret. The doses of radiation were certainly high but, in spite of everything, the exposed population was so small that it has been difficult to be able to show factors such as an increase in the risk of cancer. The presence of leukaemia is still deemed to have been higher than normal, even if it concerns just a few cases. The number of cases of leukaemia in an analysed population comprising approximately half of those who lived along the Techa River was (according to Wils, 2001):

Bone marrow dose (Sv)	Observed person-years	Expected cases	Observed cases
0.005–0.10	103 031	4	3
0.10–0.20	194 858	9	13
0.20–0.50	200 144	10	16
0.50–1.00	93 873	4	9
> 1.00	49 398	2	9
Total	641 304	29	50

The number of cases of cancer (except for leukaemia) in this smaller group has been stated as 969 as opposed to the expected 939, but the surplus of 30 cases is not a statistically significant increase.

The Kyshtym disaster of 1957

However, as if the substantial discharge of radioactive substances from the Mayak complex were not enough, another event, a major disaster, ended up affecting hundreds of thousands of people. This disaster occurred on 29 September 1957 in a waste warehouse. A fourth installation at Chelyabinsk-40 was designated by 'C' and included a waste facility with large tanks for high-level radioactive waste. Up to 80 tonnes of waste, mainly in the form of nitrate, were stored in the tank that caused the disaster. The radioactive decay in the waste raised the temperature by 5–6 °C per day, which meant that the tank had to be cooled with water that was changed every twelve hours. An error of some sort occurred with this cooling system, which meant that the temperature rose to 350 °C and all cooling water turned to vapour. It is (at least to me) unclear whether the explosion that followed was a boiler explosion in the connected tank or whether it was caused by chemicals. In any case, the high-level radioactive waste was thrown high up out of the tank in which an overpressure had probably been formed. The majority of it, approx. 700 petabecquerels, ended up on the ground nearby but approx. 70 pBq were thrown one kilometre into the air and carried by the wind in a north-north-easterly direction and contaminated an area of 50 km * 300 km, the most contaminated thousand km^2 of which were coated with an activity of

70 gigabecquerels per km^2. Approximately 270 000 people lived within the fallout area, and they had to leave their homes due to the severe radioactive contamination.

The explosion in the waste warehouse was considered to be a military secret but it gained an early reputation. *Svenska Dagbladet* carried the following notice as early as 19 March 1959, in which it was stated that the event was a reactor accident:

> **100s of victims in Ural Mountains reactor accident** Vienna (AP) An accident with a Russian nuclear reactor in the Ural Mountains in autumn 1958 claimed 192 victims – 172 people were severely burned and 20 were completely blinded. An area of 8 000 km^2 was 'contaminated' by radioactivity according to the Viennese newspaper *Die Presse*.
>
> The newspaper states that the source of the information about the accident is an unnamed scientist who had recently returned from the Soviet Union to one of the Eastern European satellite states.
>
> According to him, it was a filter system in the reactor which did not work and which led to disastrous consequences.
>
> Twelve Russian villages and a large number of collective farms in the central parts of the Urals had to be evacuated when the wind carried radioactive particles in the direction of the city of Sverdlovsk.
>
> *Die Presse* maintains that the nuclear reactor accident was the primary reason why all foreigners were asked to avoid Sverdlovsk and surrounding areas at this time.

The intelligence services of the western powers were aware of the disaster in 1961. Vague descriptions showing the wrong year (1958) were given within scientific circles from the mid-1970s. More detailed information from critics of the Soviet regime who had emigrated was never considered to be all that credible. Official information from the Soviet Union was not released until 1990.

The CIA knew in 1961 that a disaster had occurred. The following is an extract from a CIA report on nuclear facilities, dated February 1961 and released in 1977 (source: Medvedev, 1979):

> In spring 1958, (deleted) he *[the information provider]* heard from several people that large areas north of Chelyabinsk were contaminated by radioactive waste from a nuclear plant operating at an unknown site near Kyshtym, a town 70 kilometers northwest of Chelyabinsk on the Chelyabinsk-Sverdlovsk railroad line. It was general knowledge that the Chelyabinsk area had an abnormally high number of cancer cases. To go swimming in the numerous lakes and rivers in the vicinity was considered a health hazard by some people. Food brought by the peasants to the Chelyabinsk market (rynok) was checked by the municipal health authorities in a small house at the market entrance where the peasants also paid their sales tax. How radioactive food was destroyed was unknown to source. Food delivered to the plants, schools, etc., by the '*kolkhozy*' and '*sovchozy*'* was probably examined by the latter themselves. Until 1958 passengers were checked at the Kyshtym railway station, and nobody could enter the town without a special permit. By what authority the permit was issued and why the checking was discontinued in 1958, source was unable to say. In addition, some villages in the Kyshtym area had been contaminated and burned down, and the inhabitants moved into new ones built by the government. They were allowed to take with them only the clothes in which they were dressed.

So, the CIA's informer mentions that 'it was generally known that the number of cases of cancer was abnormally large'. This first of all indicates that it was generally known that a disaster had occurred and that the population was afraid of a greater risk of cancer. In reality, the ability to observe a greater risk of cancer is not as easy as you might think, as is shown by the earlier table of the number of cases of

* Unlike the *kolkhozy*, which were collective farms that were not State-owned, the *sovchozy* were collective farms that were run by the State.

cancer among those living along the Techa River. On the other hand, understandably those who are concerned often tend to view that particular accident as the cause of the illnesses that subsequently occur.

Members of the public outside the Soviet Union did not find out about the Kyshtym disaster until 4 November 1976 when the *New Scientist* published an article written by the Russian system critic *Zhores Medvedev* (1925–2018). Medvedev was a molecular biologist and had criticised the infamous but previously favoured geneticist Trofim Lysenko in 1969. In 1970 he was arrested for his critical statements and was stripped of his Soviet citizenship. In 1972, he came to the United Kingdom where he has lived since. The article in the *New Scientist* was written at the invitation of the editors and concerned the role played by science among the dissidents in the Soviet Union. Medvedev, who did not realise that the Kyshtym disaster was unknown outside the Soviet Union, mentioned it as one of the events that had brought together nuclear physicists and persecuted geneticists. He wrote that enormous quantities of radioactive waste had suddenly been distributed in the atmosphere and contaminated thousands of square kilometres of the southern Urals so that thousands of people had moved away and hundreds had died.

Medvedev's article was referred to in a large number of newspapers and journals. His information aroused substantial interest but was categorically dismissed by the specialists in the west. The Chair of the British Atomic Energy Authority, *Sir John Hill*, implied that Medvedev's information was 'nonsense' and added 'I think it is a figment of the imagination'. In the US, the CIA announced that they knew of the accident but had made the mistake of thinking that it was an accident in one of the Russians' plutonium-producing reactors. In early November 1976, the *Los Angeles Times* and a few other newspapers said (according to Medvedev, 1979):

> American intelligence experts said Tuesday that a major nuclear accident in the Soviet Union nearly two decades ago involved a reactor that went out of control, not an explosion of atomic waste as an exiled Soviet scientist asserted last week.

This statement from the CIA provoked another exiled Soviet scientist, *Lev Tumerman*, into writing a letter to the editor of the *Jerusalem Post*. Tumerman, who had emigrated to Israel in 1972, was a strong advocate of nuclear power in that country. He wrote (according to Medvedev, 1979):

> In order to counter reports that the major nuclear accident in the Soviet Union was connected with nuclear power reactor malfunction, I would like to add my eye-witness account of the disaster.
>
> In 1960 I had occasion to make a trip by car to a place near Chelyabinsk in the Southern Urals from northeast of the city of Sverdlovsk in the Northern Urals. We began our trip shortly after midnight and reached the main highway leading from Sverdlovsk to the South at approximately 5 a.m., when it was clear enough to see the surrounding area.
>
> About 100 kilometers (60 miles) from Sverdlovsk a road sign warned drivers not to stop for the next 30 kilometers and to drive through at maximum speed.
>
> On both sides of the road as far as one could see the land was 'dead': no villages, no towns, only the chimneys of destroyed houses, no cultivated fields or pastures, no herds, no people . . . nothing.
>
> The whole country around Sverdlovsk was exceedingly 'hot'. An enormous area, some hundreds of square kilometers, had been laid waste, rendered useless and unproductive for a very long time, tens or perhaps hundreds of years.
>
> I was later told that this was the site of the famous 'Kyshtym catastrophe' in which many hundreds of people had been killed or disabled.
>
> I cannot say with certainty whether the accident was caused by buried nuclear waste, as Zhores Medvedev wrote in the New Scientist and the Jerusalem Post or by the explosion of a plutonium-producing plant, as intelligence sources (quoted by A.P. and the Times) have said. However, all people with whom I spoke – scientists as well as laymen – had no doubt that the blame lay with Soviet officialdom who were negligent and careless in storing the nuclear wastes.

In 1979, Medvedev summarised what he knew in a book (Medvedev, 1979), in which he was surprised that so few people in the west could know about a disaster that was old news to millions of people in the Soviet Union, despite the efforts of the intelligence services. He wrote:

> Although I did know many details of the Urals nuclear disaster as early as 1958, the information certainly did not come from secret sources. Millions of people who lived in the Urals knew about this disaster, although most ordinary people thought the story that a nuclear waste storage site had exploded was absolutely false; they were more inclined to believe the inevitable rumors that an atomic bomb had accidentally exploded. It would have been unrealistic to expect to hide the existence of the disaster from the population of Sverdlovsk, Chelyabinsk, and other cities. The hospitals and clinics in those cities were filled with thousands of evacuated inhabitants, who were held for observation. After a time, when symptoms of radiation sickness began to appear in more distant areas, the evacuation zone was enlarged and people began to be placed not only in hospitals but also in sanatoria and 'houses of rest' (vacation facilities) which were re-equipped as hospitals. Hunting and fishing were prohibited throughout the southern and central Urals and for several years the sale of meat and fish in private markets and collective farm markets was not permitted without special inspection for radioactivity.

Not even when Medvedev's book had been published in 1979–1980 did the western world's engineers and scientists believe the information. I personally thought the book was credible and pragmatic, despite describing a disaster, but many of my colleagues – including several who normally had good judgement – sniffed at it, considering it to be unreliable and sensationalist. Not until 1990, when Gorbachev was at the height of his power and was able to enforce 'glasnost' even for sensitive matters, were the cards laid on the table - and that was when information on the Kyshtym disaster became generally known. However, there is still nothing about Chelyabinsk facilities, Mayak, the Techa River and the Kyshtym disaster in modern reference works such as the *National Encyclopaedia*, not even in the supplement to the latter.

The Windscale accident of 1957

Around one week after the Kyshtym disaster, a far-reaching accident occurred in a plutonium-producing reactor in the Windscale facility in the United Kingdom. In order to understand the conditions of the accident and what happened afterwards, you need to know something about the facility's background. Some of the previous history has been recounted in 'The Sword of Damocles' but I will summarise it here once more. At the same time, I will give an outline of the first decade of the British nuclear energy programme. Although this goes beyond the framework of the subject of this chapter, I think collating this material in the same place will make it easier for the reader.

A great many experiences were exchanged in the somewhat stumbling nuclear cooperation between the United Kingdom and the USA during the Second World War, but the British were completely excluded from information on the plutonium-producing reactors in Hanford.

The Harwell nuclear research station was established at an abandoned military airfield some 20 km south of Oxford in October 1945. John Cockcroft, who was called back from Canada where he led the British-Canadian nuclear energy programme, was appointed to take charge. The Division of Atomic Energy Production was set up in January 1946 with its headquarters in Risley in Lancashire. This organisation, which is commonly referred to as *Risley*, was given the task of planning and building the facilities that were needed to produce plutonium, i.e., factories to produce uranium, reactors to irradiate the uranium with neutrons plus reprocessing plants to separate the plutonium. The person who was appointed head of Risley was an engineer by the name of *Christopher Hinton* (1901–1983).

Both Harwell and Risley came under the Ministry of Supply where a third high official post was set up in 1946, that of a Chief Superintendent of Armament Research (CSAR), which was held by a young physicist and Professor of Mathematics, *William Penney* (1909–1991), who had taken part in the Manhattan Project. Penney's task was to create a secret facility for the production of the nuclear weapon itself.

In 1946, the new American Atomic Energy Act (the McMahon Act) came into being, expressly forbidding the exchange of any information with other countries. The United Kingdom therefore had to continue its nuclear operations under its own steam and was not prepared to allow the Americans to remain in a monopoly situation. Prior to the Atlantic Treaty of 4 April 1949 there was also no guarantee of assistance in the event of an attack, and the world situation deteriorated rapidly with the Berlin Blockade in 1948 and the Korean War in 1950.

The British were certainly denied all information regarding the American plutonium factory in Hanford, but they still knew that the reactors there were graphite-moderated and used natural uranium as fuel. They also knew that they were water-cooled. The work with the GLEEP and BEPO Harwell reactors gave experiences of graphite moderation, and initially the intention was entirely focused on water-cooling the plutonium-producing reactors. However, as early as 1946, the realisation dawned that the United Kingdom did not have equivalent access to water that the Americans did with the Colorado River to cool the reactors in Hanford. A decision was therefore made in 1947 to let the first reactors to be air-cooled - not as later on where a pressurised gas system was used, but with a fan-controlled through-flow of air at ordinary pressure.

In 1947, the British government decided to manufacture nuclear weapons, but Parliament was not informed until 1948.

The building work for the first two reactors began in September 1947. The site chosen was Sellafield on the Cumbrian coast by the Irish Sea where the isolated location had previously been used as a weapons factory. To avoid confusion with the uranium factory, which was simultaneously erected at Springfields near Preston in Lancashire, the name Sellafield was initially avoided and the facility was called Windscale after a steep headland nearby.

Many problems were discussed at an early stage. Each reactor core contained approx. 70 000 fuel cartridges with aluminium-encapsulated uranium. These all had to be sealed. The cooling air was blown through the reactor by eight large fans. The air was then led from each reactor through a 125 metre-high chimney. Originally, they had not expected to have to filter the discharged air, but since John Cockcroft had visited Oak Ridge in 1948 and discovered that they had problems with the emission of particles there, he insisted that Windscale reactors had to be supplied with filters. Building had already started by that time, and the only place where a filter could be positioned was at the very top of each chimney. They were nicknamed ‘Cockcroft’s follies’ (a folly is a building that has no practical purpose).

Another problem was the Wigner effects. There are two types thereof, both of which are due to the fact that irradiating the graphite with neutrons displaces some of the carbon atoms. The first effect is that the graphite expands and changes the way it fits in the reactor. Allowances for this meant that Risley’s designers were forced to redesign once the scope of the problem had been understood. The second effect is that the displacement of the carbon atoms involves an accumulation of energy. This problem had been predicted by *Leo Szilard* (1898–1964) and had been pointed out by Edward Teller on a visit that he and Gioacchino Failla made to Harwell in 1948. However, the British had considered the possibility that this accumulated energy could suddenly be released and cause a fire in the graphite to be an insignificant risk.

Windscale reactor no. 1 was commissioned in October 1950 and no. 2 in June 1951. In 1952, the first irradiated fuel cartridges could be sent for reprocessing and plutonium separation, and in October of the same year, the first British atomic bomb was tested at the Monte Bello Islands off the north-west coast of Australia.

However, the two Windscale reactors produced less plutonium than had been expected. Early work had therefore taken place at Harwell and Risley to develop new types of reactor. In 1953, Harwell had drawn up a proposal for a reactor for the dual purpose of producing both plutonium and nuclear power (dual-purpose reactor for plutonium and power production), christened PIPPA. The building of the first two PIPPA reactors began in August 1953 in an area next to Windscale, called Calder Hall.

Like the Windscale reactors, the PIPPA reactors were graphite-moderated and used natural uranium. On the other hand, they were not cooled with air but with carbon dioxide in a closed system at high pressure. Nor was their fuel encapsulated by aluminium, but by a magnesium alloy called Magnox which had the advantage that it tolerated higher temperatures and absorbed fewer neutrons. In June 1955, due to a greater need for weapon-grade plutonium, a decision was made to build a further two PIPPA reactors at Calder Hall and four at Chapelcross by the Solway Firth in Dumfriesshire north of Windscale.

It turned out that the arrangement with the Ministry of Supply having the main responsibility for the development was not effective enough. The fact that the Russians were able to test an atomic bomb as early as 1949 showed that the British were further behind than they had thought. However, Churchill wanted to wait for the first British bomb test, or rather the test detonation of a nuclear charge (called Hurricane), before considering an organisational change. In November 1952, the month after the test explosion in Australia, a committee proposed that the responsibility for the continued atomic energy work be taken over by a separate authority. In 1953, another committee (the Waverley Report) proposed the way in which this would take place. In August 1954, the United Kingdom Atomic Energy Authority (UKAEA) came into being under a director who was also the Chair of the authority's board and therefore referred to mainly as Chairman of the Board. The first head of the Atomic Energy Authority was Sir (and later on Lord) *Edwin Plowden*, and the board included Cockcroft, Hinton and Penney. The birth of the new authority led to a fair amount of work with reorganising the previous units as well as new recruitment.

The reactors at Windscale, Calder Hall and Chapelcross were indeed designed to also be able to produce nuclear power, but the production of plutonium was considered to be the most important. However, at the same time, concerns began to be raised regarding the energy supply in the United Kingdom. The cold winter of 1946–1947 had been challenging as regards lack of gas and coal, and some industries had been forced to discontinue the activity. The completely dominant energy source in the 1950s was coal, but the continued production of coal in the future was uncertain in the long-term and insufficient in the short term, particularly as coal also needed to be exported for international trade policy reasons. Oil was not yet seen as an easily-available or value-for-money energy source. This basis made the concept of nuclear power tempting.

In 1954, an investigation was set up into the possibilities of nuclear power under *Burke Trend*, who later became Lord Trend. In 1955, the British government published a White Paper (UKGo 1955) on a nuclear energy programme on the basis of Trend's report. Twelve nuclear power stations with a total electrical output of up to 2 000 MW were proposed for the 1955–1965 period. The first stations were to be built as Magnox reactors like the reactors at Calder Hall. Following the shake-up that was brought on by the Suez crisis, in 1957 the original programme was tripled to a total effect of up to 6 000 MW. This first British civil nuclear power programme was completed in 1971 when nine stations produced a total electrical output of 3730 MW. The 1955 White Paper was enthusiastic about the assignment, far more than the previous Trend Report had been. It said:

> This formidable task must be tackled with vigour, imagination and courage. We must not be put off by setbacks or uncertainties . The stakes are high but the final reward will be immeasurable. We must keep ourselves in the forefront of the development of nuclear power so that we can play our proper part in harnessing this new form of energy for the benefit of mankind.

The government had thought that the private industry would build the reactors, but to start with there was no British industry with the capacity to take on the task of designing and building a nuclear power reactor. Nor did the industry have any experience of running a nuclear power plant. This eventually meant that endeavours required by the Atomic Energy Authority (UKAEA) were greater than expected, which strongly increased the burden of work for Hinton's group (Risley), which was now known as 'The Industrial Group'.

This was the situation in which the Windscale accident occurred. In order to prevent the accumulated Wigner energy from becoming too substantial, this energy was regularly released through the process of annealing (the verb 'anneal' which was used in this context actually means to slowly cool glass or metal after heating). To begin with, this was done following an interval corresponding to a relatively low fuel burnup, but the interval had gradually been increased so that in October 1957 it was significantly longer than it had been at the start. This was now the ninth time that Wigner energy was be released from the graphite.

At 11.45 in the morning on Monday 7 October, the largest air-cooling fans were turned off to increase the temperature in the reactor. At the same time, the control rods at the bottom of the reactor were slowly drawn out to make the reactor critical where the greatest accumulation of Wigner energy was expected. It was not thought that the process could be controlled on the basis of output measurements; instead, the temperature reading of 66 thermocouples distributed in the graphite and the uranium was allowed to determine the way in which it could continue. Following interruptions owing to surprising measurement results for some thermocouples, which were later shown to have poor contacts, the reactor finally became critical at 19.25. The temperature was then allowed to rise until the uranium had reached a temperature of 250 °C. It now looked as though Wigner energy was starting to be released, so the reactor was shut down by pushing in the control rods. The responsible reactor physicist then went home at 2, certain that the process could now look after itself.

However, at 9 in the morning of 8 October the reactor operators found that the temperature of the reactor had not risen due to released Wigner energy as expected, but had begun to fall. The reactor was therefore re-started and it was kept critical from 11.00 until 19.25. They now attempted to keep the reactor temperature at 330 °C, but the temperatures were shown to be uncontrollable. The reactor was then shut down once again but, to all appearances, Wigner energy continued to be released impeccably beyond the morning of 9 October. However, the temperature proved to be rising to uncomfortable heights after that, so the fan housing was opened at 22.15 to allow air in to cool the reactor, but this seemed to have no more than a temporary effect. After midnight, temperatures of more than 400 °C were read off before they succeeded in stemming the temperature increase by re-opening the fan housing. At 05.10 on Thursday 10 October, the fan housing was re-opened again and the temperature was now beginning to fall.

However, the measurement instruments in the chimney were now detecting radioactivity, although it so happened that the instruments in reactor no. 2 were also showing radioactivity, to the extent that they considered shutting down that reactor. They therefore mistakenly thought that the instrument in the chimney of reactor no. 1 had been affected by emissions from reactor no. 2. In actual fact there was nothing wrong in that reactor - it was the measurement instruments that were displaying incorrectly. When, on the morning of 9 October, the facility's meteorological station was also able to show radioactive substances in the air, they also made the mistake of thinking that this was due to a leaking fuel element in reactor no. 2.

At 13.30 on the Thursday, the fan housing was re-opened for five minutes, but there was now no doubt that the instruments in the chimney really were reacting to a leak from the reactor they were working with. This was now immediately reported to operations manager *Ron Gausden*, who gave orders for the housing to be opened and the fans which were used when shutting down the reactor to be started. Despite this, the temperature of the uranium rose further than expected to above 420 °C. At 14.30, Gausden realised that the situation was more serious than they had thought and got the main fans going at full speed in the hope of cooling the reactor down.

And still no-one outside the reactor building was aware of the problems. However, the radiation protection manager *Huw Howells* had been informed of the increase in air activity outside the reactor buildings and went to see the attendant facility manager to find out whether the latter knew what could possibly be causing the air contamination. Together they visited the control room for the affected reactor no. 1 and only then did they find out what had happened. They then telephoned the usual facility manager *Gethin Davey*. As it happened, the operations manager of the AEA's industry group (i.e., Risley) *K. B. Ross* was visiting Windscale. Ross and Davey were the ones who now took over the responsibility.

It turned out that the temperature was rising to above 1 200 °C in parts of the reactor and they feared a total core meltdown with substantial emissions of radioactive substances. The thermocouples were not giving an adequate idea of the temperature distribution; there were no thermocouples where the greatest amount of Wigner energy had been released. Parts of the graphite were burning. Lorna Arnold (Arnold, 1992) gives the following description:

> The extent of the fire zone had been determined; 120 channels were involved. Around it a ring of two or three channels had been cleared as a fire-break. Now a heroic attempt was being made to eject the fuel elements from the burning channels. All the

> men on the charge hoist *[a platform that could be raised along one side of the reactor]* were wearing protective clothing and respirators and their faces were soon drenched in perspiration. Using every steel rod they could lay hands on, including scaffolding poles brought over from the Calder Hall construction site, the men worked tirelessly, pushing the fuel cartridges through to the back of the pile. The cartridges were so distorted that it was extremely hard to push them through, and so hot that the steel rods came out red hot and dripping with molten metal. Occasionally red hot graphite boats were pulled out; these were kicked to one side, picked up with a gloved hand and dropped safely over the side of the charge hoist into the well. Twice a cartridge was pulled out and had to be quickly pushed back into the pile. 'Nobody showed any signs of fear', the chief fire officer told me. 'You couldn't have seen a better display from the process workers. They were heroes that night'

At 1 o'clock in the early morning on Friday 11 October, Ross rang Cumberland's chief of police and reported the fire, forewarning him of disaster measures. The preparedness plan that had previously been drawn up in consultation with the police and local authorities was implemented. The police set up a preparedness centre at the facility. Its personnel were ready and prepared at home, fully clothed and ready to intervene. Buses stood waiting to move people from the closest surrounding area if need be.

The graphite fire continued in the reactor. Between 4 and 5 on the Friday morning, an attempt was made to extinguish the fire using carbon dioxide, but this failed. The last option was to drown the reactor in water. This was a risky undertaking. It could lead to the formation of an explosive mixture of carbon monoxide, hydrogen gas and air, but they thought this was a risk they had to take in order to prevent a disaster. At 9 in the morning, water was sprayed into the reactor from four fire hoses. At first the fire seemed to be raging as before, but when the fans that had been operating to make the work tolerable for the men on the charge hoist were turned off an hour later, the fire suddenly died. Only then did Ross send a message to the Chair of the AEA and the manager of Risley about what had happened. At around twelve, the chief of police received a report that the danger had passed and that no evacuation would be necessary.

Christopher Hinton was originally head of the AEA's industrial group (i.e., Risley), but Hinton had left this assignment just before the Windscale accident to become the first head of the electrical industry's Central Electricity Generating Board. He was succeeded at Risley by his colleague of many years' experience, *Leonard Owen*. The message that Ross sent Sir Edwin Plowden and Owen on the morning of Friday 11 October read (according to Arnold, 1992):

> Windscale Pile *[reactor]* No. 1 found to be on fire in middle of lattice at 4.30 pm yesterday during Wigner release. Position been held all night but fire still fierce. Emission has not been very serious and hope continue to hold this. Are now injecting water above fire and are watching results. Do not require help at present.

The message was not only late but also unsuspecting. At Windscale, they clearly did not have enough imagination to visualise the consequences of the accident. Plowden had done just that, however, and ordered Owen to fly to Windscale immediately. He then reported the event to the Prime Minister *Harold Macmillan* (1894–1986), who was the Minister responsible for atomic energy. At the same time, he sent a copy to the Minister of Agriculture in view of the conceivable environmental consequences. However, he wrote that 'there is nothing to indicate any danger to the public'. The message was also distributed at the same time as a press release.

At Windscale there was no longer any need to hold their breath. The vigorous efforts had prevented a disaster and there was no longer any danger at the facility. The task that remained was difficult clear-up work. But the radiation protection manager, Huw Howells, was not satisfied. He wanted to know whether there was any risk to the public outside the facility. At 15.00 on Thursday 10 October, the day before Sir Edwin had received the message about the accident, he had sent the radiation protectionists' only instrument-carrying van south to Seascale. The wind had changed direction from initially having blown in a north-easterly direction to then blowing in a south-easterly direction, i.e., towards Seascale.

At 17.00 they obtained a second van containing measurement equipment; it was sent in north-easterly direction.

It was found soon that neither the gamma radiation from the ground nor the radioactive substances in the air involved any risk that needed action. What did remain was the risk from radioactive substances in contaminated foods. The gamma radiation measurements indicated that milk could be contaminated. Therefore, at the request of Risley, Harwell started a programme on the Friday morning to collect samples of vegetation, grass and some foods, primarily milk. Milk from the Friday afternoon showed levels of iodine-131, between 0.4 and 0.8 microcuries per litre (15 000 to 30 000 becquerels per litre). However, because the milk had been analysed at Harwell, these measurement results did not become available until the Saturday afternoon of 12 October.

No recommended action levels were available for Howells to use to compare the measurement results. ICRP had certainly stated iodine-131 intake limits, but these were for lifelong exposure and were not applicable to an accident. *Dr. Scott Russell*, head of the Institute of Radiation Biology, which was run by the Agricultural Research Council, had recommended action levels for the National Defence in the event of a nuclear weapons attack, but they were also not applicable to all things. However, the action level that Scott Russell had recommended for iodine-131 was 0.3 microcuries per litre and had thus been exceeded. Howells therefore suggested to the facility manager Davey that they ought to prevent the consumption of milk from the immediate surroundings as soon as possible.

This was to be a really drastic measure for which Davey did not want to take sole responsibility. He therefore advised Howells to contact Risley's chief doctor, *Andrew McLean*. The latter had already been telephoned by Windscale's doctor in the early hours of Friday morning and had been told that there had been an accident involving the emission of radioactive substances. McLean had then in turn informed Risley's safety manager *F. R. Farmer* and the head of radiation protection *John Dunster* (1922–2006). These three had gathered in Farmer's home before midnight and had been prepared to move to Windscale immediately. However, when they rang Ross, they heard that the situation was under control and that no help was needed. When they contacted Ross again on the Friday morning, they again heard that everything was under control.

At lunchtime on Friday, McLean rang Davey and said that he, Farmer and Dunster were concerned about possible risks to the public. However, Davey did not think there was any reason for concern. The gamma radiation in the surroundings had turned out to be far below the level that had previously been agreed as the one to lead to action. However, at around 13.00 when McLean and his colleagues saw the press release that had come out and discovered that the reactor had been exposed to a graphite fire, they realised that large quantities of volatile radioactive nuclides such as iodine-131 must have leaked out. They then immediately contacted Harwell, who promised to keep a group of competent radiochemists available over the weekend, and also asked Howells to send a vehicle to take milk samples and send these to Harwell. These were the samples that showed high iodine concentrations.

Harwell's measurement results were sent to both Howells and Risley. Like Howells at Windscale, the small group of McLean, Farmer and Dunster at Risley were wondering what the response should be to measurement values that exceeded 0.4 microcuries per litre. When Howells rang McLean at 16.30 on the Saturday at the request of Davey and thought that a ban should be issued against the use of milk within the immediate surroundings, McLean's response was that he needed time to think about the issue. Howells rang once again at 19.45. McLean's answer then was that 0.1 microcuries per litre was probably a reasonable action level, but that he first wanted to consult Professor J. S. Mitchell in Cambridge and Greg Marley in Harwell.

Howells was now very impatient and thought that the action was more important than the precise action level. He rang MacLean once again at 21.00 on Saturday evening, but he was not told that he could take measures to ban the milk until 22.00. Late on Saturday evening, he reached an agreement with the district police and the local milk centre to stop the delivery of milk from seventeen farmers around Windscale.

Why, asks Lorna Arnold in her book about the Windscale accident (Arnold, 1992), had Windscale not contacted Risley much sooner? Why had even Ross intentionally avoided the experts at Risley for as long as possible? Arnold writes:

> There are several reasons for this situation. One is undoubtedly Windscale's tradition of proud independence and rugged self-help. In its brief history of tribulations and achievements Windscale – a unique and isolated site – had developed a powerful corporate loyalty and pride reminiscent of the local patriotism that Italians call *campanilismo*. Perhaps it is fanciful to think that Windscale staff looked at the tall pile stacks almost as an Italian looks at the bell tower of his parish church; but certainly the establishment's sense of identity and self-reliance was very strong, and it had a tradition of solving its own problems. […]
>
> Another reason that the Windscale staff did not naturally look at once to the Risley health and safety organisation was that the latter […] was not yet well established. It was new, it was small, it was under strength and so far had been entirely occupied with new plants and projects. It had hardly begun to make its mark with operational staff in the Industrial Group, and did not yet loom large in Windscale's collective consciousness. Huw Howells, as health and safety manager, was solely responsible to his own works general manager, not to Risley.

A total of more than 3 000 milk samples were examined and it was possible to chart the extent of the radioactive contamination. The ban on the use of milk containing a higher concentration than 0.1 microcuries per litre led to action against the milk within an area that was approx. 1.5 kilometres wide and that extended 40 kilometres south along the coast: the area covered approximately 500 km^2.

It was possible to detect radioactive iodine at great distances from Windscale, including in Belgium and Holland. The accident aroused great interest all over the world. For the first time, a reactor accident – albeit an accident in a military production reactor rather than a nuclear power plant – had distributed radioactive substances over large areas and led to action being taken against foods. The western world still knew nothing about the big disaster with the waste warehouse in Kyshtym in the Urals.

The Windscale accident obviously led to a number of analyses and reports. The first information on the quantities emitted stated 20 000 curies of iodine-131, 600 curies of caesium-137, 80 curies of strontium-89 and 9 curies of strontium-90. These values have been slightly modified in later studies; for one thing, the emission of caesium-137 may have been twice as high. Subsequent studies have also stated the emission values for additional nuclides, 12 000–16 000 curies of tellurium-132, 80– 160 curies of ruthenium-106 and 80–110 curies of cerium-144. Iodine-131 dominated the emissions partly because of its high volatility and partly because other nuclides, such as strontium-90, were largely captured by the filters in the chimney.

In 1982, the British National Radiological Protection Board (NRPB) made a new estimate of the doses of radiation from the Windscale accident (Crick, 1982). The total collective effective dose was estimated to be 1 200 man-sieverts, 130 of which within a 50-kilometre radius in Cumbria. The total collective dose for the thyroid gland was estimated at 26 000 man-sieverts, 3 200 of which were received in the immediate surroundings. It has not been possible to demonstrate any cases of cancer as a consequence of the accident, and nor was this expected. The theoretically calculated few dozen extra cases of cancer per year cannot be detected against the background of natural variations in the total incidence of cancer.

The action levels that were applied to iodine-131 in milk and the action levels as recommended by the British Medical Research Council following the accident have been very important in the selection of the action levels that were recommended by other authorities, including the Swedish ones, in the 1960s (see Chapter 15).

Depictions of disasters in the 1950s

The mid-1950s had been a time of considerable worry with regard to reactor accidents and, primarily, nuclear war. This worry was reflected in the novel *On the Beach*, published by the popular British author *Nevil Shute* (1899–1960) in 1957. In his novel, Shute, who had moved to Australia in 1950, had the whole of the world's population obliterated by a nuclear war (with 'cobalt bombs' to make the radioactive fallout fatal in the long term). The reader gets to follow the longest survivors' heroic denial of the unavoidable. The book, although very well-written and gripping, is completely unrealistic with regard to the factual content. The negative consequence of this was that, for good reasons, many experts were able to show that the fear of nuclear war was exaggerated. In reality, the world's population cannot be destroyed by nuclear weapons, although this fact can unfortunately make it tempting to say 'Oh well, it'll *only* be a few hundred million who'll lose their lives!'

Other disasters were depicted by *Robert Jungk* (1913–1994) in his books about the Manhattan Project and about Hiroshima following the atomic bomb (*Strahlen aus der Asche* or, to give it its English title, 'Children of the Ashes'), and by nuclear physicist *Ralph Lapp* in his book about the consequences of the big Bikini explosion in 1954 for the men on a Japanese fishing boat ('The Voyage of the Lucky Dragon'). Of particular interest is the exchange of views published in book form in 1958 between the American Nobel Prize winner for Chemistry Linus Pauling and 'the father of the hydrogen bomb' Edward Teller. Pauling's book was called *No More War*, while Teller, along with *Albert Latter*, wrote the book called *Our Nuclear Future*. Both were based on the objective that nuclear war must be avoided at all costs. What Pauling and Teller did disagree on was the justification of nuclear weapons testing. Pauling thought that the tests increased rather than reduced the risk of war. Teller thought that the balance of power between the USA and the Soviet Union could be maintained only through further development of nuclear weapons. Both made correct statements regarding the potential biological consequences of nuclear weapons testing, but Pauling said that these harmful consequences 'could not be denied' while Teller said that the same consequences were hypothetical and that it 'was not impossible that the radioactivity lengthened rather than shortened' people's lifespan.

In 1957, Ralph Lapp and another scientist, *Jack Schubert*, wrote a book entitled *Radiation – what it is and how it affects you*. It criticised the radiation protection legislation at the time and the radiation protection supervision in the USA, and gave many examples of inexperienced and irresponsible handling of sources of radiation. Although the book was written to arouse debate, it was more reliable and level-headed than might have been anticipated. Another popular, level-headed scientific book was published in 1959 by geneticists *Bruce Wallace* and *Theodosius Dobzhansky* (1900–1975) with the title *Radiation, Genes and Man*. It was published in Swedish in 1961.

Einstein and Schweitzer

In the 1970s nuclear power debate, references were often made to statements by *Albert Einstein* (1879–1955) and Albert Schweitzer. In both cases, the statements concerned the fact that both of these men were worried about the possibility of a nuclear war. Einstein was usually quoted as having said 'Not even scientists completely understand atomic energy, for each man's knowledge is incomplete', but the quote was rarely continued with the following sentences: 'Few men have ever seen the bomb. But all men if told a few facts can understand that this bomb and the danger of war is a very real thing, and not something far away'.

Einstein said this as early as 1946, long before any peaceful use of nuclear energy had become reality, but Schweitzer's statement was spoken over Norwegian radio in 1957. *Albert Schweitzer* (1875–1965) was an all-rounder: theologist, philosopher, musicologist, missionary and medical doctor. From 1913 he lived in French Equatorial Africa but, being a German, became a French prisoner of war during the First World War. After the war, he continued to work in Lambaréné in Gabon where he had set up a hospital for which he paid using his own finances. He was awarded the Nobel Peace Prize in 1952 and earned a great reputation as a humanist underpinned by great respect for life.

He warned about the atomic bomb in 1957 and said, 'The end of further experiments with atom bombs would be like the early sunrays of hope which suffering humanity is longing for'. Schweitzer's statement ended up being misquoted as though he had expressed an opinion on peaceful nuclear energy.

The Nuclear Accidents Act of 1960

3 June 1960 saw the adoption of the Swedish law regarding protection measures in the event of accidents in nuclear power plants, etc. (SFS 1960:331, 'The Nuclear Accidents Act'). The Act placed the immediate responsibility for protection efforts in the event of a reactor disaster with the relevant County Administrative Board of the county. An unusual authorisation for the County Administrative Board was the right in accordance with Section 6 of the Act to 'prescribe that anyone who has turned

eighteen but not sixty-five shall, at the request of a police officer allocated by the County Administrative Board, be liable to provide assistance to the extent that his physical strength and state of health permits'.

The County Administrative Board was to establish a preparedness plan. In a royal letter of 30 June 1960, in view of the Act, the Radiation Protection Authority (the Radiation Protection Committee as it was then) was given the task of drawing up instructions to guide the County Administrative Boards when assessing an accident. The Radiation Protection Committee issued such instructions on 21 February 1962. A special *Expert Commission for Advice in the event of Nuclear Accidents* (the KRA) was set up on 30 June 1960 with Rolf Sievert as Chair. Its task in connection with the big Soviet nuclear explosions at Novaya Zemlya is described in more detail in Chapter 16. The KRA was active until 1973 when it was superseded by the *Preparedness Committee against Nuclear Accidents* (the BNA), which ended up being an expert advisory committee within the Radiation Protection Institute's organisation.

The OEEC's Convention on Economic Liability, 1960

On 29 July 1960, the Organisation for European Economic Cooperation (OEEC) adopted a Convention on Third Party Liability in the event of a nuclear accident. The Convention required that those running a nuclear facility be financially insured up to a given amount.

The Lockport accident in 1960

In 1960, a serious radiation accident occurred which showed that it is not only radioactive substances that are dangerous. Nine engineers at a radar station in Lockport, New York, were exposed to x rays from a klystron*. Two of them were seriously injured.

The SL-1 accident in Idaho in 1961

On 4 January 1961, three people were killed in an accident in a small reactor with a 3-MW thermal output at the Nuclear Reactor Testing Station in Idaho. The reactor was a two-year-old experimental prototype for a mobile reactor which was intended to be able to provide military divisions in remote areas with heat and electricity. It was called 'SL-1'. The reactor had been shut down for twelve days when the accident happened.

The control rods were pushed into the reactor core. Three men worked on the reactor, probably intending to attach the control rods to some rod moving device. In order for this to be possible, the rods had to be lifted slightly. By all accounts, the rods jammed in such a way that the men had to make a huge effort to dislodge them. Suddenly, one rod surprisingly became unstuck and pulling with full force meant that it was moved far enough for a fierce nuclear reaction to suddenly start a powerful energy development. A vapour explosion broke the reactor and radioactive substances were hurled out into the building in which the reactor was being tested. The level of radiation rose tremendously and made it impossible to rapidly evacuate more than one of the three men. That man had received a high enough dose that he died within an hour in any case. It took more than five days for them to succeed in getting out the remaining two who had obviously received enormous doses of radiation but who may have already died as a consequence of the explosion.

The bodies were heavily contaminated with radioactive substances and the coffins were, by way of precaution, shielded with lead when they were buried. This subsequently led to a widespread rumour that a reactor accident in the USA had led to personnel being so heavily contaminated that they had to be kept locked in lead chambers.

The Fermi accident of 1966

In 1953, the experimental EBR-1 breeder reactor at the testing station in Idaho had demonstrated the possibility of the breeder principle (see Chapter 10) and even generated a certain amount of electricity. However, the first large breeder reactor to be included in an actual nuclear power plant was built by Lake

* Like the magnetron, a klystron is a microwave tube for the generation and amplification of high- power microwave radiation. Klystrons are used in large radars.

Erie in Michigan, south of Detroit, and was commissioned in 1963. The power plant was named after the Italian physicist *Enrico Fermi*, who was the first person to build a functioning nuclear reactor, the one that was started in Chicago in 1942 during the Manhattan Project.

The reactor in Michigan was a fast breeder reactor, i.e., it had no moderator and was perpetuated by neutrons which had not been slowed down. The coolant was liquid sodium, partly because a fast reactor has such a small volume that the cooling thereof becomes a problem, and partly because you do not want to slow down the neutrons by using water as a coolant.

The cooling is particularly important in a breeder reactor because there is a greater risk of the reactor core rapidly melting if the cooling stops. A lump of melted metal could bore through the reactor tank and spread radioactive substances, an accident which, with their macabre humour, the engineers referred to as 'the China syndrome', considering the drastic (and naturally completely unscientific) exaggeration that the lump would be able to continue straight through the Earth and end up in China.

In order to prevent the China syndrome, they decided to cover the bottom of the reactor tank with a zirconium plate (zirconium has a high melting point). However, at the bottom of the tank there was a cone that was 30 cm tall and which was intended to direct the coolant flow up towards the reactor core. When the reactor came to be assembled, the contractor noticed that this cone was not covered with zirconium.

Six triangular zirconium plates were therefore hastily made to cover the cone. The supervisory authority never found out about this.

The six zirconium plates were exposed to forces from the coolant flow, and in October 1966 one of the plates became loose and was driven by the liquid sodium towards the coolant outflow where the plate became stuck. This prevented the coolant from circulating.

On 5 October 1966, the personnel noticed that the neutron flow was suffering irregular variations and that the temperature was higher than normal. The instruments that recorded the level of radiation in the reactor hall signalled an increase and the staff realised that an accident had occurred. The reactor was therefore shut down.

They were now worried that part of the reactor core had melted. If they were not careful, some of the fuel might perhaps be shaken together to form a critical mass which could explode. With the consequence descriptions from WASH-70 (1957) in mind, they were afraid of the possibility that such an accident could kill hundreds of people. When, taking extreme care, they managed a closer inspection of the reactor, they found that the mitigating systems had functioned well. Only two fuel cartridges had been destroyed and a few nearby cartridges had been damaged. There was no damage to the surroundings. However, the caution had led to a delay of a year before the cause of the accident was discovered.

Accidents concerning fine structure analysis and gamma radiography

Less well-known are the numerous deaths which have occurred in different parts of the world due to the ignorant handling of radioactive preparations which have gone astray or have been handled with no awareness of the risks. We in Sweden have been spared such serious accidents, but we must not underestimate the risk of injuries from sources of radiation within healthcare and industry. Special x-ray devices for fine structure analysis used to be potentially dangerous because, due to lack of knowledge or breach of the safety provisions, people could expose their fingers to the primary x-ray beam with severe local injuries as a consequence, tantamount to being severely injured if you place your fingers close to a rotating saw blade. A few dozen such injuries have occurred in Sweden.

For gamma radiography, encapsulated sources of radiation containing radioactive preparations are used. In 1966 there were 37 such facilities using cobalt-60, 13 using iridium-192 and 2 using caesium-137. Such sources of radiation can go off course through carelessness or theft and thereby end up in the hands (literally) of people who do not know that the capsules are dangerous but think that they look interesting or valuable. Near-accidents have occurred in Sweden, but no *actual* accidents. However, a frightening number of accidental deaths have occurred abroad. The most sensational accident, bordering on a disaster, happened in Mexico in 1962 and warrants a detailed description.

The Mexico accident of 1962

The accident was caused by an apparently trifling radioactive source which was intended for examination of metal objects with highly penetrating gamma radiation. The source of radiation consisted of cobalt-60 with an activity of 5 curies (approx. two hundred billion becquerels). It was contained in a small capsule that was normally stored in a protective lead container. When the radiation source came to be used, it was lifted up and placed on one side of the material that was to be examined and a photographic film was placed on the other side. Following a period of exposure the film blackened, giving a silhouette as a basis for drawing conclusions regarding inadequacies in the material such as porosity, cracks or leaking joints.

In March 1962, the capsule containing the cobalt preparation had been used for examinations close to a house occupied by the Espíndola family who had recently moved in. The family consisted of the 30 year-old *Jesús Espíndola*, his 27 year-old wife *Mercedes*, the 10 year-old son *Enrique* and the 3 year-old daughter *María Eugenia*, plus Jesús' 57 year-old mother, Señora *Augustina Ibarra*.

In some unclarified manner, the capsule had been removed from the protective container, which should have been locked or at least have been supervised. All we know is that sometime after the 21 March 1962, Enrique got hold of the capsule and pushed it into his left trouser pocket. He then carried it in his pocket until 1 April.

The dose rate from 5 curies of cobalt-60 is approx. 60 milligray per hour at a distance of one metre. In Enrique's skin, a few centimetres away from the capsule in his trouser pocket, the dose rate must have been more than 10 gray per hour. One hour's irradiation would therefore have already been enough to cause tissue necrosis. The investigators later guesstimated that Enrique had had the capsule in his pocket for 8–9 days and that he had been wearing the trousers containing the capsule for at least half that time.

Enrique's mother, Señora Mercedes, later said that Enrique had previous complained of feeling weak and nauseated while the skin in his groin area had turned red and started to become covered with blisters which soon turned into an infected sore. On 1 April, the boy had become so poorly that he had to stay in bed. Mercedes then discovered the capsule in his trouser pocket but did not understand the connection, and because she had other things to think about, she just placed the capsule in a big kitchen utensils cupboard between the cooker and the front door of the house.

On 7 April, Enrique's mother took him to the medical centre, still not having made a connection between the damage and the mysterious capsule. The personnel inspected the permanent sore on his groin and concluded that it was a question of inflammation in a sore that had arisen through mechanical injury. They prescribed salves and antibiotics.

Enrique deteriorated in spite of the treatment and he was taken to a hospital on 17 April. The doctors there found that the original sore had expanded. It now covered a couple of decimetres and stretched from the groin out over the thigh. The boy was severely affected by fever and reduced stamina because the rest of his body had also received high doses of radiation.

Enrique had probably been carrying the capsule in his pocket for 100 hours. The capsule had been in his trousers for just as long a time only one and a half metres from the bed while he was asleep or ill. After 1 April, when the capsule had been in the kitchen cupboard, the distance to the boy's bed had been approximately 2 metres, i.e., the bed had been in the kitchen. His paternal grandmother, Señora Augustina, slept in the same bed.

Enrique died at the hospital on 29 April without anyone suspecting the real cause of his injuries. In the meantime, the mortal capsule remained in the kitchen cupboard at the home of the Espíndola family. In the bed in the kitchen where Señora Augustina still slept at nights, the dose rate was 15 milligray per hour and in the small room inside which the rest of the family spent the night it was 4 milligray per hour. But by the cooker in the kitchen it was higher, approx. 60 milligray per hour.

Since they remained in the kitchen during the daytime and because Mercedes spent most of her time by the cooker, she and the mother-in-law received high doses of radiation. Mercedes had a daily dose of up to 300 milligray and Augustina maybe half as much. Enrique had probably received daily doses of up to 3–4 gray. It would thus take at least ten times as long for Mercedes to receive the same total radiation dose as Enrique, but the recovery capacity of tissue meant that a higher radiation dose would also be required for the same harmful effect to take place.

Jesús Espíndola and his daughter María Eugenia slept in the interior room, but María spent a lot of time in the kitchen and may have received just as high a dose of radiation as her paternal grandmother. Jesús received the lowest dose in the family; his daily dose has been estimated as 50 milligray.

On 2 May, María Eugenia showed symptoms of urticaria for which she was given antihistamines. This did not help much, however, and the girl suffered from urticaria throughout May and June although this need not have been connected with the fact that she was exposed to radiation. However, an airway infection began on 6 July and the girl's general health deteriorated.

On 15 July, Señora Mercedes went to the hospital because she had an abnormal swelling and bleeding under her skin after having knocked her left leg. By that time she had probably received a radiation dose of more than 20 gray. Mercedes, who was now in her seventh month of pregnancy, said that she had had been bleeding for three weeks and that she was getting bruises for no obvious reason. She also had chills and fever and had had blood in her urine for the past two days.

Two days later, Mercedes had severe internal bleeding and was sent to the national hospital's gynaecological department as an emergency case. Blood tests showed that the number of thrombocytes (blood platelets) had fallen to 12 000 per mm^3 compared with the normal of more than 200 000 per mm^3 – Mercedes was suffering from thrombocytopaenia. Therefore, her blood had lost the capacity to coagulate, which increased the risk of bleeding and led to small, spontaneous haemorrhages beneath the skin (purpura).

Despite blood transfusions, Señora Mercedes died at the hospital on 19 July. The post-mortem showed that the bone marrow was damaged and the cause of death was said to be 'acute anaemia'. The foetus could not be saved. The pathologist noted that its spleen had been destroyed. On 22 July, the cobalt capsule was retrieved by its owner, an engineer who had succeeded in tracing the missing cobalt to the Espíndola family's kitchen but who apparently had not dared to disclose anything about how dangerous it was.

At the end of July, little María Eugenia also began to show signs of thrombocytopaenia. She was sent to the hospital on 1 August. The doctors there discussed the possible diagnoses without thinking of radiation damage – but María had received a radiation dose of at least 20 gray by then and it was scarcely within the human capacity to save her life.

Immediately thereafter, María's paternal grandmother, Señora Augustina, also became very ill. She went to the hospital on 10 August showing signs of anaemia. She too had then probably received a radiation dose of 20 gray. She was treated as an outpatient with blood transfusions but her condition deteriorated after a week. She was then showing clear signs of thrombocytopaenia, bleeding beneath her skin as well as from her gums and nose. Only then did the doctors begin to suspect a connection between the Espíndola family's illnesses, which sparked off the initial suspicion about the occurrence of radiation injuries.

On 15 August Jesús, the father of the family, went to the hospital where little María was still being cared for and Augustina was complaining about her symptoms. Blood samples were taken from him, but the number of thrombocytes was normal.

On 18 August, almost five months after Enrique had taken the cobalt capsule home, the causal connection had finally been clarified by contacting the Mexican Atomic Energy Commission's radiation protection experts. María Eugenia died on the same day, just after the appalled experts had reached the hospital.

On 20 August, Señora Augustina was admitted to the national hospital's cancer clinic. Her thrombocytes continued to fall in number and were down to 5 000 per mm^3 in September. Her condition deteriorated in spite of blood transfusions. They considered doing a bone marrow transplant but this did not take place. Señora Augusta died on the morning of 15 October.

When the cause of the four deaths had become clear, the as yet apparently unharmed Señor Jesús Espíndola Ibarra was examined and his condition was monitored by taking regular samples. They found that his thrombocyte level had fallen to 70 000 per mm^3 in mid-September, but that it had then increased to normal levels. According to the official report, Señor Espíndola had been exposed to 1 200 röntgen, which may have corresponded to a whole body dose of up to 8 gray. The report (Cons, 1962), which was written in November 1962, gave him an unsafe forecast but did not preclude the possibility of him recovering (which he did).

One interesting observation was that all of those injured appeared to have received a greater amount of pigmentation which made their nails black. Another important observation was that the symptoms of injuries to the bone marrow were the completely dominant ones, and that none of those injured showed any typical symptoms of injuries to the gastrointestinal tract as well, which are life-threatening in acute cases of whole-body irradiation. Had it been a question of an accident where the whole dose had been received in a short time, this would have indicated that the dose was no greater than 5 gray, but for the affected members of the Espíndola family, the dose exceeded 20 gray over a period of 100 days. This indicated that the dose which occurred over more than three months had only 1/4 of the effectiveness of a single dose, which tallies with the experiences of radiation therapy where high doses have been spread out over a longer period of time.

The Venus accident of 1965

The Venus accident in 1965 is just one of a number of criticality accidents that have occurred in various places over the years, but its name seems to have stirred the imagination. Venus was the name of a small experimental reactor of just 0.5 kW at Mol in Belgium. It was commissioned in 1963. In 1965, a criticality accident occurred which led to the irradiation of a physicist who received a dose of approx. 40 gray in his left foot. He was sent to the Curie hospital in Paris for treatment and his foot was amputated. The accident is described in the 1982 UNSCEAR report (p. 414).

The Palomares accident of 1966

In January 1966, an American bomber containing four hydrogen bombs crashed close to the Spanish village of Palomares. Luckily, none of the bombs detonated, but an ‘ordinary’ explosion in one of the bombs’ release mechanisms spread plutonium over a large area. Considering the embarrassing political consequences, the event triggered extensive efforts on the part of the American authorities. All vegetation was collected from an area of 2.4 km^2 and the ground was ploughed to a depth of 25 cm to prevent the plutonium from spreading further. Both vegetation and soil were removed from 2.2 hectares of the most contaminated area and packaged in barrels for transportation to the Savannah River for burial. One of the four bombs had fallen into the sea and had to be recovered from the bottom of the Mediterranean Sea.

The likelihood of extraordinary events

We sometimes ask ourselves questions when confronted with an unusual event, and in doing so we often have a tendency to suspect that something unfavourable is behind it. However, it is about time we realised that, in spite of everything, unusual events are actually pretty ordinary because there are so many unusual things which *can* happen that there are occasions when one *does* actually happen. Only when we have described a particularly unusual event in advance do we have reason to become suspicious if this particular event then actually *occurs*.

If we throw a die one hundred times and count how many times the six comes up, we can expect a number of around 100/6 because the total result has to be divided by six possible outcomes for each throw. The most likely result is therefore seventeen sixes, but if we repeat the experiment time after time, we will find that it does not turn out to be seventeen sixes each time, but a variation, i.e., the result is spread. The greater the deviation from the most likely value (i.e., seventeen), the fewer the number of times we obtain that particular result.

If we generally take notice of phenomena that can be measured, such as the number of accidents in a week or the height of twelve year-old boys, and draw a diagram of the result, the diagram will tend to show a dome-shaped curve, known as the Gaussian curve or the *normal distribution* of the result. There is always a distribution; not all boys are exactly the same height and the number of accidents varies randomly from week to week.

When it comes to the number of events in a large group where there is a very small likelihood of the event for each individual, the distribution curve takes on a special form (the Poisson distribution) which can be described by the following mathematical formula:

$$S(n) = e^{-m}\, m^{n}\ /n!$$

Here, S(n) means the likelihood of exactly n events occurring (e.g., the number of traffic accidents or the number of people who have had a certain type of cancer in a given period of time) and 'm' is the mean value of the number you would find if you could repeat the observation many times for periods of equal length. The letter 'e' designates the base for the natural logarithm (e = 2.71828…). The value of e^{-m} can be found in tables or looked up in many pocket calculators. The number 'n!' designates the 'factorial n' and is the product of all whole numbers, including 'n',

i.e., 1 x 2 x 3 x 4 x 5 x 6 x … x n. We find, for example, that if the mean value, the one that is the most probable here, is m = 7, the likelihood of the outcome instead being 15 can be calculated as S(15) = 0.33 %.

A regularly-recurring situation is that we have observed a number, 'a', of events, such as the number of cases of cancer in a specific year or the number of registrations in an hour in a measurement instrument. In such a case, our question may then be 'what is the likelihood of the number next time (next year or when the next measurement is taken) being 'b' instead, subject to no change of conditions'. We cannot immediately calculate S(b) using the expression for the Poisson distribution because we do not know the value of m. It may then be more obvious to say that the most probable value of m is 'm = a' because this is what we have observed so far. We can then directly calculate the likelihood of 'b' events occurring next time as being

$$S(b|a) = e^{-a}\, a^{b} / b!$$

Here, the vertical line before a means 'if the value of "a" is stated'. However, the mean value (m) is not definitely equal to the result (a) of the first observation. The outcome may be 7 events, even if it is not the case that m = 7. If, for example, m = 10, the likelihood of 7 events is 9 %, which is not all that unlikely. We ought therefore to work on the basis that 'm' can have other values than 'a'.

If we initially know nothing at all about what 'm' may be, we can do nothing other than assume that all of the values for 'm' are equally probable. We call this the *a priori distribution* of 'm'. This is what two physicists, *James Rainwater* (1917–1976, Nobel Prize winner for Physics in 1975 together with *Aage Bohr* [1922–2009] and *Ben Mottelson* [1926–]) and *Chien-shiung Wu* (1912–1997) at Columbia University in New York assumed in an article in *Nucleonics* in 1947. They were then able to show that, after having noted the outcome 'a', you had to consider that the most likely values for 'm' ought to be close to 'a' and that it was not particularly likely that 'm' would have values that were very remote from 'a'. They were able to provide a formula for what they considered to be the probability of m, having observed 'a'; let us call it w(m|a), i.e.,

$$w(m|a) = e^{-m}\, m^{a}\ /a!$$

This expression has the same form as the Poisson distribution, but here we are not seeking the probability of 'a'; we are looking for 'm'. Since in using this formula you accept that values for 'm' may be different from 'm = a', in order to calculate the probability S(b|a) of observing the outcome 'b' next time, you must take into account the possibility of these other values. This is done using *Bayes' theorem*, which describes the way in which a probability distribution changes when you find out that a new event has occurred. It is named after the British minister and mathematician *Thomas Bayes* (1702–1761). It would place too great a burden on the presentation here to give an account of the way in which *Bayes' theorem* is applied; it is enough to state the result if you make the same assumption as Rainwater and Wu:

$$S(b|a) = \tfrac{1}{2}\, (\tfrac{1}{2})^{(a+b)}\, (a+b)!/(a!b!)$$

With this expression, the likelihood of observing 15 events having first observed 7 is no longer 0.33 %, but 2.0 % because we have now accepted the possibility that m may be greater than a.

However, the assumption made by Rainwater and Wu, i.e., that the mean value (m) *a priori* may have any value whatsoever, is not realistic. It implies that, before we had observed the outcome 'a', we had no information at all of which *a priori* ‘m’ values could have been possible. However, we always know something about ‘m’. Previous experience and pure common sense tell us that ‘m’ cannot be as large as you like. Our assumption about which of the ‘m’ values are possible, as well as how likely we consider them to be, is always subjective. There is therefore no objective ‘true’, value of the probability S(b|a) to observe the value ‘b’ after first having observed the value ‘a’. The probability of the values of ‘b’ deviating greatly from ‘a’ can differ by several powers of ten, depending on our assumption of the *a priori* distribution of the possible values of the mean value ‘m’.

I found that this was the case and that Rainwater and Wu had made an arbitrary assumption in 1963 at the same time as another radiophysicist, the shrewd *Jan Cederlund*, who was at the Radiotherapy Department at the Central General Hospital in Borås at the time. Purely by coincidence we discovered that we were both chewing over the same problem. We then agreed to write a joint paper about the problem, which we did in *Physics in Medicine and Biology* in 1964. We had learned that probabilities always depend on the assumptions that have been made and that, in this connection, there are no objective values for a probability - a very important conclusion.

14. NATIONALLY AND INTERNATIONALLY

The mid-1950s were a very eventful period. As we have already seen, Stalin's death in 1953 led to something of an improvement in the relations between the western powers and the Soviet Union, which made it possible to realise Eisenhower's proposal for an Atoms for Peace programme and the breakthrough of civil nuclear power. However, in 1956 there was political unrest on a worldwide scale, partly because of the Soviet Union's intervention against the uprising in Hungary and partly due to the simultaneous Suez crisis. The latter clearly showed that the time of the old colonial power was over and that the only remaining superpowers were the USA and the Soviet Union, between which the cold war was intensifying. The concern about a nuclear war began to increase in line with the superpowers testing their nuclear weapons arsenals. I will use this chapter to provide an overview of the development of the way in which radiation protection was organised in a number of countries.

As in Sweden, the *Finnish* radiation protection activity was initially linked to an Institute of Radiophysics whose principal task was to concentrate on medical physics at a radiotherapy clinic – in Finland at the radiotherapy clinic at the General Hospital in Helsinki. This leading hospital had premises in several places in the city. The radiotherapy clinic was on Unionsgatan in central Helsinki, the street to the left of the cathedral which runs northwards from Senatstorget. In the 1950s, when the radiation protection was organised, the head of the clinic was Sakari Mustakallio, Finland's first Professor of Medical Radiology. *Kauno Salimäki* (1905–1971) had worked at the clinic's radiophysics department as a physicist since 1953 and as chief physicist from 1958.

In June 1952, the Nordic Society of Medical Radiology held its 18th Congress in Helsinki with Mustakallio as President. At the time, a committee set up by the government was working to examine the conditions for a Finnish radiation protection law. The Committee submitted its report in 1954 with no result. However, regulations concerning x-ray devices had existed in connection with the Law on Electrical Safety since 1928. Based on this Law, more detailed regulations for facilities producing x rays had been issued on 28 February 1945 in 'The Ministry of Trade and Industry's decision regarding regulations for x-ray facilities'.

In 1956, a new committee for a radiation protection law was set up. By then, the situation had been tangibly changed through the 1955 Atoms for Peace Conference in Geneva and the possibility of civil nuclear power earlier than most people had expected. The new report led to Finland having its own proper radiation protection law in 1957. On 10 October of this year, an Advisory Committee on Radiation Protection was set up with the head of the Finnish Medical Board, *Niilo Pesonen*, as Chair. This Committee also included Professor Mustakallio, as well as the Professor at Finland's University of Technology *Erkki Laurila* (1913–1998). The Committee proposed that the Radiophysics Department at the General Hospital's radiotherapy clinic function as a radiation protection institute (called the Institute of Radiation Physics), like in Sweden where Sievert's Institute of Radiophysics took charge of the Swedish Medical Board's radiation protection assignments, despite its original task having been that of the Medical Physics Department.

In 1958, the Finnish Radiation Protection Institute with Salimäki as head was formally turned into an authority, coming under the Medical Board. In the same year, the head of the Medical Board (the Ministry of the Interior) made a decision regarding the supervision of x-ray facilities, radionuclide laboratories, etc., including dose limits. The Institute was then permitted to employ four radiation protection inspectors as well as a part-time haematologist. One of the four inspectors was *Antti Vuorinen* (1932–2011), who would later become head of the Institute. However, the first regulation on the Institute of Radiation Physics did not appear until 1961. Those who went on to be employed there primarily include *Olli Castrén* (1933–) in 1961 who, like Vuorinen, came from Professor Laurila's institute, *Olli*

Paakkola (1930–2019) in 1962 and *Aulis Isola* (1935-1989) in 1963, who would go on to succeed Salimäki as head, *Anneli Salo* (1932–), *Matti Suomela* (born 1936) and *Ahti Toivola*.

Salimäki's institute reached early and informal cooperation agreements with university institutions and other authorities such as the Defence. In 1959, an environmental analysis of the radioactive fallout from the nuclear weapons testing began. This task became increasingly important when the Soviet Union carried out very powerful nuclear weapons testing at Novaya Zemlya in 1961 and 1962. The particular university institutions which cooperated with the Institute of Radiation Physics include the Institution of Radiochemistry at the University of Helsinki under the leadership of *Jorma K. Miettinen* (1921–2017). Miettinen was a student of the influential Nobel Prize winner *Artturi Virtanen* (1895–1973), who was President of the Academy of Finland from 1948–1963. Miettinen's institute, called the Institute of Radiochemistry, was formalised in 1962 and Miettinen took on the new Professorship of Radiochemistry in 1964, the same year in which a new building for the Institute was finished. Several of Salimäki's employees, such as Olli Paakkola and Anneli Salo who devoted themselves to radioecological research and control measurements in the 1960s, had been recruited from Miettinen's institute.

Another early cooperation partner was the Institute of Marine Research under the leadership of *Ilmo Hela*, who became the first head of IAEA's then newly-established marine research laboratory in Monaco in 1961.

The dominant person in the Nordic medical radiology cooperation in the 1950s was Professor Mustakallio. However, when the Nordic Society of Medical Radiology held its 22nd Congress in Åbo in 1958, Carl Wegelius was President but Mustakallio held an acclaimed Forssell lecture. The next time the Society met in Finland was in 1964 when the 26th Congress was held in Helsinki. This time it was *Carl-Erik Unnérus* (formerly Johansson) who was President, and he also held the Forssell lecture, which was about 'Radiation protection in clinical work'. Professor Mustakallio spoke about the radiological treatment of lung cancer, which was the most common form of cancer among men in Finland at the time with 1300 cases a year, a frequency that was five times higher than in any other Nordic country. The Helsinki Conference is counted as one of the Nordic Society's 'big' congresses with a rich scientific programme.

Among the Finnish medical physicists it is worth mentioning *Mårten Brenner*, who was chief physicist at the radiotherapy clinic at the University of Helsinki's Central Hospital (formerly the General Hospital) in 1963–1966. Brenner became Professor at *Åbo Akademi University* in the 1970s.

The Finnish atomic energy programme originated from an initiative on the part of the Academy of Finland in 1954 when Professor Virtanen as President brought to the government's attention the benefits that peaceful atomic energy could offer. The Academy proposed that a committee should examine these options. The Academy's letter led the government to set up a committee named the Energy Committee. Its activity was heavily influenced by the international events in 1955, primarily the Atoms for Peace Conference in Geneva. The Committee was also influenced by the development within the USA and realised the difficult problems that had to be overcome before a nuclear power programme could become reality. It therefore recommended that they prioritise the Conventional methods for producing electrical energy while simultaneously taking their time to purposefully prepare the future introduction of nuclear power. In this connection, they ought primarily to concentrate on training within the new areas that would be relevant.

As proposed by the Energy Committee, an Atomic Energy Commission was set up under the Ministry of Trade and Industry with five members and with Professor Erkki Laurila as Chair. The national budget had assigned significant amounts for 'research work for and monitoring the peaceful use of atomic energy'. This would enable a good thirty young physicists, chemists and engineers a year to increase their competence.

In 1962 it was possible to commission a research reactor. It was a TRIGA reactor (TRIGA Mark II), which is an American 'pool-type reactor' where the reactor core sits at the bottom of a large water tank. It was named 'FiR1' and had a nominal output of 100 kW. Such a reactor has several benefits from the research and training point of view. It can create powerful neutron pulses and also serves to produce radioactive isotopes. The reactor was located next to Finland's University of Technology in Otaniemi (Otnäs) outside Helsinki.

At this time, Finland had still invested only very insignificant amounts in atomic energy compared with Denmark and Sweden. However, in 1963, the Atomic Energy Commission proposed that The Ministry of Trade and Industry establish an analysis of the organisational measures that were necessary to build nuclear power plants. The analysis came to fruition in 1964. Finnish industry also began studying the viability in the same year. It falls outside the time frame of this story to go into further detail about the two nuclear power stations that were subsequently commissioned: two reactors at Lovisa (1977 and 1981) and two reactors on the island of Olkiluoto outside Eurajoki (Euraåminne) on the west coast of Finland (1979 and 1982).

In 1956, Swedish Parliament had decided on ‘the Swedish line’ for the peaceful nuclear energy programme and established the Atomic Energy Act. Denmark also had plans for a nuclear energy programme. The Institute that took the initiative was the Danish equivalent of the Royal Swedish Academy of Engineering Sciences, the Danish Academy of Technical Sciences (ATV). In November 1953, the ATV had set up a committee ‘to follow the international work with the development of the industrial use of atomic energy and, on that basis, make proposals that may be appropriate to a possible Danish work programme’ (Niel, 1998). Niels Bohr and Professor of Physics *J. C. Jacobsen* at Bohr’s institute were asked to be members of the Committee. Jacobsen had earlier that same year, in connection with the international Congress of Radiology and ICRP’s meeting in Copenhagen, been elected to ICRP’s Main Commission where he remained until 1962.

The Danish Medical Board (*The Board of Health*) was mainly responsible for radiation protection monitoring, and relied on advice from Professor of Physics *Hans Marius Hansen* who was Vice-Chancellor of the University of Copenhagen. At the International Congress of Radiology in Copenhagen in 1953, Hansen had been Chair of the Executive Committee’s Physics Committee and Honorary Chair of ICRU. However, Hansen was an extremely busy man and delegated the practical inputs to radiophysicist Paul Rønne-Nielsen, who was Assistant Professor at the University’s biophysics laboratory. Rolf Sievert had met Rønne-Nielsen at the Congress and had concluded that the latter was the person who was immediately responsible for radiation protection in Denmark. This worried Rønne-Nielsen, who was afraid that Sievert overestimated his importance. On 18 April 1955 he wrote the following in a letter to Sievert as a response to a telephone enquiry about a meeting in Stockholm:

> Dear Professor Sievert.
>
> Thank you for your call; it was lovely to speak to you. However, I must ask you to excuse me, for this week at least and in this particular context (see the next page though!). I did not realise the basis for your call when we were talking on the phone, and I am afraid you completely misunderstand and overestimate my position regarding x-ray supervision, isotope supervision and the Danish Medical Board.
>
> With regard to x-ray supervision, I am officially the assistant to the Danish Medical Board’s expert adviser in this field, Professor HMH, and the insignificance of my official position is perhaps best illustrated by the fact that the Danish Medical Board pays me DKR 225 per month. As Professor HMH is so busy as University Vice-Chancellor and President of the (Danish section of the) Red Cross, I independently took over the whole of the x-ray issue 6–8 years ago (because I was sorry to see the cases piling up), and the Danish Medical Board has (learning from long waiting times in Prof. HMH’s time) approved my decisions without comment and, after a couple of years, resolved to send the x-ray cases directly to me for a decision.
>
> The capacity of the Isotope Committee was reduced at a time when I was busy drawing up a proposal for a new x-ray rule, and when Prof. HMH happened to ask if I might have the time and desire to be part of it I said no. Since Professor Jacobsen was in America, Dr. Koch was recruited, if anything as a representative of Prof. Bohr. So were Chief Physician Jens Nielsen, Radiumstationen (as Radiologist), Dr. Børge Christensen (Medical Doctor at the Finsen Laboratory) and Dr. Hilde Levy (physicist from the University Isotope Lab.). It is this Committee, rather than Dr. Koch, which is responsible for protection against ionising. Around 6 months ago, the Danish Medical Board asked Prof. HMH whether ‘the radioactive supervision’ could be linked to the Biophysics Laboratory, and when HMH asked me for advice, my answer was that this must be conditional upon the fact that we, rather than Dr. Koch, were made responsible

leaders thereof (among other things because I know of nobody with whom Dr. Koch has had smooth cooperation for a long time). Prof. HMH agreed with me, but later said that it had to be surrendered because 'Dr. Koch did not want to give it up' – which does not correspond fully with your view of Koch's approach to the matter. The only thing I have since heard about the case is that Chief Physician Henningsen once said in passing that he would like to talk to me about the supervision of isotopes at the hospitals if an opportunity arose since there really was no point having two men travelling around looking at x rays and radium. I have thought about adopting the same point of view for the matter regarding laboratory space.

I have just been interrupted by your new telephone call and then expect to travel anyway – along with Dr. Koch, whom I will try and contact on Tuesday. I would rather you did not show this letter at the time of our visit.

With the warmest regards and looking forward to seeing you again, your devoted friend

P. RØNNE-NIELSEN

Rønne-Nielsen's letter shows that, irrespective of the conflicts between him and Professor Jørgen Koch, it was the Danish Medical Board that made the decisions. The person who did so in real terms was Medical Officer *Eigil Juel Henningsen*, the man who spent ten years as Sievert's closest Danish cooperation partner and who, by virtue of his personality, would play an important role in the Nordic radiation protection cooperation.

In March 1955, a preparatory Atomic Energy Commission was set up by the government, whose driving force was the Minister of Finance and subsequently the Prime Minister, *Viggo Kampmann* (1910–1976). Niels Bohr became Chair of this Commission. The Commission had an Executive Committee which included Jørgen Koch's brother, Hans Henrik Koch, who was Kampmann's powerful head of department. Jørgen had tremendous respect for his brother, who had played an important role in the Danish resistance during the German occupation.

In June 1955, thanks to Bohr's prestige and international contacts as well as Jacobsen's friendship with people like John Cockcroft, the Danes succeeded in reaching an agreement with the United Kingdom and the USA regarding the peaceful use of atomic energy.

In the Preparatory Commission's Executive Committee, Hans Henrik Koch thought about the continued development. The most obvious thing to do was to create an equivalent of the Swedish *Atombolaget*, i.e., a company whose capital would be funded by the State and industry. However, Koch proposed another solution, a solution that was completely unconventional, i.e., to place the responsibility with a permanent Atomic Energy Commission under the Ministry of Finance. The anniversary issue about Risø (Niel, 1998) says: 'The solution would definitely be breaking with tradition in not having such a Commission linked to a proper specific Ministry, but on the other hand it would be in keeping with another Danish tradition – that of being able to improvise'.

Koch got his way. In December 1955, the Danish Parliament adopted a law which placed the responsibility for the development of a permanent Atomic Energy Commission (AEC) with the Ministry of Finance, as well as a smaller Executive Committee to rationalise the work. The government thought that a Danish atomic research station would be necessary if Denmark were to be able to enjoy the anticipated benefits of nuclear energy. The Risø peninsula by the Roskilde fjord was selected as the site for the research station. Risø was not seen as a goal but as a 'means to solve Denmark's energy problem'.

Bohr became Chair of the Atomic Energy Commission and Koch became Chair of the Executive Committee. Because, in practice, the latter assignment involved having the greatest influence, this meant that in reality, Hans Henrik Koch became the leading player.

In 1939, the National Physical Control Laboratory (later called the National Radiophysical Laboratory) was established in *Norway* for the purpose of supervision in accordance with the X-rays Act of 19 June 1938. The Act regulated the use of radiation and specified, among other things 'To protect life and health and to make installations and devices most effective and beneficial to the sick, the King may provide regulations…'. The head of the Laboratory was physicist Nelius Moxnes. In 1964, the Laboratory became an independent organisation, the National Institute of Radiation Hygiene (SIS). By then, Moxnes had been succeeded by Kristian Koren. In 1975, the Institute moved into a new building in Bærum and in 1993 its name was changed to the Norwegian Radiation Protection Authority.

Following several months of discussions between the Norwegian Technical Science Research Council (NTNF) and the Norwegian Ministry of Defence, on New Year's Day in 1948 the Research Council decided to set up an Institute for Atomic Energy (IFA). The new Institute would come under the NTNF and its main task would be to 'do research regarding uses of the chain reactions arising when specific heavier atomic nuclei are split'.

The Institute was initially to consist of a planning committee with the head of research at the Defence Research Institute (FFI), Major Gunnar Randers, as Chair. Randers, an exuberant astronomer and military physicist, had been one of those involved in the initiative for the formation of the FFI, and when the Military Research Institute came into being in April 1946, he became head of its physics department.

While on a study trip in the USA, Randers had made a personal decision. In his book, *Lysår* [Light Years], he writes that the American trip had convinced him that a nuclear reactor should be built in Norway and that 'it is obvious that the Defence Research Institute, with its fresh new forces and decent appropriations, has to be the starting point'.

However, it was not the FFI but the IFA who ended up building the reactor. The formation of the IFA in 1948 was a surprising development and was based on the fact that some of the university physicists were afraid that unrestricted nuclear physics research would suffer if the Defence were to gain a monopoly on the reactor development. It was now also obvious that it was neither economically viable nor politically desirable to invest in Norwegian nuclear weapons. In this connection, the reactor development became less interesting to the Defence but, owing to a substantial personal interest in Norwegian atomic energy research, Defence Minister *Jens Christian Hauge* (1915–2006) still succeeded in producing funds from the Norwegian Defence budget, by virtue of which it was possible to build the first reactor.

One particular research station, Kjeller, north of Lilleström, 15 kilometres east of Oslo, was to be the site of the reactor. There was access to heavy water from Norsk Hydro A/S, but not uranium. I wrote about this problem in 'The Sword of Damocles':

> Randers tried to get the British government to substitute uranium with heavy water but the British did not dare, bearing in mind its dependence on the USA. Randers then turned to France, a country that was outside the British-American coalition. He was able to refer to the help with the heavy water that Norway had given France during the war. It was also heavy water from Norway which facilitated the first French experimental reactor, ZOÉ. Randers therefore contacted Joliot following some trepidation. The Norwegian government was not enthusiastic about cooperating with a communist scientist.
>
> Joliot thought all the trump cards were in his hand in being the only one who could help Randers. He demanded that the Norwegian reactor be seen as a joint French-Norwegian project and offered to give the Norwegians uranium, but no instruction as to how to transform it into purified metallic form.
>
> Randers refused to accept the terms. Instead, he turned to the Netherlands, who happened to have ten tonnes of uranium oxide that a forward-thinking university professor had purchased in 1939. The French regretted having been so exacting and now suggested that the project constitute cooperation between the three countries, whereby the French promised to provide all necessary technical information.
>
> This worried the Americans, who were afraid that France would gain too dominant a position. If the Norwegians limited themselves to cooperation with the Dutch, the USA was willing to isolate metallic uranium from the uranium oxide.
>
> The Norwegian-Dutch reactor in Kjeller was completed in 1951 and was the first reactor plant that was opened to scientists from other countries. It was called JEEP (Joint Establishment Experimental Pile) and the planned output was 250 kilowatts. This was to ensure an adequate neutron flow to be able to produce radioactive nuclides for medical use and (wrote Randers) 'for neutron research with a view to subsequent power reactors'.

> The Norwegian-Dutch cooperation continued until 1960. The original hope that it would lead to vital nuclear power industries in both of the countries never needed to be fulfilled. Hydropower, natural gas and North Sea oil made nuclear power unnecessary. However, another Norwegian reactor, located next to Halden and with a heat output of 25 MW, was built during the 1950s in a research project under the OECD for testing nuclear fuel, among other things.

The original cooperation agreement regarding the Halden reactor was reached in June 1958. The reactor was commissioned on 4 October 1960.

In the 1950s, a number of organisations were formed in Europe which would be extremely important to the future. On 18 July 1956, what was then known as the Organisation for Economic Cooperation in Europe (OEEC) set up a 'nuclear energy steering committee' which had the task of promoting the Member States' cooperation regarding the peaceful use of atomic energy. This committee was later replaced by ENEA (see below).

In February 1957, the Nordic Council, which had been formed in 1952 but of which Finland did not become a member until 1955, decided at its 5th session in Helsinki to appoint a Nordic Liaison Committee for Atomic Energy (NKA). The NKA held its statutory meeting in Copenhagen in June 1957. Those taking part were top politicians with an interest in atomic energy; the NKA was controlled by the Chancellery of the Nordic countries. Those participating in the Copenhagen meeting included Hans Henrik Koch from Denmark, Jens Christian Hauge from Norway, and from Sweden Harry Brynielsson, Hans Håkansson and the Secretary of State for the Ministry of Trade *Gustaf Cederwall* (1913–2008).

In the 1960s and 70s, the fact that reactor safety and radiation protection could come under different departments would mean that the cooperation between the NKA and the Nordic countries' radiation protection authorities was not be entirely without friction. Those who were politically responsible for nuclear power safety matters would have liked to also have seen the radiation protection matters discussed by the cooperation. However, the cooperation between the Nordic countries' radiation protection authorities was already excellent and efficient thanks to the efforts of Sievert, Juel Henningsen, Eker and Mustakallio, and the radiation protection authorities viewed any initiative within this area by the NKA with suspicion.

On 25 March 1957, the six countries in the European Coal and Steel Community – Belgium, France, Italy, Luxembourg, the Netherlands and (the former) West Germany – signed the Treaty of Rome, which was the Treaty establishing the European Economic Community (EEC) which was the introduction to the early EC cooperation. At the same time, a common nuclear power organisation, EURATOM, was formed. The 'European Community' (EC) initially referred to the three organisations - the European Coal and Steel Community, the EEC and EURATOM.

On 1 February 1958, the OEEC's Council of Ministers[*] formed the European atomic energy organisation called the European Nuclear Energy Agency (ENEA[†]), which was to run two European reactor projects (the Halden project in Norway and the Dragon project in England) as well as a reprocessing plant at Mol in Belgium under 'Eurochemic'. One of the most important permanent committees established by the ENEA (and later the NEA) from the radiation protection point of view is the CRPPH (Committee on Radiation Protection and Public Health) with its exuberant Secretary *Emile Wallauschek*. EURATOM set up a research centre in Ispra in Italy.

The Risø Research Centre was opened on 6 June 1958 in the presence of the sovereigns and with a tribute speech by Viggo Kampman and Niels Bohr. The following is written about Koch's input in the anniversary issue (Niel, 1998):

> When the official opening of Risø was successful as early as June 1958, this success was attributed particularly to the fact that the AEC chose to pass the initiative to the Executive Committee and its Chair, H. H. Koch. Koch's contacts with the government

[*] OEEC (Organisation for European Economic Cooperation) was an earlier name for the OECD before the organisation gained non-European members.

[†] The designation ENEA was changed to NEA when the organisation gained non-European members.

> and the political parties in Christiansborg meant that he was usually in a position to correctly judge what would be politically backed in a given situation, particularly when it came to substantial financial arrangements. The minutes from the AEC meetings also show that Koch was always very well-prepared for these meetings and that in the initial years he cooperated closely with the chair, Niels Bohr. The course of a typical AEC meeting was that once the Chair had opened the meeting he would immediately hand over to Koch, who then gave the members a thorough briefing on the most essential matters that had been dealt with by the Executive Committee since last time. When the AEC then got to grips with the other points on the agenda, it was Koch who possessed all of the background information and who therefore spoke for a lot of the time. Finally, it was left to Niels Bohr to draw the conclusions. The conclusions were very rarely far away from the intentions that Koch had expressed to start with.

Rolf Sievert attended the opening. He was Chair of ICRP at the time and had recently also been elected as Chair of UNSCEAR. The first report from UNSCEAR had been approved by the Committee and was *en route* to being published. ICRP's editorial group under Professor Failla had submitted its proposal for new recommendations and the Commission was to give an opinion on this proposal in September. Sievert was therefore seen by the news journalists who were present as an important source of news and he was therefore pretty harassed. But Sievert, who did not feel that he had a mandate to give out advance information on what would happen, sent the journalists away, perhaps a little too brusquely. A conflict arose which seems to have been echoed in the media.

The radiation protection activity in *Italy* originates from the interest in radiation measurements that were a consequence of Enrico Fermi's research activity in the 1930s, but also in medical radiology. Professor *Enzo Pugno Vanoni* (1899–1939) and radiologist *Pasquale Tandola* (1870–1934) from Naples issued joint radiation protection recommendations in 1933. Professor *Felice Perussia* (1885–1959) developed methods for dosimetry together with Vanoni as early as the 1920s. In 1938, Professor *Giulio Cesare Trabacchi* (1884–1958) built a standard chamber for measuring in röntgen units.

In the 1950s, the Italian Society of Medical Radiology set up a radiation protection committee and published radiation protection standards in 1958. In 1952, lectures were arranged at the University of Technology in Milan on the effects of radiation, dosimetry and radiation protection within nuclear physics training, with Felice Perussias' son *Aldo* as one of the lecturers, and the 18 year-old student *Arrigo Cigna* was able to measure the radioactive fallout from the nuclear weapons testing as early as 1951.

At the end of the 1950s, the Italian government set up a National Committee for Nuclear Research (CNRN) and established dosimetry laboratories in Ispra, Bologna and Frascati, plus other laboratories in Rome in 1959.

The Italian society for 'Health Physics' was formed in 1958 with Professor *Piero Caldirola* as President. After EURATOM had been established, two Italian experts, *Mario Chiozzotto* and *Carlo Polvani*, were asked to draw up European radiation protection recommendations under the leadership of Hermann Holthusen. This was how Polvani came to EURATOM's research centre in Ispra.

In 1960, the CNRN was reorganised into a National Committee for Nuclear Energy (CNEN) with laboratories in Casaccia and 150 employees. Cigna became head of the environmental laboratory.

In 1948, faced with the expectations of comprehensive radioactive nuclide production, the *British* government had already passed a Radioactive Substances Act, thereby also creating a Radioactive Substances Advisory Committee. This led to cooperation between a number of organisations with responsibility within the radiation protection area. In addition to the Medical Research Council (the MRC), the main department that was involved with radiation protection and radiation measurements, and the group which handled radiation protection matters in Harwell, was the one within the National Physical Laboratory, the Radiological Protection Service (RPS).

When the British atomic energy programme was moved from the Ministry of Supply to the newly-established Atomic Energy Authority (AEA) in 1954, Christopher Hinton's group in Risley, which was responsible for facility planning, was called 'the industrial group'. Following the Windscale accident where Risley's doctor, Andrew McLean together with head of safety F. R. Farmer and head of radiation protection John Dunster, had made an important achievement for the protection of the surroundings, a special radiation protection section was created within the AEA (the Authority's Health and Safety Branch, or AHSB). This was given a reactor safety department in Risley and a radiation protection department in Harwell, but its headquarters were established in London.

In 1956, the RPS was transferred from the National Physical Laboratory to the Medical Research Council and was moved from Teddington in the suburbs of south-west London to Sutton in Surrey, a few miles south of London where there was already a premises for a medical physics department within the Institute for Cancer Research that was linked to the well-known cancer hospital, the Royal Marsden Hospital on the Fulham Road in London. The department was managed by Sievert's old adversary, Professor Val Mayneord, and the eccentric Professor *Len Lamerton* was also there as Mayneord's closest aide.

In the hospital area in Sutton there were six Victorian buildings that had previously been used as a care home for children with tuberculosis. They were designated by the letters A–F. Buildings A, B and C were used by Sutton's general hospital. Building E was shared by the unit for cancer statistics and the RPS while Professor Mayneord's physics department was in Building F. The sixth building, D, was still empty and was pretty derelict.

There were now two organisations for radiation protection in the United Kingdom. The oldest was the RPS, which had roots going back to 1912 within the National Physical Laboratory where it had been called the Radiology Division until radiation protection dominated the activity at the start of the 1950s. Following a reorganisation which meant that the activity was fully devoted to radiation protection, Walter Binks started the job as head of the new organisation, the RPS, on 1 January 1953. In October 1955, Eric Smith left the National Physics Laboratory to become Binks' closest aide at the RPS.

The younger organisation was the radiation protection department (RPD for Radiation Protection Division) within the AEA's Health and Safety Branch. The RPD was primarily in Harwell. It was not only the years which determined what was 'young' and what was 'old' - it was also tradition. The RPS was dominated by Binks and Smith, who had experience of radiation protection work during the war (including the fluoroscopy of bombs which had not exploded) and who both worked at the National Physics Laboratory for a long time. The AHSB and RPD, whose key people were McLean, Marley and Dunster, did not have the same classical laboratory tradition behind them, but more so represented the new nuclear physics within a rapidly growing, semi-military, industrial complex.

The situation was not unusual. In many countries, as in the Nordic countries, there was an old radiation protection tradition which had experience of the problems within healthcare and research and links to the department responsible for healthcare and medical treatment. In parallel, a young competence was growing in nuclear physics with interest in radiation protection aimed at the atomic energy problems. This is where other departments came into the picture, with atomic energy authorities often being formed with an interest in nuclear power and radioactive substances and, in some countries, influenced by military interests.

However, in the United Kingdom, it was not only this competition situation between the Healthcare Authority and the Atomic Energy Authority which made the position untenable. The AEA's Health and Safety Branch functioned throughout the 1960s, but criticism began to be levelled at its dual role as representative of the government in radiation protection and safety matters while also being responsible to the AEA. Discussions over several years led to the creation of a completely new, independent radiation protection organisation, the National Radiological Protection Board, which was formed through the 1970s British radiation protection law. The NRPB would end up taking over the functions from the Radiological Protection Service, the Radioactive Substances Advisory Committee and the overall responsibility for radiation protection from the AEA's Health and Safety Branch. The personnel were taken mainly from the RPS and RPD, but the first Director and Secretary of the NRPB came from the AHSB's office in London. Andrew McLean became the first head of the new organisation and Sir Brian Windeyer became the first Chair of the board.

As it is now widely known, Sweden having declared itself to be neutral did not substantially prevent cooperation with the western powers even regarding very sensitive matters. In October 1955, Sievert and Greg Marley agreed that I, together with Bo Aler from the FOA, should visit Harwell around the end of October and the start of November to exchange experiences regarding radioactive fallout measurements. The visit was not secret as such, but Sievert wrote to the Secretary of State *Carl-Erik af*

Geijerstam at the Ministry of the Interior that the newspapers should not to be informed of the visit because contacts with the United Kingdom in what could be referred to as technical defence matters were not something that they wanted to become public knowledge. However, Sievert foresaw huge problems with the fallout from continued atmospheric nuclear weapons testing and the emission of radioactive substances while nuclear power was being built up.

Aler and I were living at different hotels in London. I was to meet him at his hotel before the trip to Harwell. I thought the exterior door was rather stiff, and when I came into the foyer, I was met by a reproachful look from an old lady in the reception area. ‘But Sir,’ she said, ‘that door was locked!’ On the way to Harwell we spent the night at the Crown and Thistle in Abingdon, a hotel which later became well-known among radiation protection people. Never since that hotel bar have I seen so many contemporary Dickens, Conan Doyle and Agatha Christie-type examples of how typical British people were expected to look and behave.

A warm greeting awaited us in Harwell and we had long discussions with the knowledgeable little scientist *A. C. Chamberlain*. The exchange was definitely worth more to Aler than to me because he and the British were mainly interested in what the fallout contained and the way in which it had arisen, while the main thing I wanted to know was how powerful it was and what risks it could lead to.

In *France*, Professor Louis Bugnard at the *Institut d'Hygiene* in Paris was initially the dominant contact person for Sievert, but he was anxious to make room for his two mentees, Henri Jammet and *Pierre Pellerin*. This feat he succeeded in accomplishing by placing these two ambitious people in such positions that they were no longer competing with one another. Jammet became head of radiation protection within the French Atomic Energy Commission (CEA) at *Centre d'Etudes Nucleaires* in Fontenay-aux-Roses south of Paris. Pellerin became head of the civil radiation protection, i.e., *Service central de protection contre les rayonnements ionisante* (SCPRI) in Le Vésinet in the western suburbs of Paris. The SCPRI had premises spread around in a park area. Pellerin preferred to view work groups as his guests and treated them to wine and good food supervised by a boss in chef's whites. Jammet, who had another job at the *Fondation Curie* on the Rue d'Ulm near Panthèon in central Paris, preferred to hold his meetings there and lunched with his visitors at a nearby restaurant. On the other hand, he did often have guests to dinner at his elevated home by the Seine downstream of the Eiffel Tower.

In *West Germany*, the *Physikalisch-Technischen Bundesanstalt* had been performing voluntary tests on medical x-ray facilities since 1955. Professor Boris Rajewsky strove to obtain Federal regulation of radiation protection with a central radiation protection institute like Sievert's in Sweden, and he saw the PTB as this central institute. In 1957, the PTB was given a radiation protection laboratory with *Walter Kolb* as the person in charge.

In *East Germany*, a national *Zentrale für Strahlenschutz* was formed in 1962. The name was changed in 1973 to the *Staatliches Amt für Atomsicherheit und Strahenschutz* (SAAS). During the cold war, international contact with the German Democratic Republic (DDR) was little and superficial. However, Dr. *A. Rakow* at the (East) German Academy of Science's Institution for Biophysics wrote to me a few times to exchange information.

The contact with radiation protectionists and scientists in the Soviet Union was also superficial, taking place mainly through UNSCEAR and ICRP. Our understanding of relevant institutions and authorities in the Soviet Union in the 1960s was particularly clouded. Karl Morgan, who had the early ambition of establishing actual international contacts, was the first (for the period of 1956–1962) to get a Russian in his ICRP Committee, *M. N. Pobedinsky*. Even following the reorganisation of ICRP in 1962, Morgan was the only committee chair who had a Russian involved, *V. Schamov* from the former Leningrad. After that, it was not until 1965 that a Russian, Professor *A. A. Letavet*, was included as a member of the Main Commission. Our contact with Russian colleagues in UNSCEAR was initially more limited than the contact within ICRP due to the political monitoring of the Soviet delegates in UNSCEAR. The first Soviet UNSCEAR delegation consisted of Professor A.V. Lebedinsky as representative as well as the advisers Professors *K. K. Aglintsev*, *B. M. Isayev*, *P. M. Kireyev*, *A. N. Krayevsky* and *A. M. Kuzin* (who would succeed Lebedinsky as representative).

In *Switzerland*, the *Eidgenössisches Gesundheitsamt* (EGA) with Dr. *G. Wagner* as head issued guidelines in 1954 for protection against ionising radiation. In 1955, Reaktor AG (RAG) began the building work at Würenlingen and the surveys of the surrounding environment were carried out by *P.*

Courvoisier. Following the Atoms for Peace Conference in Geneva in 1955, Switzerland purchased the pool-type reactor that was part of the American exhibition. It was commissioned in 1957 under the name of SAPHIR with an output of 1 MW. The noted radiation protection personnel included *F. Alder*, *P. Courvoisier*, *W. Hunzinger* and *Guelfo Poretti*. A year later, a radiation protection department was established within the EGA with G. Wagner, and later Professor Walter Minder, as head.

In 1959, Switzerland came under a Federal Law on radiation protection and the peaceful use of atomic energy. The year after that, a 20-MW heavy water reactor called DIORIT was commissioned in Würenlingen. The RAG was then replaced by the *Eidg. Institut für Reaktorforschung*.

In 1961, the *Nationale Gesellschaft zur Förderung der industriellen Atomtechnik* (NGA) started planning an experimental nuclear power plant at Lucens (not to be confused with Lucerne!) on the road between Lausanne and Berne. This reactor was commissioned in 1968 but was decommissioned the following year due to an accident. A proposal for Swiss nuclear weapons was rejected in 1962 and again in 1963.

As of 1963, the year in which the first radiation protection regulation came into being, the responsibility for radiation protection lay with a department within the former *Eidgenössisches Gesundheitsamt* and the inspection of x-ray device started. Courvoisier was the co-founder and President of a European radiation protection society (the ESG), a predecessor of the *Fachverband*. In 1966, the *Fachverband für Strahlenschutz* was formed as well as the IRPA, of which Courvoisier became treasurer, but more about this in Chapter 19.

The European Laboratory for Particle Physics (CERN - *Conseil Européen pour la Recherche Nucléaire*) was formed after the Second World War, partly to prevent European scientists from emigrating to the USA which was dominant as regards research into particles. A number of scientists, including *Eduardo Amaldi* (1908–1989), *Pierre Auger* (1899–1993) and Nobel Prize winner Isaac Rabi, had taken the initiative and, with the support of UNESCO, CERN was formed in 1953. Initially, the whole laboratory was situated in Switzerland next to the border with France near Geneva, but it extended over the border due to its ever-increasing facilities.

The organisation of the radiation protection in the *USA* was a mystery to Europeans for a long time, but not sufficiently important for us to take notice of. It was the individuals who made the impression rather than their organisations. The dominant American in the international radiation protection work was Lauriston S. Taylor who was responsible for radiation physics at the National Bureau of Standards through various positions during 1927–1965. However, the American organisation with which Taylor was associated the most was not the NBS but the NCRP, an organisation that was formed in 1929 under the name of the Advisory Committee on X-ray and Radium Protection and which was named the National Committee on Radiation Protection (NCRP) after the war, but which finally became known as the National Council on Radiation Protection and Measurements. The change from 'Committee' to 'Council' reflected the Federal support that the independent organisation received as an advisory body. Taylor was Chair of this organisation from the word go and for half a century. At the NBS, Taylor cooperated with Harold Wyckoff, who was head of the radiation physics laboratory for a long time. In 1953–1969, Taylor was Chair of ICRP's sister commission, ICRU, and was then succeeded by Wyckoff, who had been Secretary of ICRU since 1956. Wyckoff continued as Chair of ICRU until 1985.

Equally well-known to those around him was Karl Ziegler ('KZ') Morgan, firstly thanks to his achievements as Chair of ICRP's Committee II and the equivalent Committee within the NCRP in producing the comprehensive tables containing recommendations for the maximum permissible concentrations – 'MPC' values – for different radionuclides, and later as the initiator of the Health Physics Society and the IRPA (the International Radiation Protection Association). Morgan was associated with the Oak Ridge National Laboratory where he managed the radiation protection activity. Many thought that Morgan's group was behind all of the radiation protection at Oak Ridge, but this was not the case. The group had no responsibility for the radiation protection at the large uranium-235 separation facilities.

Less well-known to the surrounding world but perhaps playing a crucial role in the formulation of ICRP's policy in the 1950s was 'Gino' Failla, Professor of Radiophysics at the College of Physicians and Surgeons at Columbia University in New York. Columbia University Medical Centre also had

Failla's esteemed colleague, medical physicist Edith Quimby, as well as medical physicist Carl Braestrup at Delafield Hospital.

I have written detailed information about the organisation and the facilities and participants in the Manhattan Project in the 1940s in 'The Sword of Damocles', which describes the Oak Ridge, Argonne and Brookhaven national laboratories as well as the Hanford Engineering Works and those who worked there. It also describes the Canadian facility at Chalk River.

After the war, the Manhattan District was replaced by an Atomic Energy Commission (AEC). The Congress' Joint Committee on Atomic Energy was set up at the same time to supervise the AEC and arrange the Public Hearings which might be needed. These organisations and their representatives played a fairly insignificant role outside the USA with the exception of their involvement in and responsibility for the comprehensive nuclear explosions which were carried out in the Pacific Ocean area. However, the scientists at the AEC's New York Operations Office, Merril Eisenbud and John Harley, were active in the UNSCEAR work.

With the exception of the independent NCRP, which had no regulatory responsibility, there was no Federal Radiation Protection Authority in the USA, and the Federal authorities that dealt with radiation protection matters were usually only advisory at Federal state level. The exception was the Atomic Energy Commission, which had total control over nuclear facilities. However, in August 1959, President Eisenhower set up a new body, the Federal Radiation Council (FRC) to 'advise the President on matters concerning radiation' and to ensure that the limit values were selected in an objective way. The politicians were uneasy about radiation protection recommendations coming mainly from the NCRP and ICRP, organisations that had no regulatory responsibility and, in the case of ICRP, consisted mainly of foreigners. Initially, the FRC had six members, including the two who had the greatest interest in moderate radiation protection requirements – the AEC and the Department of Defence.

The FRC published Guidelines on radiation protection in 1960 and 1961 but was criticised for not recommending adequate protection measures against the radioactive fallout that was the result of the powerful test explosions in 1961 and 1962. As of 1962, data also started becoming available on the risk of mine workers who were exposed to radon and its daughter products. In 1967 there was so much evidence of this risk that demands were made for limit values to be sharpened, which met with opposition from both the AEC and the FRC. The FRC's Managing Director at the time, Dr. Paul Tompkins, said in a hearing before the Congress' Joint Committee on Atomic Energy: 'The primary objective of the FRC is to make recommendations which represent a reasonable balance between biological risk and the impact on uranium mining'. Following long debates regarding its role, the FRC was dissolved in 1971 and its duties and tasks were transferred to the Environmental Protection Agency (EPA), which adopted a more cautious approach and was eventually criticised by other authorities for being over-cautious.

The AEC was dissolved in 1974 and replaced in the following year by the new authorities, the Energy Research and Development Administration (ERDA) and the Nuclear Regularity Commission (NRC). The ERDA was in turn replaced by the Department of Energy (DOE) in 1977. These authorities had different ways of looking at the radiation protection matters that were on their agendas, which led to competition and conflicts in which the Federal Environmental Protection Agency also became involved, but this falls outside of the framework of this book. The authority that was primarily responsible for radiation protection recommendations regarding the medical and technical use of radiation was the Division of Radiological Health at the US Public Health Service, an authority which came under the Department of Health, Education and Welfare (HEW). The head of radiation protection was Dr. *D. R. Chadwick* and assistant manager *Mr. J. G. Terrill*. The activity was transferred to the US Food and Drug Administration (FDA) in 1971 under the name Bureau of Radiological Health, and was later named the Center for Devices and Radiological Health. Its manager from the start until 1990 was *John Villforth* (1930–2019).

In *Canada* there were mainly three groups that were responsible for radiation protection endeavours in the 1960s. Within the Ministry of Health (Department of National Health and Welfare) there was a department called Environmental Health and Special Projects where Ernest Watkinson was a manager, going by the title of Principal Medical Officer. Under this department was a work protection bureau called the Occupational Health Division. Under this, in turn, was the radiation protection organisation which went by the name of Radiation Protection Services before it became an independent bureau called the Radiation Protection Division. The head of the latter for some time was an ambitious physicist called *Peter Bird*, who gradually advanced up the Canadian administration. David Sowby, Secretary of ICRP, still had a job there until 1981 and was formally hired out to ICRP.

The Canadian department, which issued permits for all nuclear physics activities, including the use of radioactive substances (with the exception of radium the same as in the USA) had an executive body called the Atomic Energy Control Board, which initially followed the advice given by the Radiation Protection Services, which also carried out necessary inspections. However, the AECB gradually became more important and took over the actual supervision.

The third organisation of significance was the Atomic Energy of Canada Ltd (AECL) which ran the operations at Chalk River. The first head of medicine within the AECL was André Cipriani, who was also head of the radiation protection activity. Following his death in 1956, the radiation protection was separated from the medical activity. Gordon Stewart became head of medicine after Cipriani, and the best-known among those who ended up being responsible for radiation protection for different periods was *Gordon Butler*, who later came to the AECB and was Canadian representative at UNSCEAR for a long time.

In *China*, radiation protection developed mainly through the concentration of efforts in nuclear technology, initially with military interests. The activity, as with Sievert in Sweden, was initially closely associated with the development of measurement instruments and measurement methods. This activity was located mainly in Taiyuan in the province of Shanxi, approx. 400 km south-west of Beijing. What is now called the China Institute for Radiation Protection was given its name in 1988. It originated from the Beijing Industrial Hygiene Institute which was set up on 7 May 1962 with personnel from the China Atomic Energy Institute.* On 13 July 1962, the Beijing Industrial Hygiene Institute merged with the North China Atomic Energy Institute and the Shanxi Provincial Radiological Medicine Institute to form a new institution called the North China Industrial Hygiene Institute. The merger became evident when the Beijing Industrial Hygiene Institute also moved to Taiyuan in October 1964. On 25 November 1977, its name was changed to the North China Institute of Radiation Protection. On 7 October 1983 its name changed again, this time to the Institute of Radiation Protection under the Ministry of Nuclear Industry (MNI). The final name change was on 22 October 1988, when it became the China Institute for Radiation Protection (CIRP). Professor *Li Deping* was head of the Institute for many years, a man who, like Sievert, was extremely interested in both radiation protection and measurement instruments. Professor Li was a member of ICRP's Main Commission from 1981–1993.

The first Chinese nuclear test explosions took place at Lop Nor, the old, dried-out salt sea in Xinjiang (Chinese East Turkestan) in western China. The three first explosions took place on 16 October 1964, 14 May 1965 and 9 May 1966.

Following the commotion caused by the radioactive fallout on the 'Lucky Dragon', there was actually only one national organisation in *Japan* with sufficient knowledge and foresight. That was the Atomic Bomb Effect Research Commission with the head of the Japanese National Institute of Health, Dr. *Rokuzo Kobayashi*, as Chair. However, it was shown that some form of coordination between different departments was necessary in a disaster situation where the problems affected many different areas of activity. In May 1954, the Science Council of Japan therefore set up a committee (the Special Committee on the Effects of Radioactivity) to coordinate and lead the research that was necessary. The Committee and its work groups included a good hundred scientists under the leadership of the Research Council's President, Dr. *Seiji Kaya*.

A month later, the government had the Japanese Ministry for Health appoint an organisation to coordinate the scientific activities within the Ministries (the Council for the Coordination of Research for Measures Against Atomic Bomb Injuries). This cooperation council then also participated in the

* Because it is difficult to reproduce the original names in Chinese, I have chosen to use the English names given by the Chinese themselves. The same applies to the Japanese names.

scientific discussions that were held by the Japanese and the Americans and which helped to alleviate the irritation that prevailed among the Japanese scientists.

At the same time, Japan had the Atomic Bomb Casualty Commission (ABCC), formed jointly by the Japanese and the Americans, whose task was to assess the medical consequences of the nuclear bombings of Hiroshima and Nagasaki. Its activity was located in Hiroshima.

Prior to the UN's Atoms for Peace Conference in 1955, Japan had had no plans to build nuclear reactors. The conference changed that attitude. At the start of 1956, the Japanese Atomic Energy Commission (AEC) was formed as a policy-creating group as well as an atomic energy body, the Atomic Energy Agency, for the administrative assignments. Immediately thereafter a Japanese Atomic Energy Research Institute (JAERI) was established, half of which was a national body for research within the field of nuclear energy. JAERI was allocated a 250-acre area next to the city of Tokai on the coast, 100 km north of Tokyo (the Japanese write 'Tokai-mura', where 'mura' means village).

Japan's first nuclear reactor, JRR 1 (for 'Japanese Research Reactor no. 1') at JAERI, was an American boiling water reactor with a thermal output of 50 kW. It was commissioned in October 1957. The next reactor, JRR 2, had a thermal output of 10 MW and was commissioned in 1958. JRR3 was a reactor with its own design, also 10 MW, with natural uranium and heavy water. Building work for Japan's first nuclear power plant started in 1960 in Tokai, done by the Japan Atomic Power Company, a company formed by the power industry. It had a graphite-moderated, gas-cooled (carbon dioxide) reactor and had an electrical output of 166 MW. It was commissioned in 1966 and was used until 1988.

In 1957, Japan's National Institute of Radiological Sciences (NIRS) was formed under the Japanese government's Science and Technology Agency. It was allocated premises in Chiba, which is a port 35 km east of Tokyo (the Japanese write 'Chiba-shi', where 'shi' means town). The NIRS would play an important role as forum for the international radiation protection activity.

The first Japanese UNSCEAR delegate was Dr. Masao Tsuzuki. He was succeeded in 1960 by Dr. *Kempo Tsukamoto* (born in 1904), who was then also head of the NIRS. No Japanese or Chinese became a member of ICRP until the 1970s.

The radioactive fallout from an increasing number of atmospheric nuclear weapons tests in the mid-1950s worried the public in many countries. This concern did not lessen when it became known that Chicago physicists Leonidas Marinelli and Charles Miller had been able to use scintillation spectroscopy to show caesium-137 from the test explosions in the Pacific Ocean in the bodies of people in Illinois.

Following the destruction of Krakatoa in 1883 in the Sunda Strait between Java and Sumatra, the volcanic ash had been distributed all over the world and caused wonderful, colourful sunsets all over the world for several months. Similarly, the radioactive substances from the Bravo detonation on the Bikini Atoll on 1 March 1954 (local time) had spread themselves all over the globe. The report from Miller and Marinelli in *Science* in 1956 made an impression which is difficult to imagine today. The bombs that were tested in the Pacific Ocean spread radioactive substances which could be shown in the bodies of people on the other side of the globe! It is no wonder that Rolf Sievert sat at his desk at the Institute of Radiophysics in Stockholm carefully studying reports on what had been observed following the Krakatoa disaster.

The international unease had already led to the UN setting up its scientific radiation committee, UNSCEAR, in December 1955. The unease also led to 'white papers' from the United Kingdom and the USA in 1956.

The British white paper was published by the Medical Research Council under the title *The Hazards to Man of Nuclear and Allied Radiations*. It was a veritable gold mine where valuable information was concerned. The report was written by a special committee of 17 scientists. The Chair of the Committee was Sir *Harold Himsworth* (1905– 1993), who was then Secretary of the Research Council.

The report consisted of a main text, approximately 80 pages long, as well as 13 Appendices. The main text gave an excellent account of the knowledge of ionising radiation at the time and the biological effects thereof. It discussed the risk of leukaemia but dealt primarily with the genetic effects of ionising radiation. Other forms of cancer than leukaemia were discussed very summarily; the knowledge was as yet pretty inadequate. With regard to nuclear weapons testing, the risks were summarised in the following two paragraphs (UKMe, 1956):

> The genetic effects to be expected from present or future radioactive fall-out from bombs fired at the present rate and in the present proportion of the different kinds are insignificant. They might not be so, if present rates of firing were increased and particularly if a greater number of thermonuclear weapons were tested.
>
> So far as radioactive fall-out may affect the individual, we believe that immediate consideration would be required if the concentration of radioactive strontium in bone showed signs of rising greatly beyond that corresponding to one-hundredth of the maximum permissible occupational level.

However, the greatest value in the British report lay in the 13 Appendices which contained the classic reports from W. M. Court-Brown and Richard Doll on the elevation in the incidence of leukaemia among the survivors in Hiroshima and Nagasaki as well as in patients who had been treated with x rays for Bechterew's disease. There was an outline of the doses of radiation from natural sources of radiation, written by Professor Spiers. *C. H. Waddington* and *T. C. Carter* gave an account of what they knew about the doubling dose for genetic effects following the irradiation of various animals and plants. It contained the first essays on genetically-significant doses of radiation: Sidney Osborn calculated it for diagnostic x-ray examinations and E. E. Smith for the irradiation in the work life. W. G. Marley reported the result of fallout measurements in the United Kingdom. And in the final Appendix, Professors Mayneord and Mitchell attempted to estimate the risk from the absorption of strontium-90 in the human body. Before the reports from UNSCEAR started to be published, these essays constituted the primary source of knowledge for the radiation risks from ionising radiation.

The American white paper was published by the US National Academy of Sciences' National Research Council with the title *The Biological Effects of Atomic Radiation*.

Sievert realised early on that the worry about the radioactive fallout from the nuclear explosions made it politically possible for him to obtain greater resources for research, right at the time when the 1951 radiation protection committee had just finished its report in 1956. By now, Sievert was meant to have been able to link several experts on important limit areas to his institution, but he realised that such experts would probably be isolated from their actual specialist areas and, after a while, be no more than substandard radiation protectionists. Instead, he procured funds to support a number of scientists in their own research environment and was assisted in this by Torsten Magnusson of the FOA. The support primarily concerned *Lars Fredriksson* at the National Agricultural Experiment Farm* in Ultuna, Lennart Hannerz at the National Board of Fisheries, and *Bertil Åberg* (1925–1992) at the Royal Veterinary College of Sweden.

In July 1957, the Livermore Laboratory at the University of California initiated what was called Plowshare, a programme whose aim was to find peaceful uses for atomic energy, primarily nuclear explosions. This programme also gradually introduced international discussions on using nuclear blasts to create new harbours or change the course of rivers to irrigate dry areas. Some of these plans were discussed at the meetings arranged by IAEA. The plans raised little enthusiasm among radiation protectionists, not just because of the radiation risk but also due to the often amateurish ideas that were thought to be drawing board products with inadequate analyses of the ecological consequences.

One project that worried many but which luckily did not come to fruition set out to blast out a canal to link the Amazon River with the Orinoco. The consequences of linking these enormous rivers could not exactly be overlooked (to compare these with something like the Dalälven's 380 m^3/s, the average water flow at the outflow of the rivers is 14 000 m^3 per second for the Orinoco and 220 000 m^3/s for the Amazon River).

Another project for which the United States was making serious preparations was the use of blasts to create a new and better Panama Canal. Preparatory surveys were begun in 1958 and the result was reported to President Eisenhower in spring 1960. Four years later, the American government resumed

* From 1948, the National Experimental Agriculture was the name of what had previously been called the Agricultural Research Institute. In the 1970s, the activity was incorporated into the Faculty of Agriculture at the Swedish University of Agriculture,

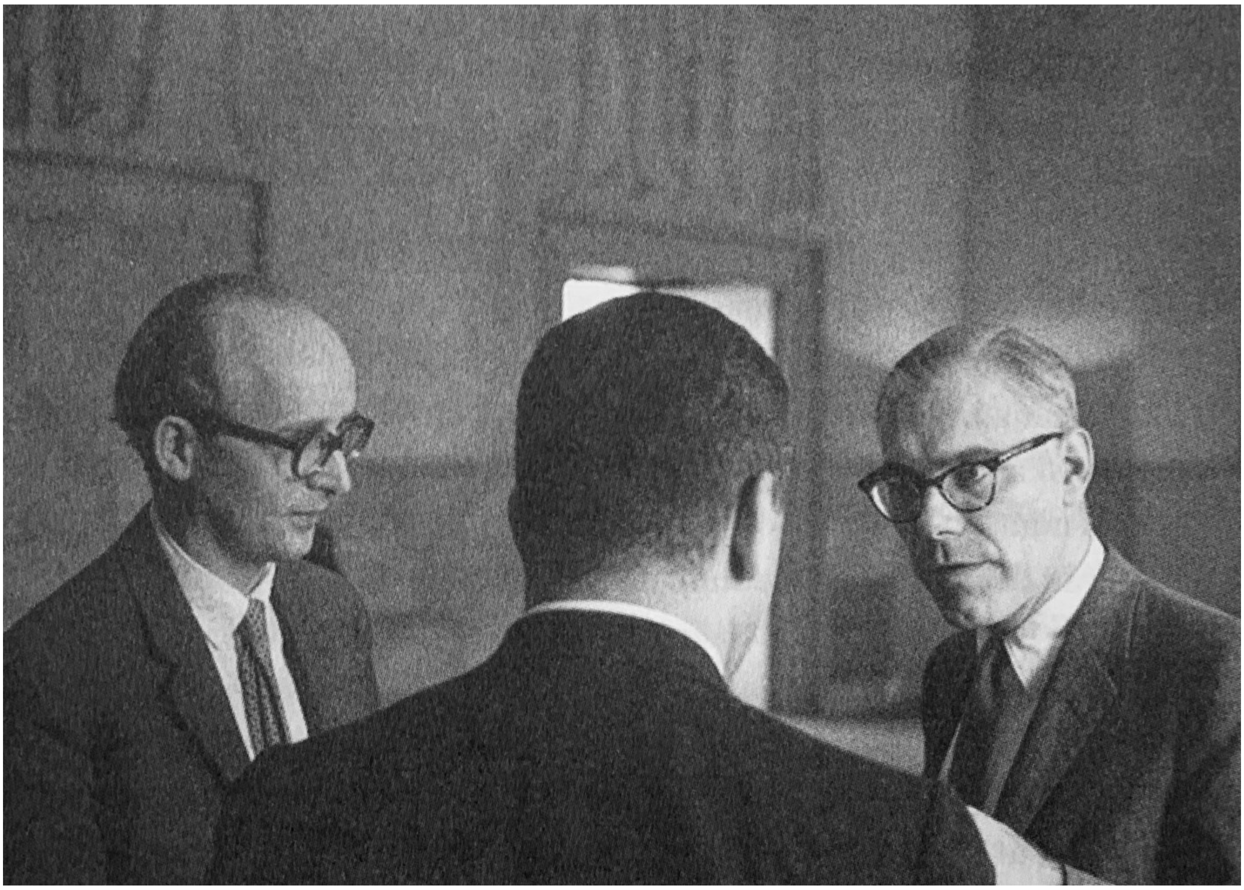
its analysis of the matter and Congress decided to appoint a Commission to provide a detailed proposal. The Commission reported in 1968 but no decision was made.

The first edition of the IAEA Bulletin came out on 1 April 1958, with a picture of the organisation's headquarters, the old Grand Hotel on Kärntner Ring on the cover. A special edition on 1 September concerned the second Atoms for Peace Conference in Geneva.

In February 1959, Sievert held a lecture at WHO in Geneva on the primary problems of radiation protection. He was concerned about an expected increase in people's exposure to ionising radiation when more and more sources of radiation could be expected to start being used. He said:

> However, the increase in the population's irradiation may be so rapid that we don't have time to wait for results from research into fundamental mechanisms of radiation effects if a period of uncontrolled increase of radiation injuries is thought to be unacceptable. We must therefore turn directly to the problem with the connection between irradiation and the risk of harmful effects on people. Animal experiments will help to shed light on the nature of the effects of radiation and on the prevention and cure of some types of radiation injuries, but they will not give us the direct information that is now required.
>
> We may find the threshold values below which the risk of specific harmful effects can be disregarded, but this will probably not mean that there is a sudden general change at a specific measure of irradiation. We're more likely to have to think that, when we go from high to low quantities of irradiation, the risk–irradiation curve will have different slopes for different types of effect. When we're dealing with all of the different harmful effects in a population, we therefore encounter a risk that gradually falls and reaches zero at a dose or dose rate which may not be far from the natural radiation level. It may therefore be necessary, as is usual for other types of protection work, to accept a small frequency of harmful effects, the size of which must be determined on the basis of what we gain from the use or the production of radiation. When the uncertainty regarding the risks from ionising radiation has been dispelled, it will be down to the international and national authorities to decide which risks can be seen as acceptable.
>
> So, the most urgent radiation protection research assignment appears to be to gather information so that the risks of harmful effects on people under the existing conditions of exposure to ionising radiation can be evaluated. This is what can be said to constitute the *main problem*.

In 1958–1960, Sievert was Chair of UNSCEAR, which then held its 6th and 7th sessions and began to prepare its second comprehensive report which was published in 1962. Sixteen scientific secretaries, including Rolf Björnerstedt, *Roy Ellis* (1925–1981), *L. D. Hamilton*, D. *W. Keam*, Lars-Eric Larsson and *Francesco Sella*, were at the Secretariat in New York for this work under the leadership of Appleyard, although not all at the same time.



Bo Lindell, Dan Beninson and Ray Appleyard at an UNSCEAR meeting in 1960. Photo: Unknown.

In the summers of 1959 and 1960, the meetings were held at the ILO in Geneva to draw up a Convention for the protection of employees against ionising radiation. The meetings were held as part of the big international labour conference, which is the ILO's decision-making body. It is held every summer and attended by a large Swedish delegation which, in the relevant years, was led by the Minister for Health and Social Affairs Torsten Nilsson, the former Director General of the Swedish Agency for Public Management *Wilhelm Björck* (1888– 1975) and the Secretary of State for the Ministry of Health and Social Affairs *Ernst Michanek* (1919–2007). The Chair of the Swedish Trade Union Confederation *Arne Geijer* (1910–1979) was also in Geneva. The Chair of the Committee who would draw up the Convention was Henri Jammet. Sievert had been asked by Chief Administrative Officer *Sten-Eric Heinrici* (1910– 1990), who was Secretary of the Swedish ILO Committee, whether he could provide a Swedish expert, and Sievert proposed me. And this is how I, along with the then Deputy Assistant *Lars-Åke Åström* (1924–2006) and the Ministry of Health and Social Affairs' Legal Director, Judge of Appeal *Liss Granqvist* (1912–1987), ended up flying to Geneva one day at the start of June 1959. I was to act as the Swedish government's expert. At the lunch that the Swedish Embassy later held for the ILO delegation, the ladies who had laid the table emphasised that this was why I had been given a red serviette.

The Committee Convention met according to the ILO's practice in a three-party conference. This meant that in the large meeting premises there were four long tables in parallel before the podium where the Chair sat. At the left long table from the Chair's view, next to the windows, sat the fifteen employees' representatives, dominated by a powerful American union man with good vocal resources. At the long table to the right sat the ten employers' representatives, which is where I got to know the consultant for the Swedish Employers' Association in technical and hygienic matters, *Gideon Gerhardsson* (1920–2008). Between the parties' long tables there were two long tables for the government experts who, with no fewer than thirty-five in number, constituted the majority of persons present.

The meeting was very formal. We worked with a draft drawn up by the Labour Office*. Before each discussion, written proposals were submitted for additions or changes or amendments to the proposed changes. All of these cases were voted on, whereby for the sake of fairness the employers' representatives each had 21 votes, the employees 14 votes and the government experts 6 votes. All results were carefully noted. Early on I asked Michanek whether I was free to vote according to my own judgement and was told, oh yes, that I was the one who was expert on these matters. On a couple of occasions, my stubbornness led to embarrassingly long discussions and I asked once again whether I could stand my own ground and received the same reply.

This was the first time I had been in a meeting with Henri Jammet as Chair. He could be seen to be a useless Chair who did not completely follow what was happening and did not intervene when necessary, but that was just an illusion. He actually knew exactly what he wanted and he was far from an impartial Chair. There were times when he took to formalities in order to get rid of an uncomfortable proposal, which exasperated many people.

In general, the atmosphere in 1959 was rather tense. The greatest conflicts were between the employees' representatives and representatives of the Eastern states, and neither before nor since have I taken part in meetings where physical force was actually used so that blood was shed. A total of fifteen sessions were held. The conflicts culminated when the ILO's appeals board approved the attendance of a further two employers' representatives at the eleventh session. The ten original employers' representatives then left the meeting in protest. The new representatives stood for the employers' point of view in White Russia (Belarus) and Bulgaria, and the 'free' employers' representatives thought that these representatives were in practice government representatives.

My own attitudes regarding a couple of questions aroused surprise and raised laughter. I said that I was against requirements for extended holidays for radiation workers and was reminded that Sweden was one of the few countries that did have a statutory holiday extension. But I stood my ground; there was no scientific reason for a holiday extension and nor was any such thing recommend by ICRP.

The second matter on which my view surprised people concerned a proposal that the Convention should order compliance with ICRP's recommendations. The fact that I, as Secretary of ICRP, was against this proposal was thought to be strange, but I quite simply thought it was not possible to assume that ICRP would *always* issue wise recommendations in the future, although that was what I did hope.

Among the government experts were a few whom I knew from before. One was *Scott Smith* from the US National Bureau of Standards, a confident, knowledgeable expert on radiological equipment. Another was Kristian Koren from Norway. There was also Ernest Watkinson from Canada. The United Kingdom's expert, *Frank Pickford* (1917–1984), came from the Ministry of Labour, which gave me an insight into the way in which the British often determine matters at departmental level rather than through expert authorities as in the Nordic countries.

The differences were also made clear to me as regards the application of laws in the Continental countries compared with the United Kingdom and the Nordic countries. The experts from primarily the Benelux countries and France submitted proposal for detailed regulations in a law which had never been possible to apply in practice. When Pickford, Koren and I pointed this out, the answer was that the regulations would be applied only 'when necessary'. I got the impression that this originated from the fact that compensation in these countries could never be imposed if no-one had infringed regulations in a law, and that they therefore wanted such regulations to remain available 'on the shelf'.

The Committee decided to draw up a Convention that was supplemented by a recommendation. The latter had spaces for a recommendation to comply with ICRP; the recommendations section was not as binding as the Convention. The labour conference then decided to allow the Committee to also continue its work in 1960; the work could then be completed and the ILO's Convention with regard to protection for workers against ionising radiation could be adopted at the conference.

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* The nomenclature is slightly confusing. ILO stands for International Labour Organisation. The ILO's Secretariat is called the International Labour Office.

In July 1959 I was invited by WHO to become a member of their Advisory Panel on Radiation and partly as a consequence of this, immediately thereafter, to take part in an expert meeting in Geneva to draw up advice on medical examinations of personnel dealing with sources of radiation or who are exposed to radiation in some other way as a consequence of their work. Such medical examinations, which had already started under the first Swedish radiation protection law from 1941, had been greatly emphasised in the Swedish radiation protection activity, not least due to Matts Helde's interest in the obligatory blood examinations.

However, the value of the medical examinations had started to come under scrutiny following the discussions that were held at the big radiation protection conference arranged by Sievert and de Hevesy in Stockholm in 1952. There were two types of examination. There were requirements regarding examinations of people who were to be employed in what was called 'radiological work'. These examinations were criticised because they were not thought to have been brought about to protect the employees but to ensure that the latter had no deviating blood values or other afflictions which the employer could later be accused of having caused. There were also requirements regarding regular examinations, primarily with blood tests, for the purpose of detecting any radiation injuries. Helde had certainly considered that he was able to prove a connection between blood changes and the level of radiation exposure in the work, but this was a statistical connection and not exactly something which told them something about particular individuals. People began to say that blood examinations to prove radiation risks in the work constituted a blunt instrument which could lead the people concerned into a false sense of security. Radiation risks could be shown more definitely using physical measurements.

WHO's expert group was affected by this development, but the recommendations that ended up being published by ICRP in its Publication 9 six years later would have a greater capacity for impact; paragraph 121 of said publication said:

> The assessment of health, both before and during employment, is directed towards determining whether the health of the worker is compatible with the tasks for which he is employed. The type and extent of the surveillance should be essentially the same as in general industrial medical practice and should include both pre-employment and routine examinations, the frequency of the latter being determined mainly by the individual's general health and the conditions of work.

Participants in the expert group included Katharine Williams from Harwell and Andrew McLean, who was then head of the British Atomic Energy Authority's Health and Safety Branch in Risley. Hermann Lisco from the Argonne National Laboratory and Bernard Wheatley, who was working at CERN at the time, were consultants who had the task of completing a report. Hussein Daw from IAEA was also there as an observer, and there was a French doctor, *J. Reboul* from Bordeaux, whose publications I read during my time at the UNSCEAR Secretariat. *Lowry Dobson*, head of radiation protection from WHO, participated as Secretary. However, the person with whom I had the most contact was Carlo Polvani from EURATOM's research centre in Ispra in Italy, whom I now met for the first time but about whom Failla had often had a good word to say. We went for a walk together every day from the *Hotel des Familles* on the Rue de Lausanne to Palais des Nations where the meeting was held, and discussed radiation protection and the world in general.

The substance of our work has been forgotten owing to the rapid development in the area. What I do clearly remember, however, is the way in which Hermann Lisco opened out the pages of our work document on the large meeting table, cut out paragraphs and sections of text, moved them around and inserted newly-written paragraphs, as well as stapling everything into a new order on new pages. This was the editing process before the age of the computer.

My next assignment for WHO came in spring 1960. In January, WHO's board had decided to do a study of radiation risks and radiation protection to form the basis for a report for the 13th meeting of the World Health Assembly, WHO's superior body. I was invited to carry out this study and write the report in cooperation with Lowry Dobson. For this purpose, I found myself in Geneva between 13th and 19th March and created a draft which was then analysed by five specially-appointed experts: Ray Appleyard, Merril Eisenbud, Greg Marley, Val Mayneord and Bill Pochin. I was honoured with an invitation to dinner from Dr. Dorolle and the lady accompanying me at the table was an aristocratic Indian lady who, with refined one-upmanship, explained that she had never visited the Taj Mahal despite having lived close to it and, when the champagne was served, took from her handbag a gold champagne whisk decorated with diamonds as if it were the most natural thing in the world.

From WHO's expert group on medical examinations in radiological work in Geneva in 1959. Seated at the table from the left: J. Reboul, Hussein Daw, Bo Lindell, Andrew McLean, Carlo Polvani and Hermann Lisco.
Photo: WHO

The meeting of the World Health Assembly expressed a desire for the report to be more widespread. It was therefore published in 1961 under the title *Ionising Radiation and Health* as number 6 of WHO's Public Health Papers with me and Dobson as the authors (Lindell, 1961). In his review of the work, Professor Bacq was kind enough to say that it afforded the authors '*grand honneur*'.

The OEEC (Organisation for European Economic Co-operation) had been formed in 1948 to administer the Marshall plan. When this task had been completed there was still a need for cooperation, and in 1961, 20 first-world western countries formed the OECD with its headquarters in Paris. After 1961, the OECD was strengthened with several non-European members like Japan (1964), Australia (1971) and New Zealand (1973). At the end of the 1950s, the OEEC began to be increasingly active where radiation protection matters were concerned. When the OEEC formed the ENEA* (European

* ENEA was superseded in 1972 by the NEA (the OECD's Nuclear Energy Agency) when Japan had become a member and the organisation was no longer fully European.

Nuclear Energy Agency) in 1958, the Committee was set up there and soon became known as the Committee on Radiation Protection and Public Health. Despite its length, the English acronym 'CRPPH' can easily be pronounced with enthusiasm to humorous effect The CRPPH, where Sweden was initially represented by Arne Hedgran, lost no time in becoming active. Too active in the opinion of a few who were worried about double work when the Committee's energetic little secretary Emile Wallauschek was eager to take on work assignments which the critics thought that IAEA and other organisations already had on their programmes. At its meeting on 16 April 1959, the ENEA was already discussing the need for cooperation between the OEEC countries as regards collecting and exchanging measurement results of the radiation and the radioactivity in the environment. On 12 June, the matter was discussed at the CRPPH and on 28 August the Secretariat sent out a compilation of what was known about the scope of ongoing measurements. In October 1959, the ENEA finally established a system for the exchange of measurement data.

On 12 June 1959, the ENEA adopted radiation protection standards based on ICRP's recommendations. These standards were revised on 18 December 1962 and were published in a booklet entitled 'Radiation Protection Standards' in 1963. They followed ICRP's recommendations regarding a dose limit which, for employees, was described by the formula $D_{max} = 5 * (N - 18)$ where D_{max} was the total radiation dose, expressed in rem, which had accumulated at the age of N years. The dose could not exceed 15 rem in any one year. Most of the leaflet was taken up by tables of the MPC values and other data for over two hundred radioactive nuclides taken from ICRP's Publication 2.

However, the CRPPH did not issue publications of the type that later aroused widespread interest until the 1970s. The first of these was published in 1970 but was initiated within the period of time which I am dealing with in this book. In 1965, IAEA, WHO and the ENEA reached an agreement regarding a study of the radiation protection problems with radioactive consumer items. The three organisations engaged *Richard Cunningham* from the American Atomic Energy Commission's Division of Materials Licensing as a consultant. Cunningham carried out a review, the result of which he reported to his employers in June 1967. He found no worrying radiation protection problems for the moment but it was considered appropriate to draw up international recommendations because a rapid increase in the number of radioactive consumer items was to be anticipated. Since IAEA and WHO did not have the option of getting involved in such work over the next few years, they agreed that the ENEA should take on the task. The ENEA set up a work group with E. E. Smith from what was the British Radiological Protection Service as Chair. The group drew up a very valuable report which was published in 1970 under the long title of *Basic approach for safety analysis and control of products containing radionuclides and available to the general public*. Thanks to this initiative, they succeeded in intercepting an unnecessary flow of radioactive consumer items and limiting the use of not inconsiderable radioactive sources of radiation for items that are meant to save lives (fire detectors, smoke detectors, luminous exit signs, etc.).

One consumer item which worried the radiation protection authorities at an early stage was radioactive wrist watches and alarm clocks – the radioactive aspect was the luminous paint which was on the hands and dials so that you could see the time in the dark. In order for it to be able to shine in darkness for a long time, the luminous paint had to be supplied with energy, and this initially came through the addition of radium to the luminous paint. This led primarily to substantial radiation risks for the personnel who were dealing with the luminous paint, which is something that I have described in detail in 'Pandora's Box'. When artificial radioactive substances became available, people tried to replace the radium with strontium-90. This was jumping out of the frying pan into the fire; the penetrating beta radiation of strontium led to risks for both the personnel and the users. The use of tritium started later, which led to risks only if tritium gas leaked out.

In 1960, a colleague, *Per Åke Wiberg*, and I had the opportunity to take radiation measurements from wristwatches worn by the visitors at an exhibition in Stockholm. We found that some types of watch, such as sports watches that could be used while diving, radiated a great deal more, probably owing to high activities of strontium-90. Such watches were simply unsuitable for wearing. At this time, the watchmakers also had other disturbing plans up their sleeve. It was thought that the energy from the radioactive substances could be sufficient to keep the watches going so that radioactive preparations could replace batteries. The watchmakers had made a mental note of the dose limits recommended to the public by ICRP and thought they could dimension the activity of the radioactive substance so that the wearer received a radiation dose that was just under the dose limit. This was of course a very ignorant interpretation of ICRP's recommendations - the recommended dose limit did after all apply to the sum of all dose contributions to which a person could be exposed, not each individual dose contribution. This mistake gave ICRP grounds to give special *source-related* recommendations in the future. The ENEA's health and safety committee discussed the matter in 1962. Luckily, a bit of convincing, training and, later on, the ENEA's recommendations in particular, led to the demise of these risky subspecies of luminous dial.

In 1960, the Nordic Society of Medical Radiology held its 23rd Congress, this time in Bergen with *S. Bakke* as President. The history of the Society (Unné, 1984) says that 'this Congress was one of the first at which megavoltage therapy was discussed in more detail. There were now up to 8 years of experience to build on and results were announced at 9 of the 20 lectures. The rapid development of chemotherapy and of the radioactive isotopes was reflected in 8 lectures'. It was also said that 'R. Thoraeus gave a valuable overview of radioactive caesium as a source of gamma radiation in radiotherapy'.

The first national organisation for medical physics was the British HPA (Hospital Physics Association) which used the journal *Physics in Medicine and Biology* as of 1956. The HPA did not just have British members – there were also radiation protectionists from Australia, Canada, Sweden and the USA. The HPA arranged visits to other countries and took the initiative for the initial discussions regarding the formation of an international society of medical physicists. In 1959, the HPA organised a special one-day meeting concerning medical physics at the end of the 9th international Congress of Radiology in Munich (see Boag, 1960). There, the HPA was asked to appoint an international contact committee for discussions with the national societies regarding a merger.

At the International Biophysics Congress which was held in Stockholm in August 1961, an international organisation for 'pure and applied biophysics' (the IOPAB) was formed with the Swede *Arne Engström* (1920–1996) as Chair.

A Contact Committee for Medical Physics meeting was held at the same Congress, which was chaired by the Chair of the HPA, Professor Len Lamerton. It was agreed that an international organisation for medical physics ought to be formed and that it ought to be independent rather than a committee that came under the IOPAB. A steering committee was set up with Sven Benner as Chair, Len Lamerton as Deputy Chair, and *John Mallard* (Secretary of the HPA) as Secretary. The Committee met on 26 August 1962 at the International Congress of Radiology in Montreal with Benner as Chair and *John Greening* as Secretary. They agreed to form an International Organisation for Medical Physics (the IOMP).

The organisation was formed on 1 January 1963 with four national members: Canada, the United Kingdom, Sweden and the USA. For the present, the steering committee functionaries were asked to act as the board of the new organisation. Its first conference was held on 8–10 September 1965 in Harrogate (south of Leeds) in England with Professor Mayneord as President. A further five members were then added. Harrogate was where the IOMP's statutes were established and the IOMP's first functionaries were appointed: Val Mayneord (Chair), John Laughlin (Deputy Chair) and Berndt Waldeskog* (Secretary). The IOMP now has more than fifty affiliated national societies.

On 23 April 1960, a heritage festival was arranged for Gino Failla at the Columbia University Faculty Club because he was retiring after having been a scientist for forty years. He was in the process of moving to Chicago at the time, where his wife Patricia would have better research options and where he obtained a position as 'emeritus in physics' at the Argonne National Laboratory. The considerate Sievert handed over a silver goblet. At the festival, a song was sung which ended with:

* Berndt Waldeskog was Assistant Professor in physics and medical physicist at the Malmö General Hospital ('MAS') since 1958.

We hope you're good for forty more
(Unless you find it just a bore)
And so we gather here tonight
To bring you honour and delight.

The further forty years never came to pass. On 15 December 1961, Failla died in a car accident in Chicago. A lady collided with a car which was driven by John Rose. Her car drove into the right side of the car, which is where Failla was sitting by Rose's side.

In June 1960, Ambassador *Sven Allard* (1896–1975) reported from Vienna to the Ministry for Foreign Affairs that an expert conference would be held in Vienna in the autumn concerning Basic Safety Standards. Allard referred to a letter from IAEA Director General Sterling Cole:

> Mr Cole says that he would appreciate it if a Swedish expert could be appointed to participate in the conference. Travel expenses and a daytime subsistence of 20 dollars are contested by IAEA.
>
> In a letter delivered to me by hand on the 10th of the current month [June 1960], which is also enclosed with the copy, Mr. Cole emphasises the great importance of the conference and states the qualifications that in his opinion the participants should possess. Mr. Cole then mentions a Dr. D. Beninson, whom he apparently considers to be a very suitable Swedish representative.

Cole had written the following in the enclosed letter:

> The experts should therefore be familiar with the control of radiation safety at high management or governmental levels. In this connection, I would venture to suggest the name of Dr. D. Beninson, whose reputation in the field is eminent.

The cooperation between me and Dan Beninson in Argentina was clearly so well-known that people could not tell us apart. However, Sweden ended up being represented by neither Beninson nor me, but by Arne Hedgran. However, Beninson *was* there in the capacity that he should have been – representing Argentina. In August, Sievert also wrote to Cole and announced Lars-Eric Larsson as representative of ICRP in the IAEA expert group. John Dunster represented the United Kingdom and Lauriston Taylor the USA.

Chair of the expert group was Professor Louis Bugnard, head of the French *Institut National d'Hygiène*. He had a difficult task because several of the experts were very articulate and enthusiastic about debating. John Dunster was one man in particular who founded his reputation as the Chair of the meeting's nightmare in this respect. Regularly and without concern he broke the rule that nobody took to the floor until having requested to do so – and then having been invited to. When the IAEA Basic Safety Standards were due to be reviewed, India was represented by *P. Krishnamoorthy* from the Indian Atomic Energy Commission. The discussions between Dunster and Krishnamoorthy have gone down in history. They scarcely gave anyone else the chance to get a word in. In the end, Bugnard grew tired of this and said (with reservations regarding the numbers): 'I've taken the minutes. Mr. Dunster, you've taken the floor 148 times and Mr. Krishnamoorthy, you've done so 137 times. That's enough. You've both spoken for quite long enough!'

The IAEA Basic Safety Standards were published in 1962 as number 9 of the IAEA Safety Series. Like the OECD/NEA's subsequent 'Radiation Protection Standards', the content was based on ICRP's Publications 1 and 2. This meant that the accumulated dose (expressed in rem) for employees up to the age of N years was limited to the formula 5 * (N – 18). A large part of the document was taken up by tables containing the MPC values and other data for 236 radioactive nuclides. The revised version, which was published in 1967, contained data on a further ten nuclides.

The intended application of the IAEA Basic Safety Standards was described in the introduction to the document in a sentence which consisted of no fewer than 141 words in English. I reproduce it here:

> Under Article III. A. 6 of its Statute the Agency is authorized to establish or adopt, in consultation and, where appropriate, in collaboration with the competent organs of

> the United Nations and with the specialized agencies concerned, standards of safety for protection of health and minimization of danger to life and property (including such standards for labour conditions), and to provide for the application of these standards to its own operations as well as to the operations making use of materials, services equipment, facilities and information made available by the Agency or at its request or under its control or supervision; and to provide for the application of these standards, at the request of the parties, to operations under any bilateral or multilateral arrangement, or, at the request of a State, to any of that State's activities in the field of atomic energy.

In other words, intention was for the provisions in the IAEA Basic Safety Standards to be applied to IAEA's own activities and to activities at the request or with the support of IAEA, and for them to serve as standards within the atomic energy field for the States who requested this.

Sterling Cole would step down in 1961 and it was necessary to find a successor who was acceptable to the Americans and the Russians. This meant, as always, that they were looking for someone who came from a small, neutral country. The Nordic countries were therefore relevant, but there were also candidates from elsewhere, such as the former Indonesian delegate. One Swede who could be appropriate was the MD of *Atombolaget*, Harry Brynielsson, who was also on the IAEA Board of Governors. However, the Russians were averse to Brynielsson because he had led an investigation into the disposal of radioactive waste in the sea. On 25 January 1961, the then Embassy Counsellor *Harald Edelstam* (1913–1989) wrote from the Swedish Embassy in Vienna to 'His Excellency, the Minister for Foreign Affairs':

> The head of A/B Atomenergi, civil engineer Harry Brynielsson, has often been suggested as the person to succeed the Director General of the International Atomic Energy Commission in Vienna, the American Sterling Cole, who is thought will step down this autumn. He is believed to be an acceptable candidate to all of the Member States, including the Soviet Union.
>
> According to my experience in connection with the ongoing Council meeting, 'Pravda' published an article by the newspaper's Viennese correspondent on the 19th of this month in which Brynielsson's report from the expert meeting in Monaco in November 1959 concerning the question of discharging radioactive waste into the sea was strongly criticised. As we know, the Soviet Union's IAEA delegate has constantly criticised this method of disposing of radioactive waste but, as far as I am aware, has not attacked Brynielsson in his capacity as expert in these matters until now. […]
>
> I respectfully enclose an unofficial translation into English of 'Pravda's' article of the 19th of this month.

The heading of the article in 'Pravda' was 'Poisoners of Wells and their Accomplices'. It read as follows:

> Despite the stringent measures that have always been taken everywhere against well poisoners, some of these poisoners do still remain.
>
> The old method of releasing toxin into wells is child's play compared to what is being done by the modern 'well poisoners' – the American and British monopolists who use the open seas to dispose of radioactive waste from their nuclear facilities.
>
> American and British monopolists who are still going about this dirty and dangerous method are now attempting to give some sort of legality to their goings-on. Through their people in the Secretariat of IAEA, they want to get us to use the expert panel set up by IAEA to draw up a draft Convention which favours the poisoning of seas and oceans. In order to conceal this terrible plan, the panel has received a vindicating document – the Brynielsson report.
>
> Hammarskjöld's compatriot Brynielsson is trying to prove that which cannot be proven – that the disposal of radioactive waste in the sea is not dangerous. In his report, Brynielsson deliberately conceals the fact that kills all of his 'scientific' conclusions, and this fact is that today, as a result of the nuclear weapons tests, the radioactivity in the Pacific Ocean has already almost reached the limit of the permissible level.

> Brynielsson's report is a document that has been written under order from the nuclear monopoly in the USA and the UK. No wonder its author is destined for the post of Director General of IAEA. […]

The Soviet attack on Brynielsson meant that it was not possible for him to be a candidate. However, there was another Swede who was well qualified for the task, i.e., Sigvard Eklund, who had been Secretary General of the second Atoms for Peace Conference in Geneva in 1958 and had led the construction of the first Swedish reactor, 'R1', on Drottning Kristinas väg in Stockholm. Eklund thus had unusually solid specialist knowledge but was also a great diplomat, which meant that all the superpowers finally accepted him, although the Soviet Union initially also accused him of being a 'western agent'. The Swedish government's attitude to Eklund was not as enthusiastic, which is shown by *Lennart Petri's* memoirs. Petri was Ambassador in Vienna from 1969–1976 and a close friend of Eklund. Those in the Government building had not forgotten Eklund's negative attitude to the Marviken project.

Sigvard Eklund started as Director General of IAEA in 1961 and remained in that post until 1981. His knowledge and diplomacy meant that he commanded substantial respect but at the same time found himself really isolated at the top of a complicated hierarchy. On the occasions that I visited him in the old building on Kärntner Ring, the personnel who were on a comparable level to mine viewed me in the same sort of way that Japanese look up to someone who has met the Emperor. Eklund invited me to lunch several times. He had his own dining room high up in the building and was rarely seen in the personnel refectory. The two of us sat at a large table and were waited on by a *chef de cuisine*, and I thought about the days when he had sat there on his own. The arrangement was not result of enormous pride on the part of the modest Eklund - he had inherited it and appeared to be a prisoner of the system. But although he was pretty powerless when it came to IAEA's bureaucracy, Eklund was efficient where foreign policy matters were concerned. This primarily concerned the control of nuclear weapons development. Eklund contributed to the 1963 test ban treaty and until 1968 the Non-Proliferation Treaty (NPT), as well as the Safeguard Agreement which entitles IAEA to inspect civil nuclear energy facilities to check that fissile material is used for peaceful purposes only.

On 5–9 September 1960, the UN (through UNSCEAR) and WHO arranged a common seminar on population statistics in Geneva. I had prepared a lecture for this on the conditions to be able to show small increases in risk on the basis of the random variations in the 'natural' presence of the injuries concerned. If the frequency of the injuries studied, such as death from cancer, follows a Poisson distribution (see Chapter 13), the standard deviation, sigma (σ), is equal to the square root of the number of cases (N), i.e., $\sigma = \sqrt{N}$. If you repeat an observation of an average number of N, approximately 2/3 of the outcome will be between $N - \sqrt{N}$ and $N + \sqrt{N}$. The values that deviate are thus 'not ordinary'.

If we use 3σ as a distribution measurement instead, depending on the size of N (if N is greater than 25), between 99.2 % and 99.9 % of the outcome will be between $N - 3\sqrt{N}$ and $N + 3\sqrt{N}$. The values that deviate further can therefore be said to be 'improbable'. Therefore, if we study a situation where, without any extra radiation risk, we could expect N deaths from cancer during a given period but wish to investigate whether an outcome greater than N really does involve a greater risk, the outcome must be substantial enough for the result to be improbable if no radiation risk is found. With our definition of 'improbable', this requires the outcome to be greater than $N + 3\sqrt{N}$ or else it could very well be a result of the random variation of N from time to time.

If you know the expected number of cases of cancer in a given population for a specific period following a known radiation dose, i.e., the risk coefficient (or assume a specific value for it), you can calculate the minimum radiation dose required for a sufficient increase of N for it to be possible to observe the increase. On this basis, I concluded that the only group of irradiated people who could then be expected to fulfil the terms to confirm or refute the assumed risk coefficient (which I then assumed to be a 0.007 % per rem* risk of dying of cancer) was the adolescents who had been subject to x-ray examinations using fluoroscopy on a yearly basis in France for a long period of time. Unfortunately, as far as I am aware, no such survey has yet taken place.

The seminar in Geneva was where I met Alice Stewart for the first time, a small, white-haired card of a lady. Her tendency to get involved in conflicts made her many enemies. I was sceptical some of her research results; she was often too eager to gain support for her theories. Sometimes she *was* right, despite resistance and scepticism. However, when it was my turn to come into conflict with her, which was not difficult, she was surprisingly friendly and I could not help but like her in spite of everything.

UNSCEAR held its 8th session on 19–30 September 1960, this time not in New York but in Geneva.

This had been preceded by discussions regarding the suitability of holding all the meetings in New York. The Czechs had invited the Committee to meet in Prague, but this was not thought to be politically possible, primarily because it would open the door to forthcoming invitations which could be difficult to reject if the invitation to Prague were accepted. Appleyard also emphasised that the expenses for the Secretariat would be very high if they met outside the UN's headquarters. Instead, the result of these discussions was that the Committee should alternate between meeting in New York and Geneva because *Palais des Nations* could be counted as the UN's European headquarters.

And so it came to pass that in 1960, the Swedish delegation travelled to Geneva and checked into *Hotel d'Allèves* for a couple of weeks. The completely male-dominated Committee now had a female member. Formally speaking, Belgium was a member but the Belgian delegation which was initially led by Professor Bacq had subsequently been filled with Dutch people. One of these was *Dr. Zwanette Nooteboom-Beekman*, 'Nettie' for the sake of simplicity, who gave us Swedes enough praise for us to accept her as an equal colleague rather than just a lady in the way her compatriots did.

In January 1961, I received a letter from Ray Appleyard who said that he had left UNSCEAR to work with the EC's research administration in Brussels. He was succeeded by one of his Scientific Secretaries, Francesco Sella, an aristocrat from the north of Italy. In March that same year, Lars-Eric Larsson came to New York to take up a post at the UNSCEAR Secretariat as Scientific Secretary for a year, with the same assignment as I had had – to compile an Appendix of medical irradiation. He wrote to me after three weeks. He began by thanking me for my letter and continued with: 'It was an obliging yet cutting comment you wrote about the Agency's proposal. However, it was fair […]'

Lars-Eric was actually referring to a letter that I had written to Hussein Daw at IAEA's radiation protection department containing points of view on a work document that Daw had sent me for comments. I had become angry about a number of elementary errors, among them the fact that there was no differential dx in formulae with integrals $\int f(x)\, dx$. I did not think you should be showing off using integrals if you did not understand what you were doing. What I did not know was that letters to officials at IAEA were copied and sent to a number of other people for information. This meant that I laid the good-natured Daw open to unintended ignominy and humiliation, which saddened me a great deal.

Lars-Eric complained about the prevailing lack of organisation at the UNSCEAR Secretariat once Appleyard had left and Sella had not yet taken his place (he was to come in May). In the meantime, the Secretariat was 'directed' by the '38th floor', i.e., that of the General Secretary, where the efficient *Brian Urquhart* (1919–), assistant to Ralph Bunche at the time, took care of most things.† Lars-Eric's letter continued:

> I am currently going through the gonadal dose material. Unfortunately, most of the publications contain errors or ambiguities so I am now waiting for answers from various places. Unfortunately there are no staff meetings as yet; the work continues as per the

* With the current unit (sievert, abbreviated to Sv), this is 0.7 % per Sv. In ICRP Publication 26 (1977), the likelihood of dying from cancer was still given as approx. 1 % per Sv. With the arrival of new information following long latency times, ICRP stated in Publication 60 in 1990 that the risk of dying from cancer was approx. 5 % per Sv

† Brian Urquhart, 'Mr UN', worked in different posts at the UN from 1945 to 1986, latterly as Under-Secretary-General for Special Political Affairs. He had a substantial influence.

cloister principle – each person in his own small cell. My cell looks out onto the East River.

At the moment, 17–21 April, a joint symposium on dosimetry is taking place in Geneva. The cooperating parties are ICRU, WHO and IAEA. Kurt Lidén and Martin Lindgren are there from Sweden and Dick Chamberlain, Braestrup and Laughlin among others from the USA, as are Tubiana, Cohen, Meredith and a few others. The Secretariat here was invited to send a representative at the last minute. The undersigned was appointed but the 38th floor backed out. Personally I was pleased, but it would probably have been valuable in some way from the work point of view. However, I can get the necessary information from Laughlin when he has returned. The UN is dominated by the Congo and Cuba crises*. Many of the scribes are in the Congo.

We live in a 2-family building between Mount Vernon and Yonkers in a villa area not far from the Yonkers raceway. It is fine for us. We have the upper floor: kitchen + 4 rooms + dining area + garage for our car (1959 Chevrolet Impala). The flat costs $245 per month, including everything except for the telephone. It takes 50 minutes to 1 hour to travel from the front door to the UN's lifts.

In June 1961, IAEA arranged a symposium in Salzburg concerning whole-body measurements. 122 scientists from 27 countries and 4 international organisations took part in this very well-organised and fruitful meeting. Both Kurt Lidén and Rolf Sievert from Sweden obviously took part. As for me, I gave a lecture on the simple facility I had had built in the 'The Pit' at the Institute of Radiophysics and was able to show that it was more important to have constant background radiation than very low background radiation. The Assistant Professor in Aviation Medicine at the Royal Central Institute for Gymnastics, *Wilhelm von Döbeln* (1912–1995), was particularly interested in the possibility of determining people's muscle mass by measuring the gamma radiation from potassium-40 which, as Sievert had shown, is found predominantly in the muscles. The symposium resulted in a bibliography of literature on whole-body measurement as well as a list of whole-body counters in the IAEA Member States.

In 1961, I was asked to write a general overview, *Present-day Assessments of Radiation Hazards* in the journal called *Physics in Medicine and Biology* which was about the relevant knowledge of the radiation risks (Lindell, 1961b). Joseph Rotblat was the editor of the journal at the time. The overview was well received and I had a number of appreciative letters. Unfortunately, I had made a banal calculation error when estimating the risk of hereditary injuries, which embarrassed me.

UNSCEAR met twice in 1961, in March and September (the 9th and 10th sessions); the 1962 report was then drawn up, the second one from the Committee. Sievert was the Swedish representative as usual but was now no longer the Chair. At the Geneva meeting in March, Caspersson was the substitute representative and at the New York meeting in September, the task was shared by Arne Nelson and me. As usual, the delegation included Bo Aler but now also K-G. Lüning as geneticist and Lars Fredriksson as agricultural expert. The Committee had found its work method. Apart from a few of the plenary meetings, it worked divided into two sub-groups, one biological and one physical. The biological group soon sprouted a genetic sub-sub-group where discussions tended to be held amongst themselves.

One major topic of discussion was the absorption of strontium-90 and caesium-137 from the ground by grazing cattle and also in the food chain. There were three big experts in the area, one of them being Fredriksson. The other two were Dr. Scott Russell, who was head of the British agricultural research's radiation biology laboratory in Letcombe Regis outside Wantage, just west of Harwell, and *Dr. C. L. Comar* (1914–1979) from Cornell University, Ithaca in New York. Scott Russell was the person who dominated the discussions with a loud voice and superior, drawn-out Oxford English, but Lars Fredriksson made himself heard during the debate.

* The difficult Cuba crisis which so nearly caused a third world war actually took place in October 1962.

Rolf Sievert with his indispensable cigar at a UNSCEAR meeting in the 1960s. The order of the delegates around the table is controlled by the English names of the States. From the left: Richard Chamberlain (USA), 'Bill' Pochin (United Kingdom), M. El-Kharadly (United Arab Republic), A. M. Kuzin (USSR), Rolf Sievert (Sweden), M. Martinez-Báez (Mexico). Bo Lindell is seated opposite Sievert. Photo: The United Nations.

Arne Nelson was an indefatigable soul. In our free time, he went with us other Swedes (Sievert was not one of them) to one art museum after the other (he was actually related to the prematurely-deceased painter *Olof Sager-Nelson* and did some painting himself). Arne had a strange ability to never appear harassed but to always have time for everything. If someone called him at the FOA, he almost always answered the telephone himself. In New York, when we others went direct from the hotel to the UN in the morning, he managed to get off to the Swedish Embassy or buy records on the way there, all without being in a hurry. Whereas we others simply observed that something needed to be done and put it to the back of our minds for the future, Arne immediately did what needed to be done, wasting not one minute of his time. Since this, when I have mismanaged my time and have not managed to get done what I should, I have thought of Arne and felt ashamed.

In 1958–1964, *Agda Rössel* (1910–2001) was the Swedish UN Ambassador in New York. In addition to her unique qualifications as a diplomat and appreciated achievements within the Swedish Foreign Service, she had an obvious interest in Swedish delegations obtaining practical support for different UN assignments. During the UNSCEAR meetings we were greeted at our hotel by boxes of all sorts of useful equipment such as corkscrews, bottle openers, cutlery and other things to facilitate self-catering. We were invited to dinner in her home on several occasions. Agda's daughter helped to serve us on one such occasion. It was funny to see how the sophisticated diplomat was anxious to make a good impression on

Sievert, whose international greatness had obviously made an impression on her. She was unexpectedly nervous about serving and made sure it was done according to all the rules. At the same time, Sievert and his wife were equally as nervous and anxious to behave correctly in front of the Swedish ambassador. So, two big personalities were quite unnecessarily equally anxious about not living up to the expectations they thought were held of them.

On one of the trips to New York I took the opportunity to visit my previous landlady from the year when we had lived in White Plains. I found her very disturbed. She was a Canadian citizen but would soon be retiring from her job as a teacher in Chappaqua just to the north. However, in order to be able to receive a proper pension, she had to be an American citizen when she retired. She had therefore applied for citizenship. Some time after doing so she noticed a car parked down the hill below her villa with two men in the front seats. This was not a temporary thing; the car sat there day after day. After a few weeks the doorbell rang. There stood the two men, who said they were representatives of an opinion poll institute. My landlady stared at them and said: 'Aren't you the people who've been sitting in a car down there for three weeks? What's that all about??'

The two men were embarrassed. 'You noticed then?' they said sulkily. It turned out that they were from the FBI. My landlady gave immigrants English lessons in the evenings. The neighbours had called the police and said that mysterious foreigners were coming to visit her. The men from the FBI said that this had to be investigated before she could obtain citizenship. 'You'd have saved time by asking me,' she said, then adding, 'you're wasting my taxpayer's money!'

After the UNSCEAR meeting in 1961 while I was still in New York, ICRP's Committee II met in the Union Carbide and Carbon premises in Manhattan, and I took part in the meeting because I was Secretary of ICRP. However, rather than one of ICRP's problems, the most noteworthy thing was a shocking news item on 18 September. Karl Morgan came to me after lunch and said: 'Mr. Hammarskjöld died yesterday.' 'DAG DIES' made big headlines in the newspapers. This was of course a great tragedy and loss for the UN, but I realised that Hammarskjöld was not the only one to have died. The day before his departure, I had eaten lunch in the UN's personnel refectory with Marrit and Karin, and someone had pointed out a powerful man sitting at a table further away. We were told that that was Dag Hammarskjöld's bodyguard. And now he was also dead, which somehow really brought the fact home.

In December 1961, the FAO (the Food and Agriculture Organisation of the United Nations)* held a seminar in Scheveningen in the Netherlands about radiation following emissions and accidents. This was another of the early international meetings which led to valuable contacts. I remember that the Swedish participants included *Stig O. W. Bergström* from Studsvik and the foreign ones included Charles Dunham, head of the AEC's Department for Biology and Medicine. Nettie invited me to an Indonesian dinner (more about this meeting in Chapter 16).

In June 1962 I received an unusual letter, unusual in so much as it came from the Dean of the theological faculty at the University of Chicago. The letter was also signed by the person in charge of the pharmacological institution's nuclear medicine department, Professor *John Rust*. It was an invitation to take part in a conference on radiation and social ethics in January 1963. The purpose of the conference was described as follows:

> The purpose of this conference is to bring together a small group of prominent theologists from the Judeo-Christian tradition (from east and west) with a small group of prominent scientists to tell each other their special views on the whole of this matter. The social ethic aspects of radiation ought to be publicly discussed by responsible people not only in the form of faith but also in the form of knowledge. We have convened this conference to achieve a deeper understanding of both the technical and the moral dimensions of the radiation issue.

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* The FAO was formed in Quebec in October 1945 and its headquarters are in Rome. The objective of the FAO is to achieve a higher standard of nutrition and living in the world and to improve the production and distribution of foods and other products from agriculture, forestry and fish farming. The highest governing body is the conference of 158 members, and the executive body is the council of 49 Member States.

I accepted the invitation out of pure curiosity and had a feeling that I had received it because I was Secretary of ICRP. The 'little group of prominent theologists' turned out to consist of seventeen very representative people while the group of scientists, in addition to myself, amounted to only four, whom I had not previously known, although I *had* heard the name of one of them mentioned, population geneticist *Adriano Buzzati-Traverso*, who managed an international laboratory for genetics and biophysics at the University of Naples.

The meeting was to be held in the University of Chicago's new Centre for Continuing Education on 16–18 January 1963. It was a very cold January with temperatures down to minus thirty degrees, and we were all grateful that the new centre had space for accommodation and discussions and meals so we did not have to go out into the biting cold.

The participating theologians included the well-known scholar of religion *Paul Tillich* (1886–1965), Bishop of Exeter *Robert Cecil Mortimer* (1902–1976), Bishop of Hannover *Hanns Lilje*, a bearded, Greek Orthodox Professor of Theology, a Jewish Rabbi and a few Catholic priests who were involved in writing an encyclopaedia of the Catholic faith.

We who were to be questioned by the theologians put forward proposals for ethical problems. Nuclear war was an obvious example. Another was the possibility of cloning people; a future dictator might be able to procure a police force or an army of cloned, very muscular minions with an instinct for discipline. But the theologians mercifully shook their heads and said that we scientists cared way too much about people; the important thing was the soul. Everything that could be done was facilitated through God's will. If He wanted cloning and nuclear war, He wanted it for a purpose.

India may be able to procure nuclear weapons if China becomes a threat, said the Indian theologian. In his opinion, atom bombs were not always for evil purposes. The Greek Orthodox Professor of Theology pointed out that his eastern orthodoxy had never forbidden or condemned scientific research and had never intervened in research laboratories or adopted a negative attitude to new scientific doctrines even though they currently appeared to be in conflict with the Christian faith. He said that one had to see the glory of God in nuclear fission, but at the same time the wickedness of mankind in creating a threat to peace from this glory.

In 1962, ICRP had stated that the grounds for the Commission's recommendations were that each exposure to radiation could involve a risk and that the likelihood of harmful effects at low doses of radiation was in proportion to the size of the dose. The 'risk philosophy' of radiation protection therefore involved attempting to find a balance between risk and usefulness. My own contribution to the conference was entitled 'The task of balancing unknown quantities' in an attempt to emphasise the substantial uncertainty. But, as several of the theologians maintained, the very fact that we live with risks is a natural phenomenon. God himself took a risk when he created man, as Professor Tillich pointed out.

The Dominican Father *William Wallace*, who was editor of the philosophical section of the new Catholic encyclopaedia, thought that one ought to differentiate between moral philosophy and moral theology. A moral philosopher can discuss the radiation problems on purely rational grounds without reference to holy documents or theological tradition, he said. On the other hand, the moral theologian has to supplement the rational analysis with reference to theological teachers. It is irrefutable, continued Wallace, that radiation can cause harm, but does that make all the radiation to which people are exposed evil? The answer from the theologian may well differ from the answer from the philosopher. The latter may think that all radiation leads to a measure of evil and that the radiation from naturally-occurring sources of radiation also contributes to that evil. On the other hand, thought Wallace, the theologian relies on God's foresight and cannot see the natural radiation as something evil.

From that point on, Father Wallace ended up in an argument which seemed meaningless and artificial to us scientists. How far, he asked, can the natural background radiation increase before the increase in addition to the good can be seen as evil? If the annual dose from the natural radiation is 100 mrem (1 mSv), can an increase to 110 mrem then be seen as evil? And if you build a nuclear power plant in an area where the background dose is 90 mrem, can an increase to 100 mrem then be seen as evil although the dose is no higher than that which God has allowed in other areas?

Another matter that was discussed was how people could determine the defensibility of an activity that causes radiation, such as a nuclear power plant. Father Wallace had stated the following in his written contribution:

> This leads me to the final problem: how are we to assess the good that comes from the use of nuclear power in relation to the evil that affects mankind? If the good benefits only a few while the evil affects all people, are they in appropriate proportion? Or, if the good itself is questionable, is it morally defensible to sustain serious hazards to achieve it? Or is the use of nuclear power expected to bring invaluable benefits for all of mankind from in comparison with which the expected evil is insignificant or not what one would call overwhelming?

The general opinion held by the theologians appeared to be that research and technology were tools with which God has supplied mankind. They therefore cannot be evil. Paul Tillich also thought that it was a sin against the gospel to forbid something only because of conceivable negative consequences. Indian theologian *Joshua Chandran* from Bangalore said that mankind finds liberation and freedom in compelling nature and exploiting its assets. Father Wallace emphasised that it was not risks themselves that were evil, but the evil moral, i.e., when someone intentionally committed evil.

Considerably bewildered, we five scientists left the meeting while the theologians prepared their report for a big ecumenical meeting which would start the following day. We had hoped for ethical guidance from the theologians but had been left to think as we pleased; our theological friends did not seem to see any problems provided the intention was good. This Jesuit view culminated on the final day when one of the theologians, who was Pastor of a Protestant church in Oak Ridge, explained to me that he was actually Catholic. ‘What does your congregation think of that?’ I asked. ‘They don’t know about it,’ he answered,’ but I probably will eventually succeed in guiding my herd towards the Holy Father in Rome.’

UNSCEAR approved its second comprehensive scientific report at its 11th session in March 1962. It was 442 pages and contained eight scientific Appendices. The report dealt with somatic and hereditary effects of radiation. The Appendix on radioactive environmental contaminants discussed the occurrence and spreading of radioactive substances from nuclear weapons testing, the transportation of these substances through the food chains to human beings and the resulting doses of radiation. It also discussed the handling of radioactive waste and radioactive emissions, including accidents, from nuclear reactors. One Appendix reported doses of radiation for ‘radiation workers’ and patients who had been examined using or treated with ionising radiation.

One Appendix in the UNSCEAR report discussed comparisons of doses of radiation as well as risk assessments. It is interesting to see what the perception of the risks at low doses of radiation in 1962 was. They wrote:

> […] For genetic effects experimental data justify the use of a linear dose relationship at low doses and dose rates. No such generalization can be made about late somatic effects of radiation. In radiation carcinogenesis at high dose levels many different mechanisms may play a part, including various kinds of interactions between damaged cells and tissues, effects of vascular and hormonal changes, as well as specific radiation-induced changes in cells. Also there may be several different ways in which the same macroscopic effect can be brought about […]
>
> One would expect, however, that the mechanisms of production of any late effects are simpler at lower doses because interactions between damaged cells, as well as general systemic effects of radiation, will play a smaller part. […]
>
> For certain radiobiological effects which have a non-linear relationship at high dose levels (e.g., certain types of chromosomal change induced by radiation), it is probable that the slope of the dose-effect curve near the origin is linear. However, the range of effective linearity may be very limited. […]
>
> So far as an absolute assessment of risk is concerned, that is, an estimate of the actual number of effects from a given radiation exposure, a clear distinction must be made between the genetic and somatic problems. For radiation-induced genetic changes there is good experimental evidence that the dose-effect relationship is linear […]. For somatic effects there are no experimental data relevant to the form of the dose-effect curve at low doses and, even at high doses […], there are very few reliable dose-response data for late effects.

> To the extent that it is a matter of absolute risk assessments, i.e., estimates of the actual number of injuries from a given exposure to radiation, a clear distinction must be made between the genetic and the somatic problems. For radiation-induced genetic changes there is good experimental evidence that the dose–effect connection* is linear […]. For somatic effects there is no experimental data that is relevant to the shape of the dose–effect curve at low doses, and for that matter at high doses […] there is very little reliable dose–response data for late effects.

As a consequence of this, the Committee thought that no absolute risk calculations could be performed, only relative estimates. The latter could be done by comparing the doses of radiation from different activities with the doses from the natural radiation, which was assumed to give an annual dose of approximately 100 millirems (1 millisievert). UNSCEAR estimated that the total global dose commitment from all nuclear weapons testing together from 1954–1961 corresponded to an exposure to one and a half years' natural radiation. If the natural radiation had caused no injuries, nor should the testing of nuclear weapons. If on the other hand a year's worldwide exposure to natural radiation were to be shown to cause a certain number of instances of injury, the nuclear weapons testing would lead to one and a half times as many instances of injury.

In March 1963, the ENEA arranged a symposium on personal dosimetry in Madrid in which Sievert and *Jan Olof Snihs* (1932–) from the Institute of Radiophysics took part. Andrew McLean from the British Atomic Energy Authority in Risley held an introductory lecture. Sievert was Chair of the first session where the subject was the application of radiation protection standards and the speakers were Henri Jammet and Lauriston Taylor. Sievert described his devices for the continuous detection of the radiation during different phases of work in a radiation environment in a later session. This was the symposium at which Stig Bergström from Studsvik called Sievert 'Professor Campari' owing to his love of this drink which he had only just discovered.

In May 1963, Lars-Eric Larsson came to WHO to work with Lowry Dobson for a while. The knowledgeable Norwegian doctor and biologist *Finn Devik* (1916–1985), the Norwegian Radiation Protection Institute's consultant, was also there. Both ended up making important achievements for WHO's radiation protection activity.

On 18–22 November 1963 a seminar was held in Geneva, arranged by the FAO, IAEA and WHO. The subject was the protection of the public if an accident which led to radiation risks were to occur. The Secretariat consisted of Lowry Dobson from WHO, *G. Swindell* (who was Hussein Daw's boss at IAEA), and *G. Wortley* from the FAO in Rome. Many international organisations were represented, including ICRP with David Sowby, Euratom with Pierre Recht and the OECD's ENEA with Emile Wallauschek.

The seminar had attracted many well-known participants, who included *H. L. Gjørup* (from Risø), Per Grande, Juel Henningsen and Jörgen Koch from Denmark, Olli Castrén, Olli Paakkola and Kauno Salimäki from Finland and *Arne Bull, Lorentz Eldjarn* and *Thorleif Hvinden* from Norway, Bugnard, Jammet and Pellerin from France, Polvani from Italy, Courvoisier, Wagner and *Serge Prêtre* from Switzerland, Binks, Pochin and Scott Russell from the United Kingdom and Chadwick, Terrill and Tompkins from the USA, alongside many more who would make the list too long. There were six of us from Sweden. Alongside me were Carl-Johan Clemedson, Lars Fredriksson, *Ulf Greitz* (from the FOA), Arne Hedgran and Bertil Åberg. Sievert was not there but he had asked me to deliver his lecture which expressed points of view as to how a preparedness organisation ought to be created.

The introductory lecture was held by Juel Henningsen, who pointed out that ICRP's recommendations applied to normal situations rather than accidents. He said it might be possible to obtain guidance from other abnormal situations such as natural disasters or epidemics. Perhaps a smallpox epidemic was the

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* The Committee had not yet started to apply the more precise terminology in accordance with which 'the dose-response relationship' should have been written here rather than the 'dose–effect connection'. It is the dose–response connection that is consistently referred to in these quotes.

most obvious one to make a comparison with. In his opinion, it was all about weighing up the health risks against the risks from countermeasures.

Hedgran and I took a walk from *Palais des Nations* to our hotel and were accompanied by Eldjarn, who was disgruntled. ‘There’s too much talk,’ he said. ‘Most of them are suffering from verbal diarrhoea!’

On the last evening, Hedgran and I sat in a small restaurant very close to *Hotel d’Allèves* talking to some Americans when another one came in with a sensational piece of news. Arne has described it in a letter to me:

> Calibrating the memory capacity is interesting, almost scientific. I think I have a good memory of the evening in Geneva. Said memory tells me we were eating at a restaurant with Americans and an American came in and said that the President had been shot. We’re in agreement up until then - but I do wonder whether we might have gone to a new restaurant later – maybe to have a beer – where we met a new British group. I seem to remember the Americans having very strong feelings which contrasted with the attitude of the British. It may be that all this took place on the first occasion. On the other hand, I am sure that sometime during the evening in an open space somewhere – maybe *Place de Cornavin* – we bumped into Mr. Binks and informed him. His answer was ‘Oh Gosh!’ with the fortitude of an imperial citizen.

The information we were given was so unbelievable that we hurried the following morning to purchase *La Tribune de Genève* whose front page carried the big headline: ‘Le président Kennedy assassiné au Texas’. So the information was true.

15. SALTHOLM AND NORDIC COOPERATION

BRAVO, the large hydrogen bomb boosted with uranium, was detonated by the Americans on Bikini on 1 March (local time) in 1954. Contrary to all plans, radioactive fission product fallout contaminated the Marshall Islands of Rongelap and Rongerik as well as the Japanese fishing boat 'Lucky Dragon'. This event aroused considerable attention all over the world. In Denmark, the Danish Defence's Research Council ordered measurements of the activity of radioactive substances in rainwater. The measurements were taken by the laboratory's boss, *Johan Ambrosen*, at the radiophysics laboratory at *Radiumstationen* in Copenhagen. At the same time, the Danish Board of Health asked radiochemist *Hilde Levi* at the University's Laboratory of Zoophysiology to examine drinking water. Hilde Levi had cooperated with George de Hevesy in the 1940s with the development of methods for neutron activation analysis.

In Sweden on 15 May 1955, Rolf Sievert published a 'Message to the Public' about the results that his measurement stations had recorded regarding increased doses of radiation as a consequence of the radioactive fallout from the nuclear weapons testing. In 1950–1952, he had had access to four measurement stations, and two further stations had been added in 1953–1954. The increase in the radiation had been short-term on each occasion and the exposure rate recorded after the increase had ceased was between 1.44 and 1.46 milliröntgen per week. There had been no noteworthy increase in the weekly exposure rate before 1953, but the maximum increases for 1953 and 1954 had been 0.43 and 0.40 milliröntgen respectively in one week.

It is interesting to see that Sievert expanded on the measurement results with the help of illustrations with cubes whose volumes corresponded to the weekly doses under different circumstances: the maximum permissible for people in radiological work, the normally-occurring in the natural surroundings, and the maximum recorded from the nuclear weapons testing.

In April 1955, Sievert invited some of his Nordic colleagues, Moxnes from Norway plus Jørgen Koch and Rønne-Nielsen from Denmark to be precise, to Stockholm to discuss the forms of a more efficient cooperation. At the same time, Nordic radiophysicists, in Sweden primarily Sven Benner in Gothenburg and Kurt Lidén in Lund, had started to discuss the need for a Nordic radiophysicists' association. The two initiatives actually concerned rather different matters. Sievert was interested in top-level cooperation between responsible radiation protection managers. This cooperation could certainly include Nordic congresses but it would have to be managed from above. This was not a case of Sievert trying to hold onto power; he was already in charge and saw this as something obvious that did not need to be discussed. But he did think that the power that he and his colleagues had ought to be exercised responsibly.

Benner and Lidén were interested in cooperation between active radiophysicists in an association that was not governed from the top down and which had democratic ways of working. Sievert was not against democratic ways of working but had the good old patriarchal desire to control matters in the best interests of others. There should actually have been (and it proved to be the case that there was) space for both of the initiatives, but unfortunately their wills ended up colliding. On 17 May 1955, Benner wrote the following to Sievert:

> […] Since the Nordic radiophysicists' association did not come to fruition at the first Congress in Oslo, I have consulted other board members of our Swedish society and outlined a proposal for a radiophysicists' association which would constitute a section of the Nordic Society for Medical Radiology. The new association would be fully independent in relation to the Swedish Society, and even though many members of the

latter may be expected to be members of the former, there would be no collective membership so that the society's members would automatically be members of the new association or, vice versa, the Swedish members of the latter also needing to be members of the society.

[…]

The Nordic Society for Medical Radiology's Board of Governors will meet on Thursday 9 June at 12.00 [in Gothenburg], and it would be the right thing for us to have met before this to discuss the matter and, if possible, to arrive at a proposal to put before the Society for Medical Radiology's Board of Governors. I have therefore convened a meeting of the Swedish Society's Board of Governors on the same day at 10.00 to discuss the matter and in this connection invite you to take part in the meeting. I have also invited Moxnes and Rønne-Nielsen, and have also invited Salimäki from Helsinki to take part if he comes here (he has not yet signed up for the Congress) and, if he does not intend to go, have asked him to let me know whether there is another Finnish radiophysicist who can come. […]

Benner's invitations to Sievert's radiation protection colleagues were not well received, especially as Sievert had been in contact with Moxnes and Rønne-Nielsen but not with Salimäki. Sievert rejected the proposal fairly brusquely in a letter of 19 May:

Thank you for your letter. Unfortunately, I will probably be unable to go to the Congress in Gothenburg, to which I had to send my apologies.

With regard to a Nordic radiophysics association, I think this is something that is no longer relevant since Moxnes, Rønne-Nielsen, Koch and I established a cooperation which also includes arranging the Nordic radiophysicists' meetings. A Nordic association would probably not be able to fulfil any practical task over and above arranging such meetings, with said participants being able to express their opinions on various matters and make the statements which could prove to be desirable. I therefore do not think that doubling the Nordic cooperation through an additional organisation is justified, particularly as our preliminary fixed agreement also includes meetings in person at least twice a year and there will be plenty of opportunities to arrange radiophysics conferences on behalf of themselves or others.

If the proposed arrangement is approved by the authorities, I will contact you and Lidén. One essential advantage of the current proposal is that it will probably be possible to obtain not insubstantial funds to cover the travel expenses.

Sievert was still clearly disturbed by Benner's letter and the latter's contact with Moxnes and Rønne-Nielsen. He hurried to send them and Jørgen Koch a copy of the letter. In the accompanying letter he wrote:

Herewith copies of letters from Benner and responses to the same. As far as I am concerned, I think there are bigger ideas to arrange Nordic radiophysicists' conferences on the basis of the agreement we prepared in Stockholm, which will then automatically eliminate all formal difficulties and controversies. In addition, the cooperation we have prepared for will be ongoing such that the arranging of the radiophysics meetings will appear to be a natural part of the job.

Have spoken to Director General Engel* of our Medical Board, who was nothing but positive as regards our agreement. Due to illness, I have not yet been able to talk to the Foreign Minister and the Supreme Commander of the Swedish Armed Forces. However, it is scarcely likely that they will object to the planned cooperation. In each case, any such objection can be seen to concern only a particular point of our programme. It would

* *Arthur Engel* (1900–1996) was Director General of the Medical Board from 1952–1967.

be good if we could join together in adopting the same view regarding the Benner proposal because I fear that realising the same may lead to complications.

Here, Sievert showed that he thought that such a formal cooperation would need to be sanctioned by both the Ministry for Foreign Affairs and the Department of Defence. He doubtless predicted that any Nordic radiation protection cooperation would discuss problems with the radioactive fallout from the nuclear weapons testing. He therefore sent Minister for Foreign Affairs Östen Undén a draft of a cooperation agreement between the Nordic colleagues. In the accompanying letter he wrote 'Since I expect the proposed cooperation to be very fruitful, I hope it will win your approval'. He was too optimistic in that respect. A week later, Undén responded with a slight admonishment:

> Brother, in respect of your letter of 19 May, might I say that I had not previously heard that these negotiations had taken place and that the proposal cannot be accepted in its current form in any case. I would like to talk to you about the matter before anything else is done.
> *With best wishes,*
> *Your affectionate*
> ÖSTEN UNDÉN.

Benner answered Sievert's letter immediately. He wrote that he had not known of 'the organisation referred to' but that he knew that Sievert had already 'lost interest in a general Nordic radiophysicists' association' last year. He continued:

> I have not done so, and the purpose of my sending my invitation, to you as well in case you might like to attend, was to try and prevent the situation of nothing being ready for this year either.

Benner continued by saying that he still thought a 'more comprehensive Nordic radiophysicists' association could also be justified':

> Therefore, when it is possible, I would be grateful to receive further information about the type of organisation mentioned by you and about the meetings. The answer should in some way be significant to the way in which you and I continue to handle the matter.

However, one reservation against Sievert's plans came from one of his cooperation partners. Rønne-Nielsen wrote to Sievert on 25 May and thanked him 'for the splendid day (and evening) in Stockholm in April. It was – thanks to you – a brilliant reminder of times gone by'. He then wrote:

> With regard to the question of Nordic radiophysics conferences, I think, unfortunately, that if the matter is now taken up on a broader basis, our initiative is doomed to fizzle out without regard to what would objectively be the most practical and therefore preferable. Conferences convened by us four will simply go against today's democratic trend as soon as the other side proposes the formation of an association which is led by elected people. And the other radiophysicists are taking a strong stand on that because what is the point of convening a congress for people other than ourselves if these others are not inclined to attend? As it is clear that the Nordic association will be formed without taking into account our point of view, I think it would be wise idea to pretty much put on a bold face.

Jørgen Koch was less abject in a letter to Sievert of 4 June in which he wrote the following about Benner's proposal: 'It is not that easy for me to form an opinion on such a proposal from here over and above emphasising once more that I am still of the opinion I gave in Stockholm, that I am still interested in and would support an extension of the cooperation in accordance with the guidelines that we spoke about'. However, he thought that 'If there were a vote in Gothenburg to establish the Benner

Radiophysicists' Section, it would probably be formed anyway since I understand that many radiophysicists will attend the meeting'.

The 20th Congress of the Nordic Society of Medical Radiology convened in the auditorium of the Gothenburg School of Economics on 10 June 1955. The Congress fee was 15 Swedish kronor for active members and the banquet charge was 35 kronor per person. The Congress was divided into a diagnostics section and a therapy section. The latter had in turn been divided up into a therapy section and a radiophysics section. Radiophysicists were now 'proper' participants in the Congress for the first time.

The day before the opening, the Swedish Radiophysicists' Society met Sven Benner at the Jubilee Clinic at *Sahlgrenska Sjukhuset* (hospital). The following day, a number of radiophysics representatives from Denmark, Finland, Norway and Sweden met to discuss the matter of a merger to form a Nordic association. An 'Account' of the meeting signed by Benner, Moxnes and Rønne-Nielsen as well as the Finnish representative *J. V. Lieto* was circulated. It was considered too early as yet to organise the cooperation into a more permanent form. This meant that the idea of the radiophysicists being a section of the Nordic Association was abandoned. The following statement was agreed to prevent this from being misunderstood:

> The Nordic radiophysicists' merger outside the framework of the Nordic Society of Medical Radiology does not mean that they are distancing themselves from the radiologists. On the contrary, in all matters of common interest to radiologists and radiophysicists, the radiophysicists hope for a good and reliable cooperation within the Nordic and the national societies for medical radiology. However, there are many radiophysics matters which are not directly attributable to medical radiology. With regard to this and to take radiophysics forward in cooperation with one and all who are interested in doing so, the idea of the merger has arisen and has now been realised.

However, the 'realisation' did not mean that a Nordic association had now come to fruition. The decision that was made was worded as follows:

> Until more permanent forms are established for cooperation between the radiophysicists of the Nordic countries, a council of trustees must be appointed. The council must include two representatives of each of the Nordic countries. One of the representatives ought to be the executive director of the country's supervisory institution (or organisation) for radiological facilities, or the person he appoints to be his deputy, and the other ought to be appointed by the country's radiophysicists.

They thus proceeded with caution, possibly due to uncertainty regarding what Sievert's plans would actually involve. At the same time, Sievert continued with no concern for other people's plans. On 22 June, he received the following, marked secret at the time, letter from the research officer at the Defence Staff, Lieutenant Colonel Torsten Schmidt:

> Having been privately handed a strictly confidential memo containing guidelines for Scandinavian cooperation in radiation protection matters dated Copenhagen, Oslo and Stockholm in June 1955 but not signed, I hereby have the honour of informing you that as far as the Supreme Commander of the Swedish Armed Forces is concerned, there is nothing to prevent the signing of said memo.

When Sievert had visited Östen Undén on 16 June and discussed the proposed cooperation, on 22 June he sent a revised proposal. On 6 July he received a message from the Cabinet Secretary, Arne S. Lundberg that 'As far as the Ministry is concerned there is no objection to the content of said memorandum'. Undén had suggested that Sievert's proposal be called a 'memorandum' rather than an 'agreement'; agreements with authorities in neighbouring countries should only be reached through the Ministry for Foreign Affairs. Sievert sent the revised document to his Nordic colleagues and asked them to approve it - it was now a matter of no more than a memorandum.

Sievert's revised memorandum was entitled 'Memo of guidelines for Scandinavian cooperation on radiation protection matters'. After a couple of introductory paragraphs on the increasing use of sources of radiation, the 'burning question regarding the treatment of radioactive waste' as well as the spreading of radioactive substances from the nuclear weapons testing, came the actual content:

In awareness of the serious position into which the rapidly-increasing need to consider radiation protection matters may very soon plunge society, the undersigned, who, are responsible for the physical radiation protection checks and the associated research and development work in Denmark, Norway and Sweden, have agreed specific guidelines which apply to the cooperation between them until further notice. They concern:

> 1:0 to send, for ongoing information in due course and without delay, one another all the official documents, publications and provisions which are established in our countries for the purpose of radiation protection;
> 2:0 to exchange with one another points of view and plans concerning radiation protection matters and, on request, advise one another concerning proposals for laws, regulations and provisions which will be established in our countries with regard to such matters;
> 3:0 to continuously exchange experiences and state results of radiation measurements on people who are exposed to ionising radiation in their work or elsewhere;
> 4:0 to, in so far as this is viable for legal reasons, send one another copies of documents and state other details of radiation injuries and incidents with such, as well as events that are important from the radiation protection point of view;
> 5:0 to, within the framework of that which is possible due to the secrecy of the matters, keep one another informed of the technical development work with regard to our countries' measurements of changes in ground radiation originating from nuclear explosions as well as radioactivity in water and air;
> 6:0 to, with the same reservation as stated in point 5:0 and after having obtained authorisation in each individual case, inform one another of the activity increases that are observed in our countries, originating from nuclear explosions elsewhere;
> 7:0 to seek to jointly organise continuous radiation measurements and to establish the distribution of changes to the general level of radiation in the Nordic countries;
> 8:0 to meet at least twice a year to discuss relevant radiation protection matters and maintain cooperation as discussed in this memorandum;
> 9:0 to arrange Nordic radiophysics conferences at which lectures, discussions and social interaction are freely and simply arranged, and
> 10:0 to also promote cooperation regarding radiation protection work that takes place in our countries.

With regard to points 2–8 above, unless otherwise agreed in special cases, we also undertake to treat information received verbally and in writing as personal and confidential.

The letter was intended to be signed by Koch, Rønne-Nielsen, Moxnes and Sievert. You may ask why Finland was not covered by the document, but at this point in time the Finnish Radiation Protection Act had not yet appeared and there was no Finnish radiation protection organisation. The person with whom Sievert was in contact was Professor Mustakallio, who was not a physicist but a radiologist. Sievert wanted cooperation between physicists who were responsible for radiation protection. Salimäki was still the only medical physicist in Finland and Sievert did not know him well.

In Denmark, Rønne-Nielsen sent Sievert's memorandum to the Board of Health and asked whether he and Koch could sign it. A copy of the Board of Health's response of 2 November 1955 was sent to Sievert. It read:

> In a letter of 25 May 1955, you informed the Board of Health that you and Dr. Phil. J. Koch held negotiations with Professor Rolf Sievert in April this year in which you discussed the matter of Scandinavian cooperation regarding the protection of the population against radiation, and that in Oslo, you and Dr. Phil. J. Koch, along with Professor Sievert and Chief Physician Moxnes, agreed a proposal for a memorandum

> containing guidelines for Scandinavian cooperation regarding the matter of radiation protection. In this respect, let it be said that the Board of Health knows of nothing to prevent you and Dr. Phil. J. Koch, who is the Board's supervisor of activities involving radioactive substances or x-ray devices, signing the presented memorandum on your own behalf in the way that it was drawn up in July of this year, although there are grounds to emphasise that the Board of Health considers that it is in a position only to pay your and Dr. Phil. J. Koch's expenses for the annual meetings proposed in 8:0 of the memorandum in so far as these may be of significance to the Board of Health's supervisory activity.

At that point, Sievert had already invited a number of key persons to attend the opening of the World Health Organization's (WHO's) radiation protection course at the Institute of Radiophysics on 14 November 1955 to then take part in a meeting on the Nordic radiation protection cooperation. Attending from Denmark were Juel Henningsen, Jørgen Koch and Rønne-Nielsen. From Norway, Moxnes had accepted the invitation while the head of the Norwegian Medical Board *Karl Evang* (1902–1981) had said that he did not have time, although Evang's Swedish colleague Arthur Engel had promised to come.

The meeting took place as planned but did not lead to any result, although there was nothing to indicate that anyone disagreed with the proposed guidelines. Sievert's interest, which changed direction rapidly, ended up being affected by the international events in 1956. One of these was the birth of UNSCEAR, as well as his assignment as the Chair of ICRP. At the same time, new players were coming into the Nordic cooperation; in Norway, this meant tangible changes. As with Sievert's institute, Moxnes' originated from a radiophysics institution at a hospital. However, unlike Sievert, neither Moxnes nor his successor Kristian Koren had any great ambitions to seek to extend his responsibility for radiation protection to also apply to radioactive fallout or issues within the field of atomic energy. As with the situation in Sweden, military physics research was also important. The person responsible for activity measurements in the surrounding environment in this respect was head of research *Thorleif Hvinden*, who would subsequently play an important role in the Nordic cooperation.

As Sievert's Institute of Radiophysics had initially done, the Danish and Norwegian radiation protection institutes acted as supervisory bodies for superior authorities but had not gained the same independent position of reporting directly to a Ministry which Sievert's radiation protection activity was afforded in 1958. This meant that when Per Grande started as head of the newly-formed Danish radiation protection institute (the Board of Health's Radiation Hygiene Laboratory) at the end of the 1950s, he was subordinate to the Board of Health whose determined Chief Physician Eigil Juel Henningsen would soon be Sievert's premier Nordic cooperation partner.

Correspondingly, Kristian Koren in Norway was overshadowed by the superior Reidar Eker, who was head of *Radiumhospitalet* in Norway but also Chair of the Norwegian State Council for 'radiation hygiene matters' which had been set up in March 1956. And in the meantime, Eker had to resign himself to the fact that Karl Evang, the head of the Medical Board (the Norwegian Directorate of Health), personally took over when matters interested him.

In 1956 the Danish Atomic Energy Commission started taking measurements of the air activity in Risø. At the same time, the FOA in Sweden began to use a multi-channel Hutchinson-Scarrott analyser at Drottning Kristinas väg in Stockholm to analyse the radioactive fallout. In Norway on 6 September 1956, Torsten Magnusson entered into an agreement with Thorleif Hvinden on behalf of the FOA regarding cooperation between the Defence research institutes in Norway and Sweden where measurements and measurement results concerning radioactive fallout were concerned. And on 11 October of the same, Evang issued a press release regarding the measurements of radioactive fallout from the nuclear weapons testing. Its final section read:

> The extended control measurements will continue through cooperation between the Defence Research Institute, the Institute for Atomic Energy and the National Radiological Physics Laboratory. A temporary position has been created for a Chief Physician in radiation hygiene and plans to extend the National Radiological Physics Laboratory are underway. The National Institute on Radiation Hygiene, which was established in March 1956, is being kept informed and is assessing the health-related

> side of the issue. The Institute has not found that there is any direct hazard from the fallout that has occurred thus far, and has therefore found no reason to advocate any special measure on the part of the health authority.

So, a great deal took place beyond the original cooperation agreement initiated by Sievert, but ICRP and UNSCEAR were now occupying his time and thoughts. In 1956, following the consultation with the Ministry for Foreign Affairs, his interest in the Nordic cooperation was limited to inviting Eker and Juel Henningsen to act as contacts for the Swedish UNSCEAR delegation. Statements regarding the position of the radiophysicists were made at the Nordic Society of Medical Radiology's 22nd Congress in Åbo in 1958. The Society's Jubilee letter shows that (Unné, 1984):

> From the board meeting, it can be noted that the Secretary General stated that the issue of the radiophysicists has now been ironed out and that the radiophysicists were now members of the different countries' radiology associations with voting rights.

In 1956–1958, nuclear weapons testing intensified and culminated in 1958. The Americans detonated forty or so nuclear charges in Nevada in 1957–1958, but these were all relatively small (the largest corresponded to 74 kilotonnes of TNT). In 1958 (but not 1957), the Americans detonated many nuclear charges in the Pacific Ocean area: 10 on Bikini with a total blast strength corresponding to 12 megatonnes of TNT and 21 at Enewetak corresponding to total of 16 megatonnes of TNT. In addition to this, they detonated 6 charges from missiles above the sea, corresponding to approximately 8 megatonnes of TNT. In 1958 the British detonated five nuclear charges on Christmas Island south-west of Java in the Pacific Ocean with a total blast strength corresponding to about 5 megatonnes of TNT. In 1957–1958, the Soviet Union detonated a good twenty nuclear charges at Semipalatinsk corresponding to an approximate total of 2 megatonnes of TNT. More powerful detonations occurred at Novaya Zemlya where previously, in 1955, the only explosion that had been carried out was an underwater explosion corresponding to 3.5 kilotonnes of TNT. In 1957, four charges corresponding to a total of 4.5 megatonnes of TNT were detonated there. The year after that and until 25 October, the Soviets detonated 24 nuclear charges corresponding to a total of almost 16 megatonnes of TNT.

The increase in air contamination from the nuclear weapons testing was reflected in measurement results all over the world. In Copenhagen, Hilde Levi ascertained that, in spite of everything, the radioactive contamination of drinking water was low except for one case, the island of Saltholm in Öresund between Malmö and Copenhagen. The island is very flat and marshy, only five metres above sea level at its highest point, and there is no subsurface drinking water. Those who lived on the island were therefore using rainwater which was collected in cisterns.

On 10 November 1958, Hilde Levi published a report on his measurement results for the period of September 1957– October 1958. She ascertained that rainwater in the drinking water cisterns on Saltholm contained a high level of activity which she expressed in the number of radioactive disintegrations per minute and litre. With this unit, the numerical value was around 400 while she mentioned for comparison purposes that the MPC was around 200. By 'MPC' she meant the Maximum Permissible Concentration that had been recommend by ICRP in 1954 in the report printed as Supplement number 6 to the *British Journal of Radiology* in 1955 for drinking water containing unidentified fission products (p. 54 of the report). Drinking water on Saltholm would thus be twice the level of activity which ICRP wanted to allow. 200 disintegrations per minute and litre corresponds to approximately 10^{-7} microcuries per millilitre, which was the value stated by ICRP.

The Board of Health was worried by Hilde Levi's report, and on 24 November 1958 issued a circular to the Danish county medical officers, stating among other things:

> During the recent increase in the quantities of fallout, the Board of Health has ascertained an increase in the content of radioactive substances in unfiltered rainwater. The content exceeds specific internationally-established limits for the content of

> radioactive substances in drinking water. However, it must be expressly noted that these limit values for the content of radioactive substances in drinking water were established exclusively for the long-term use of the water as drinking water, so if said limit values are exceeded, there will be no immediate danger to your health if the relevant rainwater is used as drinking water for shorter time periods.

In the circular, the Board of Health asked the county health officers for information on the use of rainwater as drinking water within their areas.

A memorandum was also drawn up in November 1958, ‘An overview of measurements of radioactive fallout in Denmark’, written by J. Ambrosen, H. Gjørup, E. Juel Henningsen and Hilde Levi. The authors gave an account of where the fallout measurements were taken. The Defence Research Council used the radiophysics laboratory at *Radiumstationen* where Johan Ambrosen was responsible for measurements in precipitation and air samples that had been collected. Then in 1954 the Board of Health engaged Hilde Levi to take measurements of drinking water samples from the whole country. Since 1956, the Atomic Energy Commission had arranged for measurements to be taken of air activity and precipitation on Risø, where radioecological studies of flora and fauna also took place under the leadership of engineer Gjørup. In connection with the international geophysical year, the Defence’s Research Council had also set up stations on Greenland for the collection of air and precipitation samples. Precipitation samples were also taken on the Faeroe Islands and Bornholm. Some analyses were specifically aimed at strontium-90, which was thought to be the most dangerous of the radioactive fallout nuclides. All samples collected were measured at Ambrosen’s laboratory. No sensational results had been obtained with the exception of the relatively high values in rainwater on Saltholm.

In Norway, a report was published on the measurements taken in 1957 and 1958 by the Defence Research Institute. The report was written by head of research Thorleif Hvinden and gave a very good outline of the problems and measurement results. Hvinden also calculated doses of radiation, primarily from strontium-90 in the skeleton, but found no startling values.

In Sweden, the FOA and the Institute of Radiophysics circulated a joint message on 13 November 1958 in which the two Institutes described their cooperation and the different types of radioactive fallout measurement which were obtained. This message also mentioned the accumulation of strontium-90 in the skeleton as the greatest health issue. It was said that a temporary doubling of the intensity of the gamma radiation from the ground had been observed but that this had been short-term and that no increase greater than ten per cent in the level of gamma radiation had occurred for a period of more than two weeks. However, the gamma radiation from the ground had increased tangibly in recent weeks and had doubled the level of radiation at the time the report was written.

The Swedish report attracted the attention of the mass media. On 15 November, the broadsheet newspaper *Svenska Dagbladet* ran the headlines ‘RADIOACTIVE DUST IN SWEDEN’ and ‘Radioactive fallout still strong’.

In autumn 1958 I had returned from my year at the UNSCEAR Secretariat in New York and was fully involved in studying the impacts of the radioactive fallout. One day, a young man came into the laboratory and introduced himself as Jan Olof Snihs. He lived in Uppsala and was a physicist. He now intended to start work at Sievert’s institute, he said, as though it were obvious that there was a job available and that it was waiting for him specifically. I tried to explain that there were no jobs available but he appeared to see that as an irrelevant detail. It was the first time I had been exposed to Jan Olof’s monumental stubbornness. I referred him to the head of the nuclear physics department Arne Hedgran, who remembers the moment as follows:

> I think I remember certain elements, like the fact that Jan Olof came to my room unannounced. We started a serious discussion about work. It turned out that Sievert was immediately able to offer a grant from the Atomic Committee to which he had access but for which he had no use. I think I proposed alpha spectrometry using photographic technique and that Jan Olof started this but soon switched to doing other work.

This shows that perseverance gets you ahead and that stubbornness pays off. Luckily, I have to say, because Jan Olof became an invaluable colleague, notwithstanding the international renown of his stubbornness.

In 1959 and 1960, neither the USA, the Soviet Union nor the United Kingdom carried out any nuclear weapons testing (France on the other hand detonated its first three nuclear charges in Algeria in 1960 although the blast strength corresponded to a total of only 72 kilotonnes of TNT). However, in 1959 the fallout from the previous test explosions increased until the test ban meant that it began to fall. This increase made the Danish Board of Health repeat its warning about rainwater on 20 April 1959. The local healthcare committee for Saltholm now intervened and forbade the use of rainwater as drinking water. This action had far-reaching consequences and initiated intensive Nordic radiation protection cooperation.

The mass media became interested in the Danish ban. In Sweden, Rolf Sievert was called up by journalists asking for comments. Sievert, who at that point was well-known by Swedish journalists following the publicity of UNSCEAR's first report which had become available six months previously, answered that he could not say anything without access to the measurement data, but that he thought it unlikely that there could be a question of a serious health hazard. An editor from *Bohusläningen* rang Sievert and said that around a thousand people on islands outside Tjörn also used rainwater as drinking water. This made Sievert write to the Tjörn healthcare committee on 11 May to ask for water samples.

At that point, four years later, the cooperation agreement which Sievert had drawn up in 1955 was thought to have been forgotten. Sievert heard about the situation in Denmark and the ban on using rainwater on Saltholm from Swedish journalists, not from his Danish colleagues. And from Norway it was not Evang, Eker or Koren who got in touch, but head of research Hvinden at Norway's 'FOA'. Hvinden was disturbed by the events in Denmark and wanted to meet Sievert.

The meeting took place at the Institute of Radiophysics in Stockholm on Friday 15 May. Arne Hedgran and I also took part at Sievert's request. Hvinden thought that continuing nuclear weapons testing could cause problems with the contamination of both water and milk. Islands along the Norwegian coast also used rainwater as drinking water, and forbidding this would lead to greater problems and risks than on Saltholm to which it was not difficult to ship drinking water. Hvinden proposed that the matter should be discussed by experts in Denmark, Norway and Sweden.

Hvinden also said that the Organisation for European Economic Cooperation, OEEC, was interested in collecting results of fallout measurements. Sievert, who at this point was Chair of both UNSCEAR and ICRP, replied that this was primarily the UNSCEAR's responsibility, but that nothing prevented the OEEC from looking at the measurement data. During the meeting, Sievert rang Juel Henningsen and asked who had taken the measurements of the rainwater. It would be valuable if that person could come to Stockholm for a discussion while Hvinden was there. Juel Henningsen asked to be able to get back to me and did so immediately with a telegram: 'DR HILDE LEVI WILL ARRIVE AT STOCKHOLM AIRPORT SATURDAY 9.35.'

On Saturday 16 May, Hilde Levi took part in the continuation of a discussion about Sievert's institute. Rolf Björnerstedt from the FOA was now also present. Levi gave an account of her measurement results, of the Board of Health's letter to the county medical officers and of the local healthcare committee's ban on the use of rainwater as drinking water on Saltholm. They agreed to draw up a joint Nordic letter concerning the problem and I was asked to draw up a first draft and then travel to Copenhagen on 23 May to discuss the draft with Hvinden and Hilde Levi before it was reviewed by a wider circle of people on the following day.

On 23 May, Hvinden and I visited Hilde Levi at the University's Laboratory of Zoophysiology. As Secretary of ICRP I was able to show that the Commission had never actually issued any recommendations that were appropriate with regard to rainwater on Saltholm. Firstly, the ICRP's recommended MPC values concerned a planned, normal situation, not an emergency situation where the intervention was considered. It would have been a poor protection ambition if there were no greater requirements for the normal situation than those that had to be applied in an emergency situation. The MPC value that Hilde Levi had found in ICRP's recommendations from 1954 concerned drinking water contaminated with radioactive substances of unknown composition. In such a case, in order to be cautious, it was necessary to assume that the radioactive substances were isotopes of radium. But there was no reason to believe that rainwater on Saltholm contained radium; it was a question of radioactive fallout from the nuclear weapons tests. If no-one had any additional knowledge, caution invited the assumption that the radioactive contamination consisted in full of the most dangerous fission product – strontium-90. However, this could not be; it was known that strontium-90 constituted only an insignificant share of the total fission products. If this were the case, the MPC value for the contamination according to one table (table 3) in an ICRP report which was about to be published (ICRP, 1959) would be 200 times the value which had made Hilde Levi sound the alarm.

However, ICRP's MPC values now concerned people in radiological work. A footnote to the table said that for the public in the vicinity of a nuclear energy facility, a value that was only one tenth of the amount should be cautiously used. However, it still left a value that was 20 times the amount of Hilde Levi's value and which also applied to a normal, life-long situation and gave no grounds for special measures in an emergency situation.

On Saturday 24 May 1959, Sievert had joined up. He, Hvinden and I then met Juel Henningsen, who was accompanied by Jørgen Koch, Chief Physician Børge Christensen from the Finsen Institute and Hilde Levi. The Danes and Norwegians said they were satisfied with my draft report following minor corrections proposed by Hvinden. But, they said, the draft of the report concerned the physical aspects; the medical matters had not been mentioned in the introductory discussions. The Norwegians and Swedes had nothing against sending the document to the Nordic healthcare authorities while the Danes wanted to wait until it had been supplemented with medical points of view. They agreed to prepare such points of view for a subsequent meeting which they decided to hold in Oslo in June. The group also discussed the continued cooperation and the need for joint reports to the public a couple of times a year.

The day after that, 25 May, a four-day symposium started in Risø regarding technical methods within radiation protection, or 'health physics' as dictated by the American-influenced terminology. The 110 non-Danish participants lived at a hotel in Copenhagen and were given transportation to and from Risø every day. Sievert stayed at the *Hotel d'Angleterre* at Kongens Nytorv at he usually did. In addition to Sievert, the Swedes who took part in the symposium were, Lars Carlbom, Arne Forssberg, *Sven Hagsgård*, Arne Hedgran, Börje Larsson, *Sören Lindhe*, Torsten Magnusson, *Sigvard Thulin* (1923–2002) and Gunnar Walinder.

The symposium was opened by Niels Bohr, who also held an introductory address. Sievert held no lecture but was Chair of the first session which dealt with fundamental scientific principles. He was met with great esteem and respect and was also given the honour of planting a representative guardian tree.

The next meeting of the Nordic radiation protection experts took place in Oslo on 4–5 June 1959. Juel Henningsen, Børge Christensen and Hilde Levi from Denmark took part, and from Sweden Sievert, Rolf Björnerstedt, Arne Hedgran, Arne Nelson, *Bertil Swedin* (1909–1994) – Sievert's medical Chief Physician. The Norwegians comprised ten participants, the most active of whom, over and above Reidar Eker and Hvinden, were Finn Devik, Lorentz Eldjarn and Erik Poppe.

The meeting discussed a revised version of my draft report and the action levels that I had proposed. A new draft was drawn up and approved. It was agreed to allow this report to be an Appendix to a letter from the meeting to the radiation protection authorities in Denmark, Norway and Sweden (no contact had been made with Finland as yet). The letter was signed by Juel Henningsen, Eker and Sievert 'in June' 1959. Following introductory paragraphs on the preparatory meetings in Stockholm and Copenhagen and the problems with rainwater as drinking water, the letter continued:

> With regard to the current position as regards radioactive fallout in the Scandinavian countries, the conference has considered itself able to ascertain an essential increase over the past year, which intimates that if the increase occurs at the same rate as now, the radiation may shortly contribute to the irradiation of the public with levels that should not be exceeded with regard to the maximum permissible doses of radiation recommended by ICRP for large population groups. However, it must be pointed out that the values stated by ICRP are not the limit values that represent a sudden transition from a non-hazardous to a hazardous dose area, but refer to dose levels below which the likelihood of radiation injuries are considerably less than other injury risks in modern society. […]

The letter emphasised in particular that it was difficult to determine the contamination level of drinking water and food substances at which intervention on the part of the public from the radiation protection point of view must be considered. It was also stated that it had not yet been possible to show that such small doses of radiation, especially such small radiation intensities that were concerned here, really could cause harmful effects in people and that we were therefore obliged to draw conclusions from the experience of effects of radiation at significantly higher doses and intensities than those which were currently caused by the radioactive fallout.

The letter was concluded with the following proposal:

> The experts propose that representatives of the radiation protection authorities in the three Scandinavian countries, and the other experts who are considered to be necessary, meet regularly to discuss matters concerning the radioactive fallout as well as overall radiation protection problems,
>
> that, before making definite decisions on matters concerning general radiation protection measures involving essential social intervention, if the situation so permits, the radiation protection authorities in the Scandinavian countries should wait for the result of a joint discussion of the case by professionals representing the radiation protection authorities of the three countries,
>
> that, with regard to the substantial interest shown by the population in the radiation hazard, regular notifications – twice a year as an example – be made available to the public regarding radioactive fallout and other radiation injury risks. To the extent that it is considered suitable, these notifications may be drawn up in consultation with experts from the Scandinavian countries and be submitted to the radiation protection authorities in these countries, and
>
> that the report appended to this letter be published in its entirety in the three Scandinavian countries as soon as possible, simultaneously and at an agreed time.

The appended report stated a number of action levels for intervention regarding rainwater as drinking water. If the contamination consisted of strontium-90, the average concentration over one year would not be permitted to exceed 100 picocuries per litre* (3.7 Bq/l). The total activity of all radionuclides should not be permitted to exceed 3 000 pCi/l (111 Bq/l) as an average value over one year, 30 000 pCi/l as an average value over one month or 100 000 pCi/l (3 700 Bq/l) in one individual instance. These values were selected using extreme caution on the basis of ICRP's recommendations.

In the United Kingdom in April 1959, the Medical Research Council (MRC) had published a report in the *British Medical Journal* recommending action levels for food contamination with iodine-131, strontium-90 (and strontium-89) and caesium-137. The report had been drawn up with reference to the Windscale accident and came from a committee under the Chairmanship of 'Bill' Pochin with Walter Binks as Secretary. The Committee had consisted of around twenty experts, including the well-known and knowledgeable Sir Ernest Rock-Carling, Sir John Cockcroft, L. H. Gray, Len Lamerton, John Loutit, Andrew McLean, Greg Marley, Val Mayneord, Joseph Mitchell, *Gerard Neary* (1917–1972), Bill Spiers, Katharine Williams and Brian Windeyer no less. The recommended action levels, expressed in the current unit, corresponded to about 75 Bq/l milk for strontium-90, approximately 5 000 Bq/l milk for caesium-137 and an initial concentration of 2 600 Bq/l milk for iodine-131. The situation in mind was a reactor accident with a one-off emission of the radioactive substances. They could then benefit from the extra security in the knowledge that iodine-131 with its half-life of about 8 days would rapidly disappear as a consequence of the radioactive disintegration. These values which were recommended by the MRC in 1959 would influence recommendations regarding protection measures all over the world for a long time into the future.

The British recommendations were not discussed and may not even have been known by the Nordic experts when they met in Oslo in June 1959. It was not usual to discuss scientific matters during

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* 1 picocurie (pCi) is one millionth of a microcurie, i.e., 10^{-12} curies and, in the unit used today, 0.037 becquerels (Bq).

expensive telephone conversations abroad, and reports like the British ones were disseminated by means of the ordinary post or at the international meetings.

The report from the Oslo meeting to the Nordic healthcare authorities also made it through to Nordic politicians and in September 1959 led to a members' proposal within the Nordic Council for cooperation with regard to radiation research. The proposers (Helge Larsen, Finn Moe, Hugo Osvald, Kaarlo Pitsinki and Helge Seip) wrote:

> It appears evident that extremely close cooperation between the institutions of the different Nordic countries in this field is very important, partly because the economic and personnel resources of the individual Nordic countries are scarcely sufficient for all-round, isolated research in this field. It also appears that natural conditions are created for the Nordic bearing in this field to be coordinated in international organisations.
>
> Further pursuance of the thus-initiated cooperation must be seen as an urgent task within the framework of the Nordic cooperation. The initiative taken here appears to be largely merited by the government authorities' support. Firmly-organised, close cooperation ought to take place as soon as possible. A natural starting point for continuing reviews of the forms of the Nordic cooperation are the recommendations which were adopted at the Oslo meeting on 4 and 5 June this year. In this respect, the Nordic Council has the opportunity of being proactive, a possibility which we believe should not be missed. We have therefore deemed that the Council should take up the case.

From Finland, the Medical Board and the Commission for Radiation Protection gave opinions, in both cases through Director General Niilo Pesonen who was head of the Medical Board and Chair of the Commission. In his latter capacity, he wrote:

> […] the Commission respectfully states that it finds the proposed Nordic cooperation particularly necessary, not to mention indispensable for Finland and regrets that the Commission, which has the task of ensuring international cooperation in matters concerning radiation protection, was not given the option of sending a representative to the last meeting held by the Scandinavian countries on 4 and 5 June in Oslo.

The members' proposal was dealt with by the Nordic Council's Cultural Affairs Committee which, on 5 November after hearing Rolf Sievert and the Finnish medical officer *Paavo Kuusisto*, agreed the following recommendation:

> The Nordic Council appeals to the governments to make available the best possible options to support and develop the cooperation that has already been established between the institutions which are responsible for matters regarding protection against radioactive radiation.

The politicians did not feel that the term 'radioactive radiation' was a freak term in physics (the radiation was not radioactive but came from radioactive substances), but at that time we physicists were not that scrupulous either; both Sievert and I talked ingenuously about radioactive radiation.

The Cultural Affairs Committee's recommendation was presented at the Nordic Council's 5th meeting in Stockholm on 6 November 1959 by the Cultural Affairs Committee's spokesperson Professor Hugo Osvald, who said that Professor Sievert had now found that the radioactive ground deposition had started to diminish, but that this was probably due to the dry summer (neither Sievert nor Osvald mentioned that no major nuclear weapons tests had taken place in 1959). Following a vote, the Council, with Bertil Ohlin as Chair, adopted the Cultural Affairs Committee's proposal with 54 votes for and 1 against (Finnish man Kilpi). The recommendation was designated number 17/1959. At the Presidium Council's meeting on 7 November 1959, it was agreed that Sweden would coordinate the cooperation. In Sweden, it would take almost a year for the government to act on the matter. Not until 28 October 1960 did Minister of the Interior, *Rune Johansson* (1915–1982), write to the Radiation Protection Committee to 'ask the Committee to take the measures that the recommendation required of Sweden'. On 28 November 1960, the Radiation Protection Committee decided to ask Sievert to 'take the measures that the recommendation required of Sweden'. Sievert was now free to act and he did so.

This started a Nordic cooperation that would continue for more than a decade with the support of the Nordic Council. The meetings which were arranged primarily discussed radioactive fallout and the recommendations that the healthcare authorities in the Nordic countries wanted to execute. However, the three meetings in 1970–1974 were devoted to nuclear power's environmental problems. After the 1959 meeting in Oslo, the meetings were held in Stockholm in 1959 (November) and 1961, in Copenhagen and Oslo in 1962, in Stockholm (two meetings) in 1963, in Oslo in 1964, in Reykjavik in 1965, in Helsinki in 1967, in Copenhagen in 1969, in Stockholm in 1970, in Oslo in 1972 and in Copenhagen in 1974. Apart from Sievert (and later on, me), the national representatives in this cooperation were usually Chief Physician at Denmark's Board of Health Eigil Juel Henningsen, Norway's Dr. Reidar Eker of *Radiumhospitalet* in Oslo, Finland's Professor of Radiology Sakari Mustakallio (initially), and Iceland's Chief Physician *Gisli Petersen*. Sievert and I were the only representatives who were not physicians. The Norwegians were particularly active because they had the strongest level of radioactive contamination in milk. The main active participants from Norway were Hvinden, Eldjarn and Pihl. One consistent problem each time it came to agreeing on a report to submit to the healthcare authorities was that the Norwegians and the Danes wanted the report to be 'confidential' while the Swedes referred to the Swedish principle of open access to official documents, which meant that the reports became available to anyone who wanted to read them.

In Autumn 1959, Swedish action levels for intervention against foods contaminated with radioactivity were discussed by a group consisting of Rolf Björnerstedt, Lars Carlbom, Arne Hedgran, Jan Olof Snihs, Sievert and me. For the first time, an analysis took place of which foods could conceivably contribute to the total intake of radioactive substances. Professor *Ernst Abramson* (1896–1979) at what was then the National Institute of Public Health was consulted with regard to the composition of the general overall diet.

The group was working on the basis of ICRP's table for MPC values for water contaminated with different radioactive substances and took one tenth of these values, bearing in mind that it was a matter of the public rather than radiation workers. The group then assumed that the radioactive contamination of the environment also gave rise to external gamma radiation from the ground deposition and therefore further reduced the values to half. The maximum permissible intake (in a normal situation) would thereby be as follows for

alpha emitters	10 pCi per day		i.e.,	0.37 Bq per day
strontium-90	100	"		3.7 "
other activity	3 000	"		110 "

The group then distributed the intake per kilogramme of different types of food on which consumption information had been received:

		pCi/kg		
	kg per day	alpha emitters	strontium-90	other total activity
Water	0.6	3	30	900
Grains	0.3	3	30	900
Vegetables, fruit	0.2	10	100	3000
Root vegetables	0.3	1	10	300
Meat	0.1	1	10	300
Fish	0.05	10	100	3000
Dairy products	0.8	5	50	1500
Miscellaneous	0.4	1	10	300

The work group ascertained that taking into account all irradiation routes led to very low action levels for individual foods. For the total activity in milk, for example, action levels would be as low as 1 500 pCi/l (approx. 50 Bq/l). For alpha emitters, action levels were lower than the natural radium levels in many foods. The group's summary was:

> As this overview shows, the consistent application of the ICRP's recommendations leads to very stringent requirements for the individual components in our food. However, the most constructive thing appears to be to accept the ICRP's recommendations in principle and that, if there are some unreasonable consequences, point this out for remedial action. The resort of creating standards without heeding the ICRP's values would probably lead to confusion and an untenable situation for the radiation protection activity.

What the group did not discuss was the reasonableness of working on the basis of ICRP's recommendations for normal activities when it came to intervention in an acute, unplanned situation. Because ICRP's recommendations did (and do) reflect a high level of ambition for radiation protection and concern situations involving life-long irradiation, it was scarcely reasonable to apply them to emergency situations. In those situations, intervention must primarily improve the situation for those at risk. The intervention must not lead to more force than is necessary. But this was not stated in ICRP's early recommendations, which did not deal with emergency situations. This problem did not become obvious until the Windscale accident, and not until the British MRC report in 1959 were the necessary conclusions drawn and more realistic action levels recommended.

On 23–26 November 1959, the Nordic radiation experts met once again at Sievert's initiative in Stockholm to discuss radiation protection matters of common interest. It was possible to ascertain that the amount of fission products in air and precipitation had been at its highest in May 1959 but had then diminished so that the values in October were only a few per cent of the maximum values. On average, the contamination of milk had been 5–10 pCi (0.2–0.4 Bq) strontium-90 per litre and 20–100 pCi (approx. 1–4 Bq) caesium-137 per litre, values which were below the action levels that had been discussed. However, it was disturbing that the high level of precipitation in the Norwegian coastal areas had also led to more substantial radioactive substances fallout. The report from the meeting was signed by Eker, Juel Henningsen, Mustakallio and Sievert; this was also the first time that Finnish experts had participated. The report ended with following conclusion:

> The Nordic expert group finds that nowhere does the radioactive fallout that has been recorded in the Nordic countries until now necessitate practical protection measures on the part of the healthcare authorities. The amount of radioactive material that reaches the ground depends mainly on the amount of precipitation; however, the intake of these substances and their migration into the food chain is due not only to the amount of fallout and the fallout rate, but also to a number of other factors such as soil type, pasture type and the haulage conditions. For an analysis of the connection between the radioactivity in the fallout and the contamination of foods, the conditions in the food chain must be clarified using agricultural and veterinary analyses which must be carried out in each country, taking into account the special local factors; it is particularly necessary to increase the scope of analyses in the most vulnerable areas. The Nordic expert group would therefore like to emphasise the necessity of the healthcare authorities in each individual country to support analyses that can clarify these conditions, not just in connection with the relevant fallout situation but particularly with a view to the possibility of resuming nuclear weapons testing and primarily bearing in mind the possible contamination as a consequence of the peaceful use of atomic energy.
>
> In this context, guidelines should also be drawn up for the health-related measures that should to be considered if the radioactive contamination exceeds the recommended limits.

At the same time, an English-language joint stencilled report was compiled from the Danish, Norwegian and Swedish Defence Research Institutes as well as from Sievert's Institute of Radiophysics.

The report also gave an account of the measurement results that had been obtained during the period of October 1958–October 1959. The report also mentioned that solid, highly-active particles were not unusual within the first 2–3 months of powerful explosions and that the diameters of the particles were between 0.2 and 5 micrometres. The activity of the particles was seen to be proportional to their volume. The associated biological risk was generally discussed on one occasion but was not thought to be great – after all, the irradiated volume of tissue was very small. On the other hand, some active particles got into paper production through water and led to disruptive dots on photographic film which was stored in a paper cover. The most active particles could have an activity of tens of becquerels.

16. NOVAYA ZEMLYA AND CONTINUED COOPERATION

On 12 April 1960, Hvinden came to Stockholm again, this time to the Institute of Radiophysics to discuss measurement resources with Kay Edvarson from the FOA as well as with Sievert, Hedgran and me. It turned out that Norway had an overcapacity for strontium-90 analyses while Sweden had good resources for gamma spectrometry measurements and for whole-body measurements of caesium-137. They decided to look at the possibility of coordinating measurements and exchanging samples.

The consequence of the nuclear weapons testing stoppage introduced by the USA and the Soviet Union in 1958 had been that the level of caesium-137 in Swedish dairy milk had gradually fallen to half of what it had been at its highest in summer 1959. However, in 1961, the Russians declared that they intended to detonate new, very powerful nuclear charges at Novaya Zemlya and closed off the waters in the area to all vessels.

Sweden was to some extent already prepared for the threatening situation that this brought to the country. Following the Windscale accident which spread radioactive iodine over large parts of Europe, the need to be prepared in the event of an accident had become obvious. On 3 June 1960, Swedish Parliament had established the law on protection measures in the event of accidents in nuclear facilities (‘the Nuclear Accidents Act’). In connection with the Act, the Radiation Protection Authority (known as the Radiation Protection Committee at the time) was asked through a Royal charter of 30 June in the same year to draw up instructions for the County Administrative Boards’ assessment of an accident within the county. At the same time, the King set up a special Expert Commission for Advice in the event of Nuclear Accidents. The Commission would consist of a number of experts whom relevant County Administrative Boards would be able to call on for advice following an accident. The Chair of the Commission was Rolf Sievert, with Arne Hedgran as substitute. Other members were Sievert’s radiation biologist Arne Forssberg, the FOA’s Torsten Magnusson, Professor at the National Institute of Public Health, Ernst Abramson, and head of department *Gunnar Nybrant* (1910–1991) of the SMHI.

In a memo from August 1960, Sievert proposed the use of the acronym KROA for the Commission (he wrote ‘it is unsightly but easy to remember’), but as early as the following month he had to accept that members preferred the shorter KRA. The Ministry of Health and Social Affairs provided the then Clerk to the Court of Appeal *Carl Lidbom* (1926–2004), by then already a colourful person, to be Secretary of the KRA. Lidbom was replaced on 15 November by Associate Judge of Appeal *Rune Berggren* (1925–2009), who later went on to be Director General of the National Audit Office. There was no doubting Sievert’s ability to find competent colleagues.

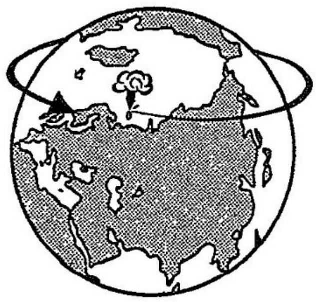
So, when the Russians announced their intention to resume nuclear charge tests, there was already a group that could give advice on preparedness. On the other hand, the KRA had no responsibility for preparedness planning. The provision of preparedness instructions was the job of the Radiation Protection Authority. However, in both cases it was Rolf Sievert who was pulling the strings. Sievert also had an additional organisation to resort to, i.e., the expert meetings which were held with the support of the Nordic Council.

Faced with the threat of the new nuclear weapons testing at Novaya Zemlya, Sievert called his colleagues in the Nordic countries to a new ‘Nordic Council’ meeting in Stockholm on 28–29 September 1961. They were concerned about what could happen and the ENEA had already created a system in the summer to warn of increased activity in the air.

23 people with radiation protection knowledge took part in the meeting, including Norwegian Per Grande from Denmark (who had now become head of the Board of Health’s Radiation Hygiene Laboratory, i.e., the new Danish Radiation Protection Institute), K. E. Salimäki from Finland, Reidar Eker, Thorleif Hvinden and Kristian Koren from Norway, and from Sweden Sievert and, among others,

Kurt Lidén from Lund, Torsten Magnusson and Kay Edvarson from the FOA, Lars Fredriksson, Bertil Åberg and Jan Olov Snihs. As for me, I was unable to participate due to a trip to America for UNSCEAR and ICRP's Committee II meetings. The Stockholm meeting resulted in a memorandum that was signed by Eker, Grande, Salimäki and Sievert and was then published in the Nordic Council's documents for its 10th session in 1962.

It is interesting to see that the harmful biological effects which it was thought the test explosions could cause if the wind were blowing in the direction of the Nordic countries were primarily leukaemia from strontium-89 and strontium-90, thyroid cancer from iodine-131 and hereditary injuries from caesium-137, in all cases with children being the most sensitive. If the situation were serious, the public ought to be encouraged to consume mainly tinned foods, dried milk, bread and potatoes but to avoid milk, meat and other fresh foods. The meeting also recommended an informative analysis regarding the contamination that could be expected in milk, meat and fish. As luck would have it, the winds did not blow towards the Nordic countries. They blew towards the east instead so a couple of weeks passed before the radioactive cloud reached us following its trip around the globe.



From Lindell & Löfveberg (Lindell, 1972)

The report from the Stockholm meeting in September 1961 proposed that immediate measures in the event of an acute contamination situation should depend on the external dose from the ground deposition during the space of a week. They distinguished between five different levels of radiation which, expressed in current units, were:

Level I	0.1–1 mSv
Level II	1-10 mSv
Level III	10-100 mSv
Level IV	0.1-1 Sv
Level V	more than 1 Sv

As soon as radiation level I was reached, analyses would start to clarify the situation. At level II, continuous drinking water and food checks would begin. At level III, indoor protection would be imposed. At level IV, all younger people would have to take shelter in the basement and evacuation would be considered if it reduced the dose of radiation. Level V would soon be life-threatening and the best possible protection or evacuation would be necessary.

These high levels of radiation reflect a rather pessimistic view of the possible development. Not even the Chernobyl accident led to level I anywhere in Sweden, but levels IV and V were reached in the areas that were evacuated around the Chernobyl nuclear power plant. The meeting did not discuss the long-term consequences of the radioactive fallout; that matter would be the subject of a subsequent meeting (which ended up being held in Copenhagen in January 1962).

The report from the Nordic meeting was submitted to the Ministry of the Interior (under which the Institute of Radiophysics came at the time) by Sievert on 17 October 1961. Faced with the threat of a disaster if the Russians detonated very powerful nuclear charges as close to Sweden as Novaya Zemlya, the government acted rapidly. On 20 October, Stockholm Palace dated a letter signed by Prince Bertil 'in the absence of my Gracious Master, His Majesty the King' as was the formal expression then. The letter referred to the memorandum from the Nordic experts, and the three subsequent paragraphs read:

> His Royal Majesty asks the expert Commission for protection measures in the event of accidents in nuclear facilities to, on the basis of the risks of radiation injuries as indicated in the abovementioned memorandum, rapidly in consultation with the appropriate County Administrative Boards, the Civil Defence Board and other relevant authorities consider more closely the preparedness measures which, in the event of an increase in radioactivity as a consequence of nuclear weapons testing, in different situations could be considered to be necessary to prevent harmful effects in various parts of Sweden, as well as to submit to His Royal Majesty the proposal which, with reference thereto, may be called for.

His Royal Majesty authorises the head of the Ministry of the Interior to, where necessary, summon special experts to assist the Commission in the fulfilment of said assignment; and that which has been prescribed for the Commission's members as regards remuneration for the assignment and for the undertaking of journeys within the kingdom shall also apply to said experts. His Royal Majesty also finds it appropriate, with the support of Section 13 of the Law of 3 June 1960 (no. 331) on the protection measures in the event of accidents in nuclear facilities, etc., to commission the head of the Ministry of the Interior, if as a consequence of the distribution of radioactivity from nuclear weapons testing special measures are required in order to protect the public, to announce such regulations and provisions that are stated in Sections 4, 5 and 6 of said Law.

Sievert, who had been forewarned of 'His Royal Majesty's', i.e., the Swedish Government's, decision, lost no time. He convened a KRA meeting on 20 October, the first day on which the Commission had power of attorney to act. It was now clear that, during the nuclear weapons testing which had been resumed at Novaya Zemlya on 10 September, the Russians intended to detonate a very powerful nuclear charge with a blast strength corresponding to 50 million tonnes (!) of TNT. The meeting convened by the KRA grabbed the attention of the press. *Expressen* ran the headline 'Top Swedish expertise summoned to discuss measures concerning the "Doomsday Bomb".' A spokesperson for the Civil Defence was quoted as saying:

> We are facing the terrifying prospect of that which was predicted in the event of war possibly becoming reality in times of peace. […] But the worst thing of all would be to hasten fear among the public. The situation must be judged calmly if not almost in cold blood.

The *Washington Post* was also quoted:

> The world will never be the same again following the explosion of the Russian 50-megatonne bomb. The Soviet Union intends to inflict on mankind more misery than has ever since the dawn of history been seen to have followed from just one human action or just one phenomenon.

What worried the Nordic authorities was their proximity to Novaya Zemlya. If the enormous nuclear charge were detonated close to the ground so that a dust storm were drawn up into the detonation cloud, the radioactive fallout could be very powerful in the direction of the wind. This could mean a disaster for the northern areas. Luckily, this did not happen, but no-one could have known this when the protection measures were prepared.

The Swedish government considered asking the Soviet Union when and where it intended to detonate the Doomsday Bomb. The Prime Minister Tage Erlander said:

> The notification regarding the forthcoming explosion of a bomb with dreadful effect must awaken the deepest of disappointment. The government will work through the UN to endeavour to bring the tests to a close.

With reference to the Swedish Government's decision, the KRA was boosted on 20 October 1961 by head of office Lieutenant Colonel *Sven-Eggert Bergelin* (1916–2000) of the Civil Defence Board and Assistant Professor Arne Nelson from the FOA. On 15 November, Associate Judge of Appeal Rune Berggren succeeded Carl Lidbom as Secretary of the KRA. During a hectic week, 20–26 October, the KRA in consultation with the Civil Defence Board and the County Administrative Boards drew up a memorandum proposing preparedness measures. The memorandum was submitted to the Minister of the Interior on 26 October and the KRA was immediately asked to take the proposed measures.

Two conditions needed to be fulfilled in order for the preparedness to become fully functional. The first was that the meteorological conditions meant that radioactive substances would be able to reach any part of Sweden for the next few days following an explosion. The other was that a very powerful nuclear explosion had occurred relatively close to the ground. In order to obtain information on the weather situation, Sievert had already agreed a warning system with the SMHI on 25 September which meant that the Institute of Radiophysics received a message twice a day about the possibility that the airstream over Novaya Zemlya could reach Sweden within a few days. Since the KRA had drawn up a preparedness plan at the request of the Minister of the Interior, the SMHI began to increase its monitoring of the weather situation from 26 October.

Monitoring to establish times for nuclear explosions was organised with the help of the seismological institution at Uppsala University and Kiruna Geophysical Laboratory. The FOA also registered the explosions.

In Uppsala, Assistant Professor *Markus Båth* (1916–2000) was responsible for the seismological monitoring. As of 11 September, in consultation with the FOA and Sievert, Båth had already moved the paper change from the seismographs from 8.00 to 11.30. Because the Russian explosions usually took place during the morning, this meant avoiding an observation delay of almost one day. From 21 October, the observation frequency was increased to every three hours and to an even greater frequency as of 26 October if the weather was critical.

If the preparedness organisation had become valid, i.e., if the two abovementioned conditions had been satisfied, Minister of the Interior *Rune Johansson* would have been made responsible for the central management and the KRA would have been available to him as an advisory body. The number of people in the central management was dimensioned to the need to be able to work in three shifts. Initially they were promised the use of premises at F8 in Barkarby where they would have had access to the army's teleprinter network. The offer was subsequently withdrawn and they ended up having to use the Civil Defence Board's premises at Jakobsbergsgatan 32 in Stockholm.

The central management was to include sections for operational management, meteorology, foreign contacts and signal connections, but primarily a radiac centre with the task of receiving and processing measurement results as well as making forecasts. The radiophysicists who were meant to have been part of the radiac centre's three shifts were Jan Olof Snihs, Rune Walstam and Gunnar Walinder.

The regional management was created at each County Administrative Board with the County Governor as the boss. At his disposal he would have an indication section, a radiac centre, a signal section and an information section. The County Governor would have an important task because police intervention might be necessary to carry out evacuations. There was access to the intensimeter 11 for indications.

The 'Doomsday Bomb', whose blast strength corresponded to 50 megatonnes of TNT, was detonated on 30 October 1961. Luckily, the winds blew eastwards. 15 November was the end of the period for which the Soviet Union had closed off the waters around Novaya Zemlya to all traffic due to the nuclear weapons testing. On 17 November, Sievert sent a draft of circular for approval to the Secretary of State *Carl Persson* at the Ministry of the Interior. He then sent 'following directions from the Minister of the Interior' a letter to all concerned stating that 'the state of alert is now ceasing but the preparedness organisations shall remain in a position to be able to return to their function should the need arise'. The need did arise in August 1962 when the Russians started a new series of tests at Novaya Zemlya by detonating a nuclear charge with a strength of 21 megatonnes of TNT.

In Sievert's account of the Novaya Zemlya preparedness, he mentions a Nordic meeting in Stockholm, also on 7 November 1961. I have not managed to find a report or any notes from this meeting if it did actually take place.

At the Radiation Protection Committee's meeting on 7 December 1961, a group of experts was established to coordinate the Swedish measurements of the radioactive contamination of foods. The group consisted of me as convenor and Kay Edvarson from the FOA, Lars Fredriksson from the National Agricultural Experiment Farm and Bertil Åberg from the Royal Veterinary College of Sweden. Our report was submitted to the Radiation Protection Committee in April 1962.

On 11–15 December 1961, the FAO along with IAEA and WHO arranged an international seminar in Scheveningen in the Netherlands regarding the radioactive contamination of foods. 280 people from 34 countries took part in the seminar. This was the first time that a comprehensive policy discussion was held for a large number of representatives of radiation protection and healthcare authorities, as well as representatives of 12 international organisations. 10 people from Sweden took part, including Ernst Abramson from the Swedish National Institute for Public Health, Stig Bergström from Atombolaget, Lars Fredriksson from the National Agricultural Experiment Farm, Jan Olof Snihs from Sievert's institute, and Bertil Åberg from the Royal Veterinary College of Sweden. As for me, I participated as representative of ICRP, of which I was Secretary at the time.

The noteworthy participants from other countries included several of the most important people within the radiation protection field such as Arrigo Cigna, Charles Dunham, Merril Eisenbud, Henri Jammet, John Loutit, *Rupert Maushart*, 'Bill' Pochin, R. Scott Russell, Conrad Straub and Paul Tompkins. *Asker Aarkrog*, Henry Gjørup, Per Grande, Thorleif Hvinden and Kristian Koren were noted from the Nordic countries. The representatives of the international organisations included Emile Wallauschek (ENEA), Pierre Recht (Euratom), Hussein Daw (IAEA), *R. A. Silov* (FAO) and Finn Devik (WHO).

I am providing these details because the meeting was important and it is interesting to see who the most important players were. The seminar's Proceedings (FAO, 1962) had noted the panel discussion concerning principles for protection measures. The discussion began with the Chair (Professor *J. Spaander*) asking me to give an account of ICRP's recommendations. I began by emphasising the difference between what was recommended in situations where the authorities have the source of an environmental contamination under control and, as in the case with the radioactive fallout from the nuclear weapons testing, situations where the authorities that are responsible for protection cannot influence the source but have to intervene in the case of contaminated foods.

ICRP's recommendations applied to the first case. With regard to the second case, I said (according to the FAO, 1962):

> The next requirement is to know the action that should be taken against the dietary component, as such, once it has been contaminated. This is indeed the major question of interest in the case of fallout from nuclear weapons tests, where source control is difficult to achieve. The setting up of 'action' levels of dietary contamination introduces the consideration of measures that directly concern the individual members of the public, insofar as they may be requested to change dietary habits, be it avoiding one dietary component or adding some other, for example, mineral calcium. It seems mandatory that any prescribed action level be qualified by the specification of the action. This implies a wide spectrum of possibilities and it seems very likely that the result will, and should, be different in different countries and, in fact, even in each special case. The important thing in this connection is to realize that the levels derived from ICRP recommendations are not necessarily such action levels as far as the environmental control is concerned. The fact that a dietary component is, or tends to become, contaminated in excess of the levels based upon ICRP recommendations does not in itself constitute a reason for taking any measures against this foodstuff*.
>
> In practice, most action levels, as far as they imply changing dietary habits, will probably be higher than ICRP levels that justify measures against the source, because it

* By the 'ICRP level', I meant the contamination that would correspond to the dose limits recommended by ICRP for the public under normal circumstances when you could control what happened with the source of the contamination.

> seems reasonable to assume that the latter levels should always contain an additional margin of, if not safety, at least reduced risk. However, this may not always be the case. For example, one can envisage cases where there is conceivably no harm or substantial cost involved in the exclusion of one dietary component already at ICRP level, and indeed one may think of situations where it would be appropriate to reduce an unnecessary exposure even if the doses would be below the maximum ones recommended by ICRP.

My last statement aroused mixed feelings, but I received support from Dr. Pochin, who said:

> I would certainly like to support this particular point by quoting a specific example. It seems quite clear that the use of radiation in the course of diagnostic medical radiology is very fully justified by its value. Yet this justification does not excuse the medical profession from reducing, when practicable, the amount of radiation involved in obtaining diagnostic information. Where the radiation, for technical reasons, causes unnecessary exposure in diagnostic tests and can be reduced without the loss of diagnostic information, it should certainly be reduced, although the amount of radiation involved, even without reduction, is still very fully justified by the value obtained.

The Chair of the meeting, Dutch Professor *M. J. L. Dols*, thanked the panel with the following final words:

> I understand that the panel has not reasonably been able to solve all the problems that were in mind. Nevertheless, I think the panel did a good job of clarifying to us, and particularly to those who are more interested in the administrative matters, that we must weigh up the advantages against the risk. I believe this is a very important point because there are always some who think it is possible to set an absolute level. We have now learnt today that there are many factors that must be taken into account when determining [action] levels and applying them.

A new Nordic authorities meeting was held on 9–11 January 1962, this time in Copenhagen. It was now time to discuss the long-term problems with radioactive fallout. A few work groups were set up to investigate these problems more closely.

One of these groups consisted only of physicists and was led by Thorleif Hvinden. Its task was to draw up a forecast for the radioactive contamination over the next few years. Those taking part in this group included Asker Aarkrog and Johan Ambrosen from Denmark, Kauno Salimäki from Finland, Arne Bull from Norway and Kay Edvarson and Torsten Magnusson from Sweden.

The other group was led by the Norwegian biochemist Lorentz Eldjarn. It consisted of physicians and biologists (with one exception – I was also part of the group). From Denmark there were Børge Christensen and *J. Schultz-Larsen* and from Norway, *Per Oftedal* and Erik Poppe. The Eldjarn group's report was not finished until the Nordic meeting, which was held in Stockholm in September 1963, but the physicists' report was published in the Danish *Meddelelser fra Sundhedsstyresen* [Notifications from the Board of Health], and as an Appendix to *Ugeskrift for Læger* [The Journal of the Danish Medical Association] number 114 of 23 March 1962. Based on the assumption that the fission yield in the nuclear charges that had been detonated in 1961 (it concerned 'hydrogen bombs' boosted with uranium) and a comparison with those which had been detonated in 1958, the following conclusion was drawn in the report, which was signed by Hvinden:

> It is assumed that the Soviet series of tests at Novaya Zemlya in autumn 1961, and earlier tests, will give an average dose during the course of 1962–1963 to newly-formed skeleton tissue of approx. 0.05 rem [corresponding to 0.5 mSv] and to bone marrow associated with such newly-formed tissue of approx. 0.02 rem [0.2 mSv] (or approx. 10 % of the dose from the background radiation in 1962–1963). The 30-year gonadal dose for a period starting on 1 January 1962 will be approx. 0.05 rem [0.5 mSv] or 1–2 % of the background radiation's contribution over the course of 30 years. The uncertainty of these dose estimates may be approx. ±50 %.

> Individual population groups in Scandinavia who live in areas that have particularly high amounts of precipitation or who have extreme dietary habits may be expected to reach doses of the same magnitude as those they receive from the natural background radiation.

In plain language, the ‘individual population groups’ referred to were the reindeer-herding Saami because their basic food was reindeer meat. In turn, the reindeer meat contained proportionately more primarily of long-lived strontium-90 and caesium-137 than beef, mainly because the reindeer needed to graze on larger and more contaminated surfaces than the cows because their main food (the lichens) more effectively retained the contamination contained in the precipitation and was less satisfying than the plants on which the cows fed. As was usual at the time, Hvinden’s group was mainly interested in the strontium, but we now know that it is caesium-137 which poses the greatest problems. In Norway, Saami in the Kautokeino area appear to have received body concentrations of approx. 600 Bq/kg of caesium-137, which gives an annual dose of approx. 1.3 mSv. In Sweden and Finland, the maximum caesium levels in adult Saami were approx. 7 000 Bq/kg, which corresponds to an annual dose of 15 mSv. For comparison purposes, it is worth mentioning that ICRP’s recommendation regarding a maximum permissible annual radiation dose for people in radiological work at this time was an average of 5 rem (50 mSv) over longer periods of time. The measurements that were simultaneously taken from personnel at Sievert’s institute in Stockholm showed at most approx. 20 Bq/kg. This was in 1964 as a consequence of the delay in the food chain – the explosions in 1962 did not contaminate the grazing pastures until spring 1963.

In April 1962, a meeting was held in Helsinki concerning radioactive contamination of foods and the ecological transport chains that were relevant. It was part of a series called ‘Radioactivity in Scandinavia’ (RIS), initiated by Jorma Miettinen and Kurt Lidén in close cooperation with Dietrich Merten of IAEA in Vienna.

In Sweden, the tangible radioactive contamination of foods had accelerated the Radiation Protection Committee’s work with instructions to the County Administrative Boards regarding action in the event of accidents in nuclear power plants. The Committee stated a number of action levels based on reports from the United Kingdom and the USA following the Windscale accident. The recommendation was that the stated actions would be taken as soon as it was feared that the action levels might be exceeded. The recommended levels were:

For evacuation:			
Whole body dose of gamma radiation		10 rad	[100 mSv]
Skin dose of beta or gamma radiation		50 rad	[500 mSv]
For foods to be discarded:			
Calculated future intake of	iodine-131	0.6 µCi	[22 000 Bq]
	caesium-137	6	[220 000 Bq]
Concentration in milk	strontium-89	0.2 µCi/l	[7 400 Bq/l]
	strontium-90	0.002 “	[74 Bq/l]
	iodine-131	0.07 “	[2 600 Bq/l]
	caesium-137	0.15 “	[5 500 Bq/l]

These recommendations were more or less forgotten when the Chernobyl accident occurred in 1986, although they did still formally apply, although having said that they would not have been fully applicable because the conditions were so different.

On 23 April 1962, the work group set up the by the Radiation Protection Committee submitted its report (on 87 stencilled pages with yellow cover and entitled ‘Coordination of radioactivity measurements from foods’). The report proposed a measurements programme during 1962. The group wrote (Strå, 1962):

> It appears to be absolutely clear that the current situation does not necessitate taking radioactivity measurements from foods as a direct protective measure, bearing in mind the radioactivity that our foods may be expected to contain in 1962. On the other hand,

> it seems highly desirable to take some measurements in accordance with a programme proposed here, bearing in mind future protection considerations.

The group explained that these measurements would be made using the scintillation spectrometers which had been obtained through the Radiation Protection Committee (and at Sievert's initiative) for Sievert's Institute of Radiophysics, the National Agricultural Experiment Farm and the Royal Veterinary College of Sweden. The device could not be used for direct protection measurements that had to be carried out in a critical situation on virtually all milk and all meat. Such measurements could be taken using significantly simpler devices at all food distribution centres. The planning of such measurements was outside the group's assignment.

The measurements that were now proposed would chart the considerably lesser contamination from the nuclear explosions in 1961 for the purpose of providing a basis for decisions on protection measures in any potential future situation with much heavier radioactive fallout. The existing device did not permit measurements of strontium-90 which, together with its radioactive daughter product yttrium-90, emits only beta radiation. The limited number of chemical analyses that could be taken to measure strontium-90 therefore had to be selected with care in order to give the greatest possible amount of information.

The group's recommendation was:

> The expert group suggests that radioactivity measurements be taken from foods in 1962 to a significantly greater extent than previously, but that it is made clear that this is not considered justified as an immediate protection measure but as a use of the temporary fallout maximum of long-lived radioactive substances which can be expected in April – May 1962 and which may also be expected to lead to an increase in the radioactivity in foods during the summer, as it did in 1959. It is proposed that the measurements be planned in such a way that, guided by the results, in the future there will be a greater possibility of deducing directly from primary measurements of the activity in air and precipitation, and without too comprehensive measurements of large quantities of foods, where it is most legitimate to initiate protection measures or more detailed control of the food distribution.

For this purpose, the experts proposed three types of measurement during 1962, i.e.:

> A. Measurements to clarify the connection between the activity in the precipitation and a number of consequences thereof, such as the activity in the soil, the activity uptake into the crop and the activity uptake in grazing cattle and thereby in meat and milk.
>
> B. General measurements of all essential dietary components. The main activity intake was expected to be from dairy products, grains and meat. By concentrating on these foodstuffs, random sampling of other foods would make sure no further essential activity transportation occurred.
>
> C. Routine measurements of dairy products, grains and meat to chart the contamination in different parts of the country and at different times of the year.

The group proposed that the type 'A' measurements be taken from samples from 85 test farms distributed throughout 13 areas within 4 parts of Sweden. A number of organisations had promised to assist with the collection of samples for the B and C-type measurements, primarily the Swedish Dairies' Association and the Swedish Slaughterhouse Association.*

It is particularly interesting that the group had calculated reference values for the activity concentration of, e.g., things such as caesium-137 in different dietary components, taking into account the relativity of their presence in a normal Swedish diet and chosen so that the total activity intake after

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* These organisations were active until the end of the 1990s.

a long period could be expected to give an adult 1/20 of the maximum body content that ICRP had recommended for an adult person in radiation work. This gave values between 1 000 and 20 000 picocuries per kilogramme (corresponding to 37–740 Bq/kg). With these reference values, the total intake of caesium-137 was calculated as 10 500 pCi per day (370 Bq/day), i.e., with current units an annual intake of 135 000 Bq of caesium-137.

The assumptions made were very pessimistic, however, and postulated that each food was contaminated up to the reference value, which was scarcely realistic. The group also gave reference values for children, but even here, the assumptions were based on what we now know to be unrealistic; one assumption was that the dose for the same activity intake would be significantly greater for children than for adults.

The Radiation Protection Committee followed the group's proposed measurement programme which was estimated to cost 115 000 Swedish kronor. The programme was carried out in 1962 and the results were reported to the Radiation Protection Committee in January 1963.

On 6 July 1962, as head of the FOA's department 4, Torsten Magnusson wrote to the relevant personnel at the FOA and forewarned them that the preparedness against radioactive fallout which had been established in autumn 1961, but had since pupated, might need to return to function with short notice. The collection point for the central management's personnel had then changed from the F8 premises in Barkarby to the Civil Defence Board's premises in Stockholm.

On 27 July 1962, Sievert received the following letter from the head of the Defence Staff Major General *Carl Eric Almgren* (1913–2001):

> Dear Professor,
>
> <u>Preparedness in the event of possible Soviet nuclear charge tests</u>
>
> It has now officially been stated that Soviet exercises in the Arctic Ocean will take place on 5/8–20/10/1962 within an area whose western borderline is closer to Scandinavia than the corresponding border to the previous nuclear charge tests.
>
> Previous Soviet test series have been preceded by a message stating the time and area of exercises. The first nuclear explosion has taken place close to the start of the exercise. It therefore seems not unlikely that Soviet nuclear charge tests can start as early as 5/8.
>
> Our military activity for radiac protection in the form of intensity measurement at the Air Force's permanent weather stations and during flight continues unchanged and we do not currently intend to increase it.
>
> Since the Expert Commission for Advice in the Event of Nuclear Accidents the Swedish Government should still maintain the task of monitoring radiation risks that may arise in connection with nuclear charge tests, I have chosen to bring the above to your esteemed attention. Corresponding information has also been supplied to the Civil Defence Board.
>
> Yours truly,
>
> Carl Eric Almgren

On 31 July, Sievert wrote to all the County Administrative Boards on behalf of the KRA, stating:

> In respect of information stating that the Novaya Zemlya test area used in 1961 has been barred as of 5 August 1962, from which time new Novaya Zemlya nuclear weapons testing thus ought to be anticipated, the Expert Commission for Advice in the event of Nuclear Accidents would like to submit that the County Administrative Boards control the personnel access within each preparedness organisation, particularly with regard to holidays that are taken. Available instruments will also need to be checked (calibrated) so that they can be put to use immediately. If necessary, the organisation's competence to function in other respects should also be analysed.
>
> There is currently no intention to introduce any form of preparedness for the organisations' personnel. The purpose of this letter is purely to remind you that preparedness with reference to new nuclear weapons testing at Novaya Zemlya may be required in the near future and at fairly short notice.

On the same day, Sievert wrote a corresponding to letter to the authorities and institutions which were involved in the 1961 preparedness organisation. At the same time, he wrote to those who were monitoring the Institute of Radiophysics' measurement stations and told them that for the foreseeable future they would be called each morning for information on the position of the measurement instrument's counter. Sievert also wrote to the SOS centre at the Swedish Telecommunications Administration, supplying an alarms list with the messages that should be read out for different levels of alarm. On 3 August, he issued a memorandum to members of the KRA who were summoned to a meeting at the Institute of Radiophysics in the afternoon of 7 August. By then, the first nuclear explosion of the new series had already taken place at Novaya Zemlya. Later compilations from UNSCEAR (UNSCEAR, 2000) show that the Soviet Union detonated 36 nuclear charges there in 1962, 8 of which had a blast strength corresponding to more than 4 million tonnes of trinitrotoluene (4 Megatonnes). The times of the latter explosions were:

5	August	21	Megatonnes
25	"	4	"
27	"	4.2	"
19	"	4	"
25	"	19	"
27	"	17.6	"
22	October	8.2	"
24	December	24.2	"

As in 1961, the explosions took place at high altitude and when winds were not blowing in the direction of Scandinavia. The short-lived radioactive substances fallout from the most powerful explosions was therefore insignificant while the long-lived substances such as strontium-90 and caesium-137 were spread throughout the northern half of the globe. In 1962, the Americans detonated some ten nuclear charges of megatonne strength (on Christmas Island and Johnston Island); the most powerful was approximately 8 megatonnes on Christmas Island on 27 June. The Americans also detonated about thirty nuclear charges of some kilotonnes strength in the Pacific Ocean area and the Russians forty or so such charges at Semipalatinsk. These smaller charges gave rise to a tropospheric distribution of short-lived radioactive substances, including iodine-131.

The explosion at Novaya Zemlya on 5 August 1962 was registered seismically by Markus Båth in Uppsala,[*] gravimetrically by Professor *Arne Bjerhammar* (1917–2011) at KTH in Stockholm and microbarographically by Bengt Hultqvist in Kiruna. In Uppsala, Markus Båth initially estimated the blast strength as 'at least 20 megatonnes', and later as 'around 40 megatonnes'. On 21 August, Sievert wrote to the Newspapers' Telegram Bureau promising that the Institute of Radiophysics and the FOA would provide information on measurement results each week with regard to the amount of radioactive substances in ground air, the fallout activity and the gamma radiation from the ground. Sievert drew up detailed plans for the alarm notifications for various people who would be part of the Swedish preparedness organisation in the event of actual danger.

On 22 August, Professor Mustakallio wrote to Sievert saying that Finland had set up 'a surveillance body whose job it was to prepare and lead the activity in the event of any disasters, bearing in mind the danger of radiation'. The Chair of this was Major General *Uolevi Poppius*. Mustakallio was Deputy Chair and the monitoring body also included Professor Salimäki.

The scientists and the people responsible for radiation protection were aware that the problems to which the radioactive fallout could lead would not arise until next spring when the global fallout of long-lived radioactive substances would contaminate grazing land. It was then that foods could be expected

[*] In Sweden, seismographs existed at Båth's premises in Uppsala and also in Kiruna, Umeå, Skalstugan (in Jämtland), Gothenburg and Karlskrona. Data was compiled in Uppsala by Assistant Professor Båth.

to be contaminated, firstly milk, then meat and grains. They still had approximately six months in which to prepare any action. However, the fallout of short-lived substances such as iodine-131 had largely already occurred.

The KRA met once again on 15 September. On the previous day I had been asked to assist the Commission as an expert and had given an account of the ongoing measurements of iodine-131 in dairy milk and in milk from individual farms in the previously-agreed measurement programme. The most powerful iodine contamination had not yet been measured; it took place in September 1962. The KRA also discussed protection measures and action levels in preparation for the next Nordic authority meeting which would take place in Oslo the following week.

At the meeting in Oslo, the report from Hvinden's work group regarding expected doses of radiation from the nuclear charge tests was available. The following was written in the report from the meeting to the Nordic countries' healthcare authorities:

> Currently, the most important consequence of the new explosions is that measurable quantities of ^{131}I have occurred in milk. The impacts that this may have from the health point of view as well as the possible actions that may conceivably be prescribed were therefore the main subject of the meeting
>
> […]
>
> Radioactive iodine is considered to constitute a risk mainly to children under the age of 1. This is partly because the children's most important food, milk, is also the most important source of radioactive iodine, and partly because their thyroid gland is small and is therefore exposed to proportionately larger doses of radiation than is the case in adults. It is known that large doses of radiation in the thyroid gland can lead to cancer. However, it is unlikely that the radioactive fallout which is now expected will lead to any cases of thyroid cancer within the Nordic countries. However, since you can never discount the fact that risks may exist at higher doses of radiation, the measures that may be relevant to reduce the uptake of ^{131}I were discussed.

The discussion showed that all protection measures for the purpose of reducing the children's intake of radioactive iodine also led to risks because intervention against the milk could have unfortunate impacts on the children's nourishment. Additives of ordinary, inactive iodine could fully or partly block the uptake of radioactive iodine in the thyroid gland, but the experts were hesitant about this type of recommendation, bearing in mind possible oversensitivity reactions. The most effective measures would be to place grazing animals in stalls; the substantial fall in the level of iodine-131 in milk in autumn 1961 was due to the very fact that the grazing season had ended. The use of dry milk for children was thought to be beneficial but could also lead to risks such as the use of unsuitable water.

An expectation that the sum of the daily values for the iodine concentration expressed in nanocuries per litre of milk would exceed 200 if no action were taken was proposed as an appropriate action level. If the value were 200, this would correspond to a total thyroid dose of 5 rad (corresponding to 50 mSv). For a consumption of one litre of milk per day, the number '200' indicates a total intake of 200 nanocuries. If the activity concentration is stated using today's units, i.e., in becquerels per litre, the comparison figure for the total intake is not 200 nanocuries but 7400 becquerels. However, if the iodine contamination were to reach these values, it would not necessarily lead to measures but rather to considerations as to whether measures would be suitable in the given situation. The thyroid dose that formed the basis for these thoughts, 5 rad, was significantly lower than the dose (25 rad) used when calculating previous levels at which action should be taken.

The KRA met again on 21 September, this time to look at what had been discussed at the authority meeting in Oslo earlier in the week.

At the end of the year, it became obvious that contamination though iodine-131 had been at its highest at the end of the grazing season. The maximum content of iodine-131 in milk from any of the test farms (it concerned a farm in Norrbotten's mountainous area) had been 2 000 picocuries per litre (approx. 75 Bq/l) when measured in September and perhaps 50 % more when the milk was fresh. At the time, we estimated that this would have given adults a radiation dose of 2 mSv in the thyroid gland and children 20 mSv. The margin to the action levels (0.07 microcuries per litre, i.e., approx. 2 600 Bq/l) which the

Radiation Protection Committee had recommended to the County Administrative Boards earlier in the year was considerable, even if you took into account that the recommendation applied to a one-off emission whereas the contamination through iodine was now coming from repeated nuclear charge tests. Even the lower action level which had been recommended at the Oslo meeting in September had a good margin. The contamination through iodine-131 in 1962 was therefore deemed not to have made the milk unsuitable to consume anywhere in Sweden.

The average content of iodine-131 in Swedish dairy milk was also at its highest in September, but was then scarcely 300 picocuries per litre (approx. 10 Bq/l).

The milk was also a critical food when it came to the long-lived radioactive substances strontium-90 and caesium-137. Due to the absence of gamma radiation, strontium-90 could be measured only by means of chemical analysis of cremated samples, while caesium-137 could easily be measured using scintillation spectrometry. However, the FOA scientists Kerstin Löw and Kay Edvarson had already shown in September 1958 that there was a connection between the levels of strontium-90 and caesium-137 not only in soil samples – as would be expected – but also in milk. In areas with similar conditions, it is therefore possible to use the caesium-137 measurement results to estimate the amount of strontium-90 provided the source of the contamination is nuclear charge explosions rather than a reactor accident.

At Sievert's institute we had measured caesium-137 in the bodies of a group of personnel since 1959 and in dairy milk since 1958. In 1963–1964, regular measurements were taken from milk from 32 Swedish dairies. The following Figure shows the way in which the contamination reaches peaks during the grazing season and its lowest values during the winter when the animals are in stalls. You can also see the impact of the temporary test ban in 1958. The 1958 test explosions certainly did cause a maximum activity peak in 1959, but the contamination then fell until the 1961 test explosions led to a substantial increase in 1962, followed by an even higher value in summer 1963 as a consequence of the major test explosions in 1962. Because the 1963 test ban treaty between the USA and the Soviet Union forbade nuclear charge explosions above ground, the contamination then fell substantially until Chinese test explosions in 1967–1970 (3 megatonnes per year) may have led to a certain increase. The level of caesium in Swedish foods was then low until the 1986 Chernobyl accident led to a fallout of caesium which again caused a situation similar to the one at the start of the 1960s.

Understandably enough, the radioactive contamination caused concern. Radioactivity and radiation have never been everyday, well-known concepts. Lack of knowledge of the concepts sometimes led to protection measures which could either be ineffective or lead to harmful consequences. Those who were experts understood that the risks were completely insignificant compared to all other risks to which we are exposed on a daily basis but, faced with an inflicted risk, many people react as though their lives would otherwise be free from risks and the new risk is the only thing that is important. If asked what the likelihood was of dying over the next year, most of us would probably accept the accuracy given by the answer 'one in a hundred' [*]and not demand an impossible level of accuracy of '1.0016 per hundred'. Rightly or wrongly, we in radiation protection circles thought that we as individuals ought to feel secure provided that extra risks did not tangibly increase our total annual likelihood of being injured or dying. When it came to the radioactive fallout from the nuclear charge tests at the start of the 1960s, we could therefore have a clear conscience in attempting to pacify people by saying that they did not need to be concerned. Unfortunately, as we learnt, there are few statements that worry people as much as the classic 'there is nothing to worry about'.

However, paradoxically enough, for the people who are responsible for protection, it is not enough that no-one needs to worry about the risk to themselves. This is where ethics plays a major role in the approach. Although we may feel secure, we may feel uneasy in the knowledge that someone else may come to harm. An extra annual one in a million likelihood of death ought not to make us worry about our own security. Nevertheless, this would cause ten or so deaths each year in Sweden. In the 1960s, we began to think that such a possibility ought to be taken into consideration even though no individual ran any obvious risk. In addition to the risk of individuals, we ought also to look at the expected total damage.

[*] The annual probability of death in Sweden is as much as one in one hundred for men around the age of 60 and ladies around 65.

On 29 October 1961, *DN* ran an article which I had called ‘Fear of radiation with sense and moderation’. I wrote that scientists often thought that the fear in the face of the ‘Doomsday Bombs’ could largely be blamed on the attitude of the press and sensationalist journalism, but that the scientists themselves also had ample responsibility in not coming forward and remedying obvious misunderstandings. I continued:

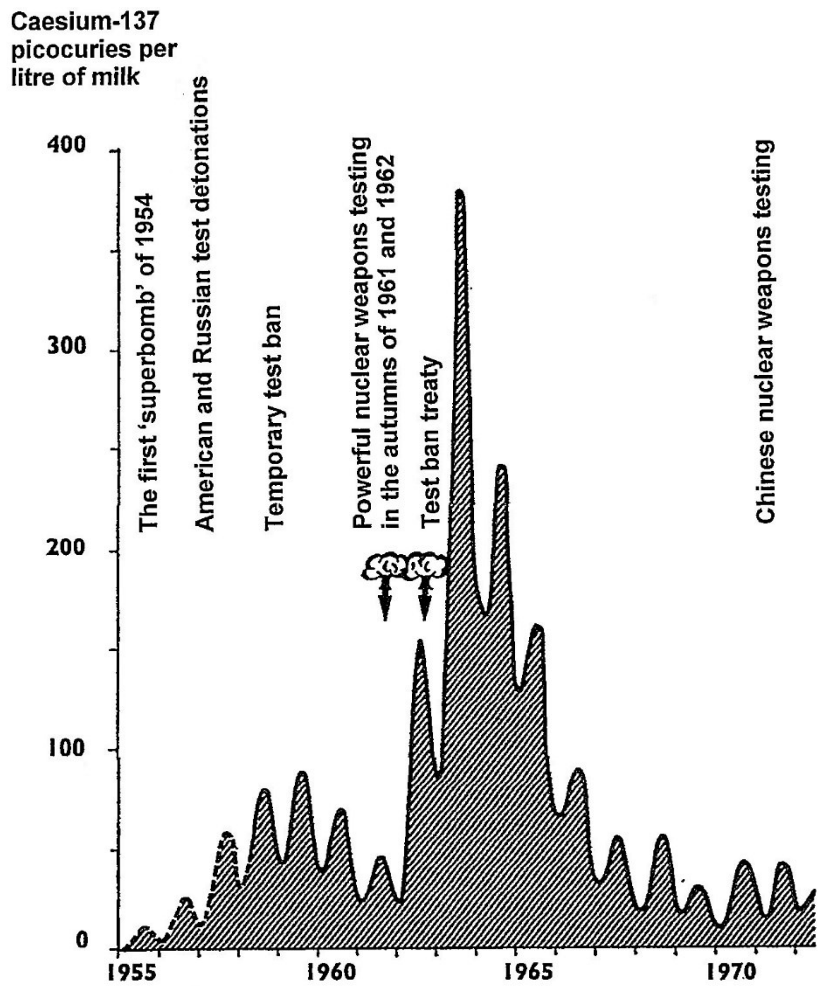
> One of the reasons for their silence may be that most scientists who might be able to contribute information do not realise how afraid many people actually are at the moment. It is a fact that ‘radiation’ has become a simplistic concept, a symbol of horror and death, like the plague. While it should be made clear that nuclear weapons can expand a forthcoming war to a destruction of our civilisation which defies all description, it must be made equally clear that the small doses of radiation such as those we have experienced thus far from nuclear weapons tests and other contamination of the surroundings do not give grounds for any person living in Sweden to be worried about his own health or that of his relatives.

I then gave an account of what we knew about the impact of high and low doses of radiation at the time and concluded the article with:

> But why are people worrying so much? Is the whole radiation debate meaningless and should it be completely limited to the high doses of radiation, to the risk of war and the consequences thereof? No. Is all worry not equally emotional and actually exaggerated, like the fear of thunder or the hope of a maximum win on the Lottery? Most, but not all. Multiplied by the millions in the population, even a very low risk leads to a number of victims. With the risk numbers mentioned above, a ‘Doomsday Bomb’ could possibly cause some tens of cases of leukaemia for Sweden, maybe as many cases of other types of tumour as well as perhaps a hundred or so cases of genetic afflictions. Much of the most heated radiation debate has concerned whether, counted over millions of people, the lowest doses of radiation may claim a few victims, and whether the mere *possibility* of this would mean a powerful argument against nuclear weapons testing. This is a political, ethical and to some extent philosophical issue which certainly must be openly discussed and which is worthy of all the attention. However, it is important that it is not discussed in such a way that the individual citizen believes that it concerns his life and the lives of those closest to him. It concerns only his conscience.

My article caused sensation in different directions. The thing that pleased me the most was a letter from a young mother, who wrote:

> Thank you for the article in today’s *DN*. My level of gratitude is actually beyond description. Ever since the ‘new’ Russian tests began I have ploughed through every article, listened to every message and finally, not usually being someone who tends to become hysterical, become scared out of my wits and started to think that there is no future. Have a 5-month-old baby and



The level of caesium-137 in Swedish dairy milk as a consequence of the nuclear weapons testing
From Lindell & Löfveberg (Lindell, 1972)

> read with horror that in England people are buying up dry milk stocks since children under the age of one do not tolerate the 'radiation' contained by the milk following the tests that have already been done. Now have a stock of calcium tablets and ready-made children's gruel which would last until my child turned one.
>
> After having read the article, what can I do but laugh at it all and go out to enjoy the beautiful Sunday weather with a wonderful feeling of relief.
>
> Dear Licentiate Lindell, *do* also let those who do not read *DN* know because I now know how a layperson can put together crazy ideas on the basis of too little information.
>
> With my conscience intact,
> (signature)

Less appreciative were three Assistant Professors at Uppsala in biochemistry, *Per-Åke Albertsson* (1930–2018), *Hans G. Boman* (1924–2009) and *Bo Malmström* (1927–2000), who refuted my article with a response under the heading 'Constructive fear' in *DN*. All three were capable scientists within their field and each later held Professorships in Umeå (later Lund), Umeå (later Stockholm) and Gothenburg respectively. My critics thought that I had underestimated the consequences of the nuclear weapons testing and that it was 'obvious that a fear of the consequences of the radiation may be constructive as it creates a basis for political argument and social measures'.

I had the opportunity to respond to this criticism in *DN* on 2 November, and was then able to demonstrate that my critics had misinterpreted their sources regarding the risk of radiation. The rejoinder written by the three Assistant Professors was that 'the numerical examples have never been the most essential part of our criticism'. They concluded with:

> In Lindell's eyes, radiation which can be expected to lead to a hundred defective children in Sweden is something that 'concerns only his conscience'. This is an objectionable wording which, whether personal or not, is made even clearer in that it is repeated by an employee of the country's radiation protection bodies. In our eyes, the total risk figures to which nuclear explosions that have already been carried out can be expected to lead – figures which we agree on – stand out as an appalling reality. All measures that can be taken to prevent harmful effects from continuing are of a scope requiring political decisions. The fear of radiation which leads to the formation of opinions is therefore constructive.

DN gave me the chance to reply on 9 November. I wrote that purely the *possibility* of serious injuries all over the world was reason enough to incite indignation without also having to use 'an understandable but uncalled-for fear regarding personal health risks'. I concluded with:

> Based on generally-accepted risk figures, I have pointed out that no Swedish person has any real reason to be worried about his own health or that of those closest to him because of either the global radioactive fallout to which we are currently exposed or that which we can probably expect to occur. My opponents think that this worry, although actually unfounded, should be upheld for political reasons to stir up an opinion against nuclear weapons. I have said that although the possible risks owing to the nuclear weapons testing, as I hope to have shown, do not give grounds for personal concern, they do constitute a serious ethical problem which touches our conscience. The interpretation by my opponents that it is offensive to call the problem a matter of conscience reveals an inadvertent but essentially quite appalling disregard for the individual's capacity to feel a social responsibility, even in cases where he does not experience a personal fear.

Sveriges Radio had also cottoned onto the worry about radioactive fallout and took the opportunity to pitch me and Lars Gyllensten against one another in a radio debate on 31 October. A year earlier, Gyllensten had shown that he did not trust experts' attempts to put radiation risks 'in the right perspective' and had had a heated debate in *DN* with *Vattenfall's* radiation protection expert, engineer *Carl-Eric Holmquist* (1921–2001) with regard to acceptable doses of radiation for the public if the nuclear power plant at Marviken were to come to fruition. The programme leader probably expected an equally heated debate between Gyllensten and me but may well have been disappointed. I repeated that people did not need to worry about their personal health. Gyllensten was against using only the individual's perspective: 'Looking at the whole world, the situation is serious though.' I agreed that we ought to protest 'a great deal and loudly' against the bombs. And Gyllensten agreed that 'as things stand today, people should not be concerned'.

My article in *DN* was also noticed appreciated abroad. In *Time* on 10 November 1961, I was quoted as though I had said in an interview that 'nobody needs to worry about the global fallout from the nuclear weapons testing'. I wrote to the editor to say that I had not been interviewed and that the quote concerned my article in *DN*. It was literally correct but it did not say that I had said at the same time that the consequences of the test explosions probably led to considerable injuries which demanded discussions, studies and concern.

Rolf Björnerstedt, who worked at the UNSCEAR Secretariat in New York at the time, read the same quote in *Life* and wrote to me immediately:

> Brother, Life does bring surprises but it really was quite something to read your statement in *Life* on 10 November. You are in good company with Eisenbud and Comar* and other politicians but a disclaimer would not be out of place, if for no other reason

* Merril Eisenbud and C. L. Comar were American scientists who Björnerstedt thought had belittled the consequences of nuclear weapons testing.

> than at least because I feel compelled to 'worry about the global fallout from nuclear tests'. Otherwise there will probably be no report next year.

Rolf was relieved when he read the *DN* article and understood the context. On 21 November, he wrote:

> Thank you for your letter! It was interesting to read your *DN* debate. If you have a copy of their responses to your answer, I would be interested in the opportunity *[to read it]*. Otherwise I can only say that I thought your work was sensible and well-written. If they cannot read from the paper in U-a, they really should take things a bit more slowly. For instance, why not a letter from them to you before putting their foot in it? The whole thing is a warning to you with regard to the risk of becoming an assistant professor! Ask me. I don't think you need to take the *Time* quote so hard. It certainly has been translated verbatim, but it was bewildering taken out of context.

One of the people who would later play an important role in the radioecological research was *Gun Astri Swedjemark*. Gun Astri was employed by Sievert in April 1962 and her first task was to continue the experiment with fruit flies which had so nearly cost the then Crown Prince his life. Sievert now wanted to repeat the experiments with a low radiation environment by performing them in a laboratory at the Henriksdal treatment works. The experiments brought no result and the continuing Soviet nuclear weapons testing at Novaya Zemlya occupied most of Sievert's interest. Swedjemark had to leave the fruit flies to concentrate on the measurement stations instead which Bengt Håkansson was responsible for operating. The situation became particularly pressing when the Institute of Radiophysics received many questions about the measurement results evaluated by Gun Astri.

Radioactive fallout was not the only thing that caused radioactive contamination in the environment. Arne Hedgran wanted to know whether the discharge of radioactive substances from the hospitals could lead to problems. For example, patients were given large activities of iodine-131 to treat thyroid illnesses, and much of this iodine ended up in the hospitals' toilet bowls and then out in the wastewater mains. Hedgran asked me to help with an analysis. We obtained a map of the wastewater system's labyrinths and found that what had been released into a toilet bowl at *Radiumhemmet* eventually reached the treatment works at Ålkistan, the narrow sound between Brunnsviken and Lilla Värtan. We flushed approx. 100 curies of iodine-131 down one of *Radiumhemmet's* toilets, armed ourselves with sensitive measurement instruments and began to follow the map. The walk instilled great fear in us. The overloaded wastewater system was already flooding by Solnavägen and leaving traces in a ditch which must once have been a brook running in the direction of Ulvsundasjön. Toilet paper and other hospital waste was floating along there, but we found no measurable radiation. The greatest shock hit us when we followed the wastewater pipe around Brunnsviken to what is now the Ulriksdal junction. This is where the pipe could no longer cope with its load, and wastewater flooded out and the uppermost, greasiest level flowed out into Brunnsviken, which in this area was covered by an oily, ill-smelling coating. But we were also unable to measure any radiation here.

When we arrived at the treatment works at Ålkistan we found that this could not cope with the flow of wastewater either. There were a number of sedimentation basins there arranged in stairway formation, but the wastewater was not able to rest and sediment out - it flowed, full-bodied, over the edges of the basins and out into Ålkistan having undergone little treatment.

We stopped here and took water samples at regular intervals from one of the basins in the hope of being able to detect the discharged iodine when it appeared. We succeeded in showing its arrival but activity of the iodine was scarcely measurable. The iodine had probably become stuck along the way and would have had time to disintegrate in the wastewater system before any important activity reached Ålkistan.

Not long after that I was invited to lunch by *Dagens Nyheter's* Ingemar Wizelius. He wanted me to meet one of his colleagues, *Olle Alsén* (1923–2011), who had just become editorial writer. Or should I say perhaps say that he wanted Alsén to meet me. I found Olle Alsén full of suspicion where the authorities' and experts' opinion on the radiation risks were concerned. He seemed eager to start a warning campaign. I maintained that while the impacts of the nuclear weapons testing in the form of the number of possible cases of cancer was substantial and needed to be criticised, the personal risks to people in Sweden were very small. I was more disturbed, I said, about the toilet paper on Solnavägen, the layer of fat at Brunnsviken and the untreated wastewater flow in Ålkistan. But Olle Alsén seemed to think that this was actually an aesthetic problem rather than a deadly threat like the radioactive fallout.

In 1962, the Nordic radiophysicists met for the second time, this time in Oslo in well-organised groups with Berndt Waldeskog from the General Hospital in Malmö as Chair.

Of the four 'Nordic Council-type' expert meetings that were held in 1963–1965, the first two took place in Stockholm in 1963, on 7–9 March and 5–6 September to be precise. At the September meeting, the Eldjarn group's report on the biological risks of the radioactive fallout was presented. The experts then met in Oslo on 17 April 1964 and in Reykjavik on 1–3 July 1965, which was the final meeting in which Sievert took part. There was now radiation protection activity in Iceland, similar to what had happened previously in the other Nordic countries in connection with a medical physicist's activity, where the radiophysicist responsible was *Gudmundur Jonsson*.

In 1964, *Karolinska Sjukhuset's* preparedness for 'atomic accidents' was discussed at Sievert's institute with Assistant Professors Jerzy Einhorn from *Radiumhemmet* and *Lars Engstedt* (1920-2004) from the medical clinic respectively. The consequence was a memorandum regarding the hospital's handling of radiological accidents. In 1965, Risø also had a preparedness plan which included action levels for the limitation of the use of foods contaminated with radioactivity. The action level for caesium-137 in milk was set at a value corresponding to 5 500 becquerels per litre in the current unit, i.e., the same as was recommended in Sweden based on the British recommendations following the Windscale accident.

During his final years, Sievert worked ambitiously with his own preparedness report in which he summarised experiences gained thus far. This report was never published. In 1966, the Medical Board issued its 'Notification number 114', *Radioactivity and radiation injuries*, written by Bertil Åberg (Medi, 1966). The small, 13-page work contained essential information divided among eight sections:

1. Risk situations that may call for medical intervention
2. Statutes and responsible authorities
3. Radiation injuries to people
4. Recommendations regarding maximum permissible doses of radiation and activity concentrations
5. The activity concentration – radiation dose connection
6. The application of maximum permissible levels in Sweden
7. The possibility of the doctor to assess the level of risk in a given situation
8. What the doctor can and ought to do.

17. RADIOECOLOGY, RADIOLOGY, AND RADIOLOGICAL PROTECTION

The Italian radiophysicist Arrigo Cigna has given a good outline of the birth of the new science called 'radioecology' (Cigna, 1996). The concept of 'ecology' started to be used in 1866 by the German Zoologist *Ernst Haeckel (*1834–1919) to designate the relation between living organisms and their surrounding environment. It was primarily a number of Russian scientists, including *G. G. Polikarpov* and *Vladimir Vernadsky* (1863–1945)*, who introduced the new fundamental concepts, as their ecological research also covered natural radioactive substances. In 1929, Vernadsky introduced the concept of 'concentration factor' for the ratio of the concentration of a radioactive substance in a given organism and its concentration in the water from which the substance has been absorbed by that organism. However, the information is based on the fact that Vernadsky was the first person to use the word 'radioecology' in 1935 in error (the word 'radiogeology' in a book title was misread as 'radioecology'), so the identity of the first person to use the word 'radioecology' is still unknown.

The first test explosion of a nuclear weapon, the Trinity test in the Alamogordo Desert in New Mexico on 16 July 1945, had already led to a distribution of radioactive substances which were not only detectable but also disruptive. Cardboard for cartons that were used by Eastman Kodak to package x-ray film was contaminated by radioactive particles at the time of its production in Vincennes in Indiana 1700 km from the explosion site. This caused spots on the film where it had been blackened by beta radiation from the particles. An analysis at Kodak's research laboratory showed that the source of the radiation was probably cerium-141, one of the most common fission products.

The early American findings, which also included measurements carried out by the AEC's New York Operations Office, were kept secret, as was everything concerning nuclear weapons, but they constituted the introduction to increasingly comprehensive measurements of radioactive substances in the environment in other countries which were carried out at an early stage in Canada, France, Italy, Japan, Germany and Hungary. It was soon realised that radioactive substances could be distributed over large distances and cause concerns not only through external gamma radiation but also through absorption by plants and animals and continuing transportation through the food chains to people, whose various tissues and bodily organs were then irradiated by radioactive substances. Radioecology as a science was born around about 1950.

The radioactive substances that are formed at the time of a nuclear explosion are distributed in the atmosphere and, after a while, reach the ground as radioactive fallout. Three types of fallout are usually mentioned: local, tropospheric and stratospheric.

The *local fallout* consists of relatively large particles which, due to gravity, fall to the ground early in the direction of the wind and cover a cigar-shaped area, the 'dispersion cigar'. Because the fallout occurs early on, it contains a large share of short-lived fission products. The greatest health risk is caused by inhalation and by gamma radiation from fallout on the ground.

The *tropospheric fallout* consists of smaller particles that can float in the air for longer and be carried for long distances with the wind. It usually falls to the ground by being washed out by precipitation. The radioactive substances can be carried around the globe in 2–3 weeks. North of the 30th parallel of latitude, the direction of the wind is usually from west to east but closer to the equator it blows in the opposite direction. The most short-lived fission products have time to disintegrate into non-radioactive nuclides before the fallout reaches the ground. Bearing in mind the health risk, iodine-131 is the most important

* See 'The Sword of Damocles' for information on Vernadsky.

remaining nuclide. During the grazing season it is sufficiently long-lived to be taken up by grazing cattle and contaminate the milk.

At the time of a nuclear explosion, fission products are created in the form of pairs of nuclides with mass numbers which are approximately half of the mass numbers of uranium. If the one nuclide in the pair has a mass number that is slightly less than this half, the other nuclide has a correspondingly larger mass. The half-life of the different fission products can be anything from fractions of a second to many years. Because the activity of a nuclide with the same number of atoms is inversely proportional to the half-life, the activity of the long-lived substances is proportionately small and the short-lived substances such as iodine-131 occur with greater activities.

It is different with the radioactive substances that can be dispersed if a reactor core is damaged. For each short space of time that the reactor is in operation, fission products form in the same proportions as at the time of a nuclear explosion, but the short-lived ones disintegrate gradually so that their activity can build up to only a certain level which is proportional to the thermal power of the reactor. However, the long-lived nuclides accumulate over time so that their activity constitutes an increasing share of the total activity and become proportional to the total thermal energy that the reactor has had time to develop during operation. Therefore, long-lived nuclides like caesium-137 and strontium-90 are more important in a reactor core than in the local and tropospheric fallout following a nuclear explosion.

The *stratospheric fallout* consists of very small dust particles which have been forced up into the stratosphere at the time of a nuclear explosion, i.e., past the layer, the tropopause at an altitude of approximately 10 kilometres, which separates the lowest layer of the atmosphere, the troposphere, from the stratosphere. In the stratosphere, these particles can float around for years before reaching the troposphere and then causing a global ground deposition.

The radioactive fallout which people began to notice in 1956 was a global contamination of the environment, but for the first time a contamination of the environment which was easy to observe through measurements of the radiation from the radioactive substances. It was also possible to directly measure the gamma radiation from the human body. In April 1960 a symposium, Radioactivity in Man, in which Kurt Lidén from Sweden participated was arranged at Vanderbilt University in Nashville in Tennessee where a number of lectures were held on measurements of radioactive substances in the human body. The American participants included Vic Bond from Brookhaven, Marshall Brucer from the Oak Ridge Institute of Nuclear Studies, Charles Dunham and Merril Eisenbud from the AEC, *Louis Hempelmann* and John Hursh from Rochester, Wright Langham from Los Alamos, Leonidas Marinelli and John Rose from Argonne, and Karl Morgan and *Walter Snyder* (born in 1909) from the Oak Ridge National Laboratory. A well and truly prominent crowd.

High concentration factors worried many and were often used as an argument in debates regarding the fallout from the nuclear weapons testing and regarding emissions from nuclear reactors. In the hearing that was arranged in June 1957 before the Committee on Radiation which was set up in the Joint Committee on Atomic Energy, Dr. Roger Revelle, head of the Scripps Institute of Oceanography, said when asked about concentration factors (USCo 1957, p. 542):

> […] the vertebrates, which in the sea are primarily fish, do concentrate some substances to a much higher degree than the plankton which they eat, and this is particularly true of zinc. In the case of strontium, the fish discriminate against strontium by about a factor of three. In the case of iron again there is a very considerable discrimination in the vertebrates as opposed to the invertebrates. On the other hand, the fish do concentrate phosphorous by a factor of 2 million whereas the invertebrates never go above 10,000. So it is not possible to make any generalization. We have to investigate every element – not every isotope – independently. This depends on how the organism absorbs the material, whether it comes to it by simply physical absorption or in its food supply and whether he needs the substance for growth or for his vital processes.

The environment had been radioactively marked so it was possible to study ecological distribution processes that had previously been inaccessible. The new science of radioecology had been born. The necessary measurement instruments had just been developed: ion chambers, proportional counters, scintillation spectrometers and chemical analysis methods. The research was not just of academic value;

they wanted to know what to expect if a nuclear war were to break out. They found early on that measurements from milk were particularly valuable. Grazing cattle accumulate contaminated crops from large surface areas and it was therefore possible to show important radionuclides such as iodine-131, strontium-90 and caesium-137 in the milk. Measurements from milk rapidly provide information on environmental contamination and the milk is also an important basic food, particularly for children for whom people want to provide extra protection.

Direct measurements of air activity and fallout in Sweden were taken early on by the FOA while the radioecological research took place at Sievert's institute and a number of university institutions, primarily the Radiophysics Institute at Lund University where Kurt Lidén and colleagues ran comprehensive studies of the lichens – reindeer – reindeer-herding Saami food chain. Both the FOA and Sievert's institute supported the radioecological research. With the support of Torsten Magnusson, Sievert procured funds to purchase scintillation spectrometers from Lars Fredriksson at the National Agricultural Experiment Farm and Bertil Åberg at the Royal Veterinary College of Sweden for the measurement of crops and foods.

In the 1960s, (the FOA, *AB Atomenergi*, the Institute of Radiophysics at Lund University and Sievert's institute) measurements of the level of caesium in the human body were taken at several institutes. Kurt Lidén cooperated with Jorma K. Miettinen in Finland with regard to measurements of the Saami population and took comprehensive measurements in the field.

In August 1961 we invited a number of Saami to Sievert's institute to measure their level of caesium-137 because Saami are the group who run the risk of taking up the greatest quantities of radioactive caesium if contamination is distributed in the environment. Because the reindeer graze on lichens over large areas they, like elk and roe deer, take up high levels of caesium, but when it comes to the reindeer, the meat constitutes the basic food of many Saami who can thereby end up with high body concentrations of radioactive caesium.

In this case, the measurement of the level of caesium was easy and involved no discomfort for the twelve Saami who turned up from Övre Soppero. It was easy to measure the gamma radiation in the whole body scintillation spectrometer that was in 'The Pit' beneath the high voltage hall. *Aftonbladet* took note of the event and wrote:

> The leader of the group is *Per Henning Nutti*, a gentleman who knows more about reindeer and reindeer herding than can be picked up from many books and is purely scientifically interested in the discoveries that will be made from the analysis that has now begun.
>
> He has personally travelled to Stockholm several times, but this is the first visit for the others and they hope to find time to visit the city's sights before leaving.
>
> It will be an exotic feature of the city since almost all Saami wear their national folk costumes.

Particular attention was paid to the fact that the married couple *Olle* and *Febe Blind* had taken the opportunity to make the visit to Stockholm during their honeymoon. None of the Saami were worried about the measurements. They demonstrated a sober and wise view of the problem: 'We understand from what you have told us that this is not particularly dangerous to us, but if the fallout were worse, what could happen to us then?'

In stark contrast to the twelve Saami's grasp of the situation and sensible conduct were the stereotypes who met us when I was about to help the group check into a Stockholm hotel. The hotel refused to take them at first and, following my strong protests, they were offered one large, common room with the comment: 'They might be used to sleeping on the floor, but that does not mean they can light fires in the room!' Per Nutti, who saw how indignant I was becoming, drew me aside when I wanted to continue protesting. 'This isn't worth quarrelling about,' he said with calm and determination, 'we don't want to cause difficulties. The room will have to do.'

In Finland, analyses of the radioactive fallout had begun in 1959 as a project funded by the Atomic Energy Commission. Jorma K. Miettinen, who still did not have his own institute at the time, led the analyses together with Professor of Inorganic Chemistry at the University of Helsinki (*Näsänen*). Olli Paakkola was the first research assistant in the project and was succeeded in 1961 by Anneli Salo.

Cooperation was established with a number of institutions in addition to the Institute of Radiation Physics, including the Agricultural Research Centre and *Bundesforschungsanstalt für Milchwirtschaft* [the Federal Research Centre for Dairy products] in Kiel where Dr. Dietrich Merten had taken a particular interest in the situation in the northern regions. The cooperation with Merten continued because he secured a job at IAEA in Vienna. When the Soviet Union resumed its nuclear charge tests at Novaya Zemlya in autumn 1961, Merten was quick to arrange a meeting with mainly Nordic scientists to discuss the lichens – reindeer – Saami food chain. That autumn, Kurt Lidén's research group measured caesium-137 in Saami from the Jokkmokk region and cooperated with Miettinen in studies of radioactive caesium in Finnish Saami from Inari.

The levels of caesium in foods proved to be low in regions with fertile soils but higher in poorer soils such as in the mountainous areas. The research into these connections was carried out by the knowledgeable Lars Fredriksson, who was an internationally-renowned expert in the importance of soil types. Caesium-137 could easily be measured through the gamma radiation but strontium was more difficult to measure. Samples had to be combusted, the ash dissolved in nitric acid and strontium-90 and its daughter product yttrium-90 separated using chemical methods, whereupon the beta radiation from yttrium-90 could be measured. The need for strontium analyses meant that we employed research engineer *Jorma Suomela* (1939–1998) as a chemist in 1963. One of Jorma's major achievements was that he developed a faster method for analysing strontium-90. In doing so he cooperated with the Radiation Protection Institute's information manager Sven Löfveberg, who was also a chemist, and they both received a grant from Folksam for this purpose.

The measurements of caesium-137 in milk were easier. The milk could be poured into a Marinelli beaker, an aluminium container with a cylindrical inverted well at the bottom so it could be strung over a radiation-sensitive sodium iodide crystal. The crystal with the aluminium container stood in a lead box with thick lead walls to shield against gamma radiation from the surroundings. An equally thick lead cap ran on rails and could be taken aside when you lowered the container with the milk sample.

The light scintillations that occurred when the gamma radiation from the milk sample met the crystal were captured by a photomultiplier and transformed into electric impulses which could be sorted according to height in a pulse height analyser. The number of impulses in the different height intervals was printed out on paper in an automatic typewriter which produced a ghost-like print without being touched by any hands.

We soon found out that it was easier to process the printed record if, when reading it, we covered it with a sheet of cardboard with a hole punched out for the height intervals corresponding to the gamma-ray spectral lines for caesium-137 and the most important background radiation. It was then easy to add the registered numbers to each 'window'.

Initially, we were interested in what the level of caesium-137 had been at different times before we had started to take fresh milk samples. We then collected dry milk from known periods of time.[*] Later on we measured Stockholm milk before starting to take regular measurements of milk from twelve dairies in different parts of Sweden twice a week in autumn 1961, the northernmost milk coming from Luleå. The routine analyses were gradually extended to cover more and more dairies.

In addition to the measurements from milk, we measured mainly basic foods for which a low concentration of the contamination can be more significant than a high concentration in something such as a spice which is consumed in only small quantities. My main food-measurement colleagues were the ingenious engineer *Nils Hagberg* and physicist *Attilio Magi*, who later became a medical physicist.

At the Royal Veterinary College of Sweden, Bertil Åberg measured the level of caesium-137 in beef and pork. It was possible to show a connection between the levels in milk and the levels in meat, which enabled the use of the rapid milk analyses to predict the contamination in the most important foods. The levels of caesium-137 in different crops were measured by Lars Fredriksson in Uppsala and could be

[*] In Canada, David Sowby succeeded at this point in time in obtaining, from the Polar Research Institute which was named after *Robert Falcon Scott* (1868–1912), for the same purpose a container of dried milk from Scott's expeditions to the South Pole. Sowby writes that 'It was a wonderful feeling to hold the container in my hand' (Sowby, 2001).

correlated with the radioactive fallout over different soil types. In the end, the whole of the fallout – ground – crop – grazing animals – foods – human body transport chain was very well charted. The level of caesium-137 in the human body was measured at Sievert's institute and the FOA, *Atombolaget* (where facilities in Stockholm and Studsvik were called 'Hugo') and the Institute of Radiophysics in Lund, which also took measurements in the field.

One person who appreciated Sievert's inputs was Lars Gyllensten. He writes in his memoirs (Gyllensten, 2000):

> There were a few amusing contributions at the *Karolinska Institutet* faculty. A large gold medal was established for the Institute's 250-year anniversary* and there were arguments as to who should be awarded this piece of gold. The King, Gustaf VI Adolf, was suggested as a candidate. What was the point of that, I wondered. He was a man of great honour, of course – but what contribution of value had he made to *Karolinska Institutet* or to medicine for that matter? None. The distinction would be purely a conventional acknowledgement of someone who was already quite distinguished enough as it was. I proposed Rolf Sievert, Professor of Radiophysics at the Institute. Sievert was an internationally-renown and influential powerhouse when it came to assessing ionising radiation, the effects thereof and safety limits to which people can be exposed. These matters had become very relevant over the period's fierce discussions about nuclear weapons and nuclear power, and Sievert was continually referred to in the debates. The King was naturally the one who received the medal.
>
> At the end of the meeting, I received a small note from Professor Gunnar Björck†. On the note he had written: 'I hope you do not do as Israel Holmgren‡ did when something went against him in the faculty – enter a polemic in the newspaper!' I did not utter a single sound about this affair in the press, of course.

On 18 March 1960, the Swedish Government asked the National Swedish Board of Public Building to plan and calculate the cost of an extension of the Institute of Radiophysics (the building that would accommodate the radiation protection activity and the National Radiation Protection Institute).

On 16 May, the National Swedish Board of Public Building asked SAR architect *Ingrid Uddenberg* to draw up the necessary planning documents. The cost of the new building was calculated as 4 million Swedish kronor. On 6 September, *Karolinska Sjukhuset's* management said they had no objections to the planned extension.

On 20 February 1961, the National Swedish Board of Public Building handed over a site plan as well as principal drawings to the Ministry of the Interior (at that time you wrote to 'the King'). The incorrect wording mentioned that 'The location of the extension has led to certain problems with regard to the wastewater arrangements'. It had been found the 'most suitable way of designing the wastewater system was such that runoff water was drained off to Uppsalavägen while other wastewater would be connected to the hospital's existing network'.

In a supplementary letter of 27 May, the National Swedish Board of Public Building mentioned that the extension would be designed so that it could have another 2 floors built onto it. They said: 'Since the available ground within *Karolinska Sjukhuset's* area is extremely limited, this appears to be action that is well justified'.

On 16 June 1961, the Swedish Government asked the National Swedish Board of Public Building to erect the extension and set aside 1.6 million Swedish kronor for the 1961/62 budget year. On 7 August, the National Swedish Board of Public Building asked Ingrid Uddenberg to be the architect of the project.

On 28 August, the National Swedish Board of Public Building wrote to the King asking the Swedish Government to propose that the Swedish Parliament set aside a further 2 million kronor for the 1962/63 budget year for the Board to use for the building. At the same time there was mention of the need for a

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* This should be the 150th anniversary; *Karolinska Institutet* was founded in 1810 by *Jöns Jacob Berzelius* (1779–1848), among others.

† *Gunnar Björck* (1916–1996) was a very conservative Professor of Medicine, going on to be 1st personal physician.

‡ *Israel Holmgren* (1871–1961) was a strongly anti-Nazi Professor of Medicine and a teacher of popular further education.

lecture theatre and an extension of the library which had not been allocated a special area in the planned new building. The extra cost was estimated at 300 000 kronor and floor plans were drawn up. However, no funds were requested for the 1962/63 budget year because it was not thought that this work would be able to start until the new building had been finished at the start of 1964. The Department later said no to the proposed lecture theatre.

The building started in autumn 1962. The construction company was Bygg-Oleba AB, one of Sweden's leading construction contractors, founded by builder *Olle Engkvist* (1889–1969). On 10 August, the National Swedish Board of Public Building, represented by 1st building inspector *Folke Dreber*, arranged the first of a series of building conferences which were held at the Institute of Radiophysics at regular intervals until 6 February 1964. Dreber was usually the Chair of the conferences. Those from the Institute of Radiophysics who attended (apart from Sievert and me) included Arne Hedgran, Lars-Eric Larsson, Lars Lorentzon and Carl-Gösta Rylander. Architect Uddenberg participated most of the time, as did the National Swedish Board of Public Building's site controller, engineer *S. Bergill*. Bygg-Oleba was represented by work manager *B. Nyberg*, among others. A number of consultants and subcontractors also took part depending on how the work was going.

The special whole-body measurements laboratory that was to be built in the basement led to some unusual requirements. The intention was to erect a whole-body counter with a linked pulse height analyser in a room with 60–80 centimetre walls made of low-activity iron-ore concrete. The main function of the walls was to reduce disturbances from radioactive substances that emit gamma radiation outside the laboratory, so powerfully absorbing iron-ore concrete was therefore a suitable material for the walls. However, at the same time it was important for the wall material not to emit too much gamma radiation.

At the time I was working in close cooperation with Assistant Professor *Peter Reizenstein* (1928–1993) at *Karolinska Sjukhuset's* medical clinic. Peter, who was a friendly but very exuberant man, had opened his eyes to our simplified whole-body counter in 'The Pit' beneath the high voltage hall, and I was hard pushed to prevent him from taking full possession of it for clinical measurements of patients. He now wanted to build his own facility at the hospital and was in need of a low-activity screen material. Geologists had proposed that he use a peridotite (a magmatic rock consisting mainly of olivine) from Hofors. As you might expect, Peter christened it 'Hoforsite'. It was present in sufficiently large quantities to be used as ballast in concrete.

Reizenstein and I also measured the gamma radiation from different types of cement. The lowest levels of thorium and radium and potassium were in cement from Limhamn. The best types of cement did not differ substantially with regard to the levels of thorium and radium, but the Limhamn cement had only from one twentieth to one tenth of the potassium content of the others, and potassium-40 leads to disturbing gamma radiation.

The decision was that the whole-body laboratory would be built with concrete walls consisting of Limhamn cement and Hoforsite. At the building conference on 20 November 1962, I pointed out how important it was that no other material could be present. According to the minutes, I said:

> The low-activity laboratory […] is to be erected exclusively in concrete made of Limhamn cement and 'Hoforsite'. This means that neither floor nor ceiling may be treated using any method that adds any other material; the interior of the room must be completely pure.

When the work was about to begin, I asked for samples of the ballast and found, by taking measurements, that it was significantly more active than the Hoforsite. I showed the result to the work manager, who agreed that the material looked more like ordinary sand and crushed stone than Hoforsite. I travelled with the work manager to the crusher where the Hoforsite stocks were supposed to be. The explanation turned out to be quite simple – they had not cleaned the crusher after it had previously been used.

And so the casting began with careful checks that only pure Hoforsite was used as ballast. However, my suspicions were aroused and I now took samples of the concrete. They were shown to contain significantly more potassium-40 than could have been expected in a mixture of Hoforsite and Limhamn cement. The explanation had to be that another type of cement had been used.

This time, the work manager was doubtful. When they had cheated with the Hoforsite, he had been able to see that it was the wrong material, but the fact that it would be possible to use radiation measurements to show that it was the wrong cement appeared to be pure magic. The workers had such clear and strict instructions that a mistake would be impossible, or so he thought. Having reluctantly examined the case he backed down. Those fetching the cement had not taken enough care to make sure they took the cement from Limhamn but had continued to take the ordinary cement which happened to be from Vika. What difference could there be and who would ever notice!

On 21 December, Bygg-Oleba wrote to the National Swedish Board of Public Building and notified them of the mistake. ‘We investigated the cause immediately and this has been found to be a pure misunderstanding between the control office and Sellberg’s mixing attendant who delivers the concrete’.

Bygg-Oleba offered to knock down the newly-cast walls, but I consulted Sievert, saying that the increase in radiation was really insignificant and could be accepted. However, the ceiling (as before with the floor) really did have to be cast from cement from Limhamn. And that served as a lesson that people may still do things wrongly in spite of all the instructions given if they think that they can get away with no-one being able to discover the error.

At the building conference on 2 July 1963, the loft had been cast and the bottom floor partition walls bricked up. At that time the labour force was at its largest with 33 men. The final conference was held on 6 February 1964. The work was then almost finished and inspector Dreber thanked everyone for their good cooperation. The building was ready to be moved into that year. Sievert was able to use his new boss’ office for a year. He then retired.

In spring 1962, Sievert employed a vivacious chemist by the name of Sven Löfveberg, who had grown tired of the university’s chemical institute and had developed a strong interest in teaching. The assignment Sievert gave him and for which he was initially employed for one year was to examine the need for education and training in radiation and radiation risks at the different training stages. Not specialist training, just basic training that could increase the understanding of the problems concerning radiation protection. Löfveberg’s report was quite negative. When it came to different courses from education at lower secondary school level to specialist training of nurses and laboratory assistants, the people responsible were completely disinterested in communicating general knowledge about radiation and nuclear physics. If it was a question of a specialist matter so that the knowledge fulfilled a specific purpose, then yes certainly - but not general education and training improvements.

Löfveberg’s reporting came at a time when the Radiation Protection Institute’s building had been planned, and Sievert asked him to stay and take part in the planning of the training premises that was to be set up on the top floor. The intention here was to be able to arrange courses for different categories of personnel who needed to learn radiation protection and how to handle sources of radiation. A large laboratory was planned and needed to be equipped. Sven Löfveberg and I cooperated with regard to planning laboratory work and obtaining necessary instruments. He then also started to take an interest in the communication of information and eventually became the Radiation Protection Institute’s information manager.

Sven was very interested in matters concerning training and information and soon built himself up a valuable network of contacts. He had a capacity to arouse enthusiasm and be enthusiastic about the information himself, but he was hot-tempered and had a short fuse if he came across someone who he thought was acting stupidly. In that respect he was very similar to Bill Pochin, who was also a charming man but with a bad temper.

Pochin’s dangerous temper was well known, as were the warning signs. His face would change from red to purple and then white prior to the outburst. When he acted as Chair he was forced to control his temper. In order to succeed in doing so he had a number of pencils in his pocket which he picked up, one after the other, and broke to vent his feelings. On one occasion, Dan Beninson and I bought him a number of giant pencils as a present, which he appreciated. He was not totally lacking in humour.

Pochin was an extremely polite man. The reason for his fits of rage was complicated. He could become angry when someone said or did something silly. At the same time, he became even angrier at himself for being so impolite that he grew angry at someone else. This started a chain reaction where the fury escalated until it was unbearable. He was not just irritated by people. He could not take his eyes off a picture which was crooked, and in the end his wrath led him to rush over to the picture to straighten it. In discussions with Sievert he often lost patience, possibly because Sievert, who liked experimenting, provoked him. It was not unknown for him to then go to a wall and kick it. Luckily, I had good relations with both Pochin and Sven and neither of them had ever lost their temper with me.

Sven Löfveberg proved to be indispensable when it came to practical arrangements. He never thought, in the way that many people do, that things would take care of themselves. He predicted what could go wrong and took measures to prevent it. When ICRP had its first big meeting in Stockholm in 1965, Sven was of crucial help to David Sowby and me and was pleased with the many new contacts he made from all the corners of the globe. But our most fruitful cooperation came in the 1970s.

In June 1961, the Nordic Society of Medical Radiology held its 24th Congress, this time in Lund and Malmö with the person who was possibly Sweden's best-known x-ray diagnostician, Olle Olsson, as President. Sture Lindberg held a lecture on the subject of 'Should haemangiomas in infants be treated?', and his answer to the question was mainly no. *Ulla-Britta Nordberg* and colleagues gave an account of a follow-up of 1100 haemangiomas that had been treated with radiation. There was now a consensus that radiation treatment should be used only in exceptionally difficult cases. Kurt Lidén described the Lund-based 'whole-body counter' with sodium iodide crystal, the first in Scandinavia. Together with *Ebbe Cederquist* (born in 1915) he also described its clinical use (something which formed the basis for Cederquist's doctorate in 1964). I recounted experiences of our simplified whole-body counter. Calle Carlsson recounted the calculation of integral doses of x-ray irradiation, a pioneering achievement.

A meeting of 'RIFO' (MPs and Scientists) in early 1960 debated the risks from the radioactive fallout. A number of experts, including from the FOA, pointed out that the risks were very small and did not need to worry anyone. Doctor *Elisabet Sjövall* (born in 1915) acted as Chair and appeared to be one of those who *was* worried. One of the experts pointed out that she had a watch which probably gave her a higher radiation dose than the radioactive environmental contamination. Logic should dictate that Sjövall firstly remove her wristwatch. Sjövall's response to this suggestion was: 'It is the right of each individual to be illogical!' I think this is quite a good example of the grounds for communication difficulties between experts and laypeople.

The disturbing radioactive fallout over the Nordic countries meant that it became increasingly important to examine the contamination of foods. The group that would plan the coordination of the food measurements and which consisted of me as the convenor, as well as Kay Edvarson, Lars Fredriksson and Bertil Åberg, had put forward a proposal to the Radiation Protection Committee in April 1962 to take measurements at a number of selected farms. In 1963, the report was followed by a report on the measurement results from 108 tested farms. In 1963–1964, regular samples were also taken from 32 Swedish dairies. The measurement results tallied with what was to be expected going by the experiences from the 108 farms. The maximum levels of caesium-137 which were found in milk from the mountainous areas (Tärnaby, Gäddede, Dikanäs and Sveg) and the lowest levels in milk from the agricultural areas which received little precipitation (Skåne, Mälardalen and Gotland). The maximum concentrations of iodine-131 were found in milk from individual farms in Norrbotten's mountainous areas in 1962 and amounted to 2 000 picocuries per litre (approx. 75 becquerels per litre). The maximum level of caesium-137 in dairy milk was measured in milk from Tärnaby in 1963 and was 1 500 pCi/l (approx. 50 Bq/l). The maximum level of caesium-137 measured in reindeer meat was in meat from Flatruet and amounted to 110 000 pCi/kg (approx. 4 000 Bq/kg) in 1963. Many other foods were examined. It proved to be particularly difficult to analyse flour – the milling industry refused to disclose the original source.

None of these values exceeded the action levels which had been recommended to the County Administrative Boards by the Radiation Protection Committee in February 1962, but the high levels in reindeer meat could mean problems for the reindeer herding Saami. This led to contact with the Saami and information in their newspaper, *Samefolket*.

However, not everyone was convinced of the lack of danger. A group which called itself the 'Everyman III Committee'* wrote to the Institute of Radiophysics on 3 October 1962:

> The health of our population and all populations is in danger due to the continued nuclear weapons testing. While the fallout of strontium 90 and caesium 137 from the years of American and Soviet Russian nuclear weapons testing has not yet become fully apparent, the world press is worried by the rising quantity of iodine-131 in milk. The Swedish authorities are evidently trying to trivialise the hazards that are threatening here. In Sweden, no upper tolerance limit has been established while the American healthcare authorities have settled on 100 micromicrocuries per litre of milk and the English on 130 micromicrocuries.

The group was clearly not aware that in February 1962, the Radiation Protection Committee had specified action levels for when to discard foods and that the action levels stated for iodine-131 in milk were 70 nCi/l, i.e., 70 000 micromicrocuries or 70 000 picocuries per litre. They were also clearly not aware that this level was equal to the one recommended by the British Medical Research Council and thus applied by the British healthcare authorities. The recommendations certainly applied to the situation following a major reactor accident but could provisionally also be applied to a short-term fallout situation such as after the big nuclear explosions at Novaya Zemlya. The group also wrote:

> In a report of 26 September, the Institute of Radiophysics in Stockholm stated that the average iodine-131 activity in milk on Monday 24 September in Lycksele in Lapland was measured as 1 000 micromicrocuries per litre, in Jämtland as 325, in Mälardalen as 272, in the west-coast area as 219 and in Skåne as 162. The Swedish authorities think that these figures give no cause for concern, and in an interview by *Svenska Dagbladet* on 27 Sept., Ph. Lic. Bo Lindell at *Karolinska Institutet's* Institute of Radiophysics explained that the Nordic experts at the radiation conference in Oslo had recently agreed that there would be no need for action even if the measured values were 10–20 times higher. Were we to prolong the consequence of Lic. Lindell's statement, an activity of 20 000 mmc in one litre of milk (20 times that recently measured in Lycksele) would not give rise to action in Sweden despite the fact that two litres of such milk would contain more iodine-131 than would be tolerated for one whole year in the USA.

The reference to the USA must have been down to a misunderstanding. 20 000 micromicrocuries per litre of milk, i.e., 20 nanocuries per litre, constitute less than 30 % of the action levels stated by authorities in both the United Kingdom and Sweden. There is reason to assume that such milk contamination is inappropriate and ought to lead to protests, but none of the authorities thought that it justified an intervention against the milk.

On 3 July 1962, the Radiation Protection Committee issued a statement to the Water Court of Appeal regarding the permitted discharge of radioactive substances into Magelungen Lake from the combined heating and power works at Ågesta. A number of changes to the originally-proposed discharge limit were proposed. This meant that the maximum annual dose of radiation to an individual person would not exceed 50 millirems (0.5 millisievert) and that the average annual dose for a larger group of people, no more than 10 000 people, would be less than 10 millirems (0.1 millisievert). The Radiation Protection Committee made the following comment about its proposal:

> The result of the Committee's proposal for established discharge limits for radionuclides to the (aquatic or atmospheric) environment discharges must be seen against the background that the appellants' information has shown that the discharge

* The *Everyman III* was an international protest boat against nuclear weapons. The Committee prepared to receive it in Sweden. It consisted of *Bertil Svahnström* (1907–1972), Chair of the Campaign against nuclear weapons, *Olle Wedholm*, Chair of The Citizens of the World, author *Per Anders Fogelström* (1917–1998), Chair of the AMSA, *Ulrik Herz*, Chair of The Swedish Peace Council, *Stig Jacobsson*, Chair of the Swedish World Peace Mission, *Paul Rimmerfors*, the Youth Peace Federation, *Östen Johannesson*, Chair of the Swedish Peace and Arbitration Society, MP *August Spångberg* and author *Tore Zetterholm* (1915–2001).

> can be greatly limited and that the Committee attaches great importance to the International Radiation Protection Commission's (ICRP's) general statements that all doses be kept as low as practicable, and that any unnecessary exposure be avoided.

There was an Appendix to the Radiation Protection Committee's statement in the form a report on 'Radiation biology assessments in the Magelung case', written by Arne Forssberg, K-G. Lüning and Arne Nelson. The three biologists were very critical of the statement made by Professor Linus Pauling, the well-known Nobel Prize winner. Pauling had made special reference to a British report in which the authors had studied the frequency of leukaemia and the background radiation in some areas in Scotland. The British group had summarised its results in the following table:

Area examined	Bone marrow dose (millirad)	No. of deaths of leukaemia per million and year
Aberdeen	101	46
Aberdeenshire	94	33
Dundee	86	29
Edinburgh	80	33

The three Swedish biologists write about this:

> The English authors discuss the lack of quantitative coincidence between dose and frequency of leukaemia, primarily in Aberdeen and Dundee which, with regard to living conditions, offer the greatest similarities; the frequency of leukaemia in Aberdeen is thus 50–60 % higher than in Dundee while the bone marrow dose is only approx. 17 % higher. This discussion follows next to the table and ought to have been observed by Prof. Pauling, like the following summary by the English authors: 'In our opinion, the higher mortality in Aberdeen […] therefore cannot be attributed to the higher level of radiation.

Professor Pauling reports the same information as follows:

> Measurements of the bone marrow dose in inhabitants in Dundee, Edinburgh, Aberdeenshire and Aberdeen from natural background radiation amount to 80, 86, 94 and 101 mr* per year. The leukaemia mortality rate for these cities over the past 10 years was found to be 29, 33, 33 and 46 per one million per year. These results confirm the opinion that leukaemia is caused by high-energy radiation, even in small doses.

Pauling had transposed the leukaemia frequencies for Dundee and Edinburgh, thereby giving the impression of a monotonic increase in risk with an increased radiation dose. His conclusion had no support in the British authors' own conclusions. It was this manipulation, whether intentional or a consequence of carelessness, which infuriated the otherwise friendly Arne Forssberg.

On 5–7 September 1962, another symposium was held with the title *Radioactivity in Man*, this time at Northwestern University in Chicago. Several Swedes took part this time: *Ingvar Östen Andersson* from Studsvik, *N. G. Holmberg* and Peter Reizenstein from *Karolinska Sjukhuset*, Kurt Lidén and *Yngve Naversten* from Lund, and as Wilhelm von Döbeln from the Royal Central Institute for Gymnastics in Stockholm. The symposium now had participants from 16 countries. Its participants included Boris Rajewsky from the Max-Planck Institute for Biophysics in Frankfurt, *John Rundo* from the British Atomic Energy Authority, and Jorma K. Miettinen from Helsinki. A total of 46 lectures were held.

* Milliröntgen.

The Society of Radiophysicists' (the Radiophysicists' union) annual meeting on 1 December 1962 discussed the public standing of the medical physicists. Were they doctors' assistants or on an equal footing with doctors? Kurt Lidén made a contribution to the discussion:

> The development within medical radiology over the past 10–15 years has completely changed its face, partly through the introduction of high-energy radiation and the decline of conventional x-ray irradiation, and partly through the entrance of radioactive isotopes into the arena, not only within radiotherapy but also as a diagnostic tool within several different clinical disciplines.
>
> However, at the same time, this generational change means that radiotherapy's previously simple dosimetric observations, with the visual skin reaction observation as an important element seen by the doctor every day, must be replaced with mathematical calculations of doses of radiation through accurate dose plans supported by dosimetric studies on anatomically correct models and direct measurements on the patient.
>
> Radioactive isotopes for therapy and diagnostics sometimes give the healthcare system extraordinarily difficult measurement problems.
>
> […]
>
> The public standing of medical physicists in Sweden is still not completely regulated in a uniform way throughout the country. The Medical Board's circular of 11 December 1958 gives instructions in this regard. However, it is quite clear that the type of medical physicist we are thinking of does do work within the healthcare system which is completely parallel to and comparable with the doctors, although it does take place on different fronts. There therefore appears to be every reason to place the activity on an equal footing in terms of organisation and public standing as far as possible, but obviously you then also have to take into account the fact that training and practical experience must also comparable.
>
> […]
>
> We cannot enforce full equality with the doctors' standing if we cannot indicate solid training – this is something that we must remember. […] There should obviously firstly be detailed discussions as to how far we want to go in terms of uniformity. Inroads into an old, well-established area should be made with tact and skill, and the relatively free standing that we currently have in many respects thereby ought not to be sacrificed at the moment.

In 1963, the Royal Academy of Sciences appointed a new national committee at Sievert's initiative. The national committees within different areas are contact bodies for Swedish participation in international scientific cooperation. It was within the National Committee for Physics that Sievert, in the 1940s, had acted and taken the initiative to coordinate the Swedish military physics research which was later taken over by the FOA. The new National Committee was named the National Committee for Radiation Protection Research and Sievert was obviously its Chair. Members were:

Lars Carlbom Larsson	Kurt Lidén
Torbjörn Caspersson	Bo Lindell
Kay Edvarson	K-G. Lüning
Arne Forssberg	Arne Nelson
Lars Fredriksson	Jan Rydberg
Lennart Hannerz	Rolf Sievert
Arne Hedgran	Torbjörn Westermarck
Lars-Gunnar	Bertil Åberg

In 1963, the Swedish Employers' Association (SAF) issued a 200-page work entitled *RADIOACTIVITY – use – risks – protection*. The initiative had been taken by the SAF's specialist Gideon Gerhardsson in cooperation with Arne Nelson. The work contained 13 interesting chapters on radiation, the effects of radiation and radiation protection. The 12 authors (Nelson wrote two chapters) were: Lars Carlbom, *Lars-Gustaf Erwall* (head of the Isotoptic Laboratory), *Sven Forssman* (Professor of Occupational Hygiene and later Deputy Director-General of the National Institute of Occupational Health), Gideon Gerhardsson, Arne Hedgran, Bo Lindell, Lars Lorentzon, K-G. Lüning, Arne Nelson, Bertil Swedin, Åke Swensson and Rune Walstam.

The Nordic Society of Medical Radiology's 25th Congress in Odense in 1963 held 82 lectures. Rune Walstam described an aftercharging procedure, i.e., attempts to use a remote-controlled application of sources of gamma radiation for gynaecological radiation treatment. The Society's 26th Congress was held in Helsinki in 1964 with Carl-Erik Unnérus as President. I have already mentioned it in Chapter 14 and the attention that it aroused when it became evident that the frequency of lung cancer was five times higher in Finland than in any other Nordic country.

On 30 November 1963, the Swedish Society for Radiophysics held its annual meeting at *Radiumhemmet*. Following a proposal by an election committee which consisted of Jan Cederlund and me, Berndt Waldeskog was elected as Chair and Ulla-Britta Nordberg as Secretary.

Right at the start of 1963 I received a letter from a Swedish-speaking reindeer herder in Canada, Sven Johansson, who was asking about our experiences. I sent him a number of reports and on 11 March he wrote to thank me, saying among other things:

> […] The entire picture of the radioac situation in Canada is dominated by hearsay. Not only that, scientists (outside the field of radiophysics), officials, etc. who should know better are spreading the rumour here, possibly because there are few or no measurements, which is good for the reputation of the soil.
>
> […]
>
> The Canadian Reindeer Industry is not huge, with only 7 000 reindeer, and its 33 year-old history is one big failure. It has been managed by the Administration, but this year the Government has outsourced the management on a contract basis and I have been given the responsibility for the direct management.
>
> […]
>
> I also believe there is high consumption of reindeer among our Eskimo herds. I venture to say approx. 800–1 000 g per day or 350–400 kg per year. There is a big cull here in November, but individual reindeer are slaughtered all year round. The meat that is slaughtered in November is stored in ice cellars and used during the summer.
>
> […]
>
> I have been told that there are no laboratories in Canada which can start research into and measurements of radiation fallout. I also have a strong feeling that Scandinavia is ahead of other countries as regards the problem of reindeer radiation. I would therefore ask you if it were possible to arrange close cooperation between the Canadian Reindeer Industry and the Institutes in Lund and Stockholm.
>
> […]

I answered this on 29 March with: Thank you for your letter of 11 March and the information on reindeer herding in Canada, which was very interesting to read. Because we are very interested in the whole problem of radioactive contamination on northern latitudes, we would be happy to contribute measurements if these were required. However, I would advise you to first contact the head of the Canadian Radiation Protection Authority, Dr. Peter M. Bird in Ottawa, whom I personally know very well. I know that the Radiation Protection Division at the Department of National Health and Welfare takes a very large number of radioactivity measurements throughout Canada, and it is possible that it would feel left out in the cold if you had not contacted it first. To make the whole thing easier, I have taken the liberty of writing to Dr. Bird myself as per the appended copy of the letter. If it still proves that we would be of help in taking measurements here in Sweden, I suggest you make a special arrangement with Dr. Kurt Lidén in Lund with regard to measurements of lichens so that these measurements can as far as possible be included in the research programme which he already has ongoing at this point. For Dr. Lidén's information, I will send him a copy of this letter and my letter to Dr. Bird. We are happy to measure reindeer meat free of charge provided it is not a question of a routine monitoring programme but can come under the heading of research. […]

18. REORGANISATION WITHIN ICRP

IN 1959, LEO SZILARD, the man who played such a major role in the birth of the atomic bombs and who, with equal vigour, worked so that they would never be used, was writing a book that was meant to be called *The Voice of the Dolphins* and to be about the possibility of preventing a nuclear war between the USA and the Soviet Union. But this was just one of the incredible Szilard's many projects. In 1959 while on a European trip to prepare for a Pugwash meeting about the risks of war and peace, he turned up unexpectedly and unannounced to see tumour biologist *Georg Klein* (1925–2016) at *Karolinska Institutet*. The visit is given a spirited description in Klein's book 'The Atheist and the Holy City'.

As always, Szilard enquired about what was new at the institute he was visiting and gave unstinted suggestions, 'half of them ingenious, half of them pure madness'. When using Klein's toilet it turned out that he had had blood in his urine and he recognised that this had been happening for six months. Klein persuaded his reluctant guest to visit a Swedish medical colleague for an examination, and it was ascertained that Szilard had a large tumour in his urinary bladder. He was advised to go home to the USA for an immediate operation.

However, Szilard mistrusted the American surgeons and did not want an operation. Instead, he and his wife, who was Professor of Hygiene, went to a medical library and read up on the most essential knowledge on the treatment of bladder cancer. He concluded that even a successful operation would make him an invalid and that radiation treatment would be the best. He therefore admitted himself to the well-known Memorial Hospital in New York.

The conventional radiation treatment was nothing in the eyes of the unconventional Szilard. Instead, he wanted the tumour to be given much higher doses of radiation than usual, which led to heated discussions with unwilling doctors. Szilard wanted his urinary bladder to receive a radiation dose of 8 000 rad (80 gray), but the final compromise was 6 000 rad. His bladder was badly burned but the tumour disappeared. Szilard felt that the handicap of having a plastic bag on his abdomen meant was a cheap price to pay. He could complete *The Voice of the Dolphins* and had time to make important contributions to the peace movement before dying in 1964 – of a brain haemorrhage in his sleep.

Once ICRP's new recommendations had been published by Pergamon Press in 1959, it was not long before the recommendations from Karl Morgan's Committee II also became available as ICRP Publication 2. It was then possible to conclude that the document containing the Main Commission's recommendations was ICRP Publication 1, although this had never been said.

Karl Morgan's Committee II was efficient largely thanks to Morgan's group in Oak Ridge under the management of Walter Snyder, but it had had a labour-intensive task in compiling data on doses of radiation from a large number of radioactive nuclides. Morgan's stubbornness in pursuing a policy which did not tally with that of the Commission did cause a few problems, however.

Morgan's report contained comprehensive tables of data for calculating doses of radiation from nuclides which had entered the body with water or inhaled air. The most important quantity was the maximum permissible body burden which was stated in microcuries.

The value was calculated so that such a body burden would give a maximum permissible weekly dose to the *critical organ*. 'Critical organ' means 'the body organ whose damage from radiation results in the greatest damage to the whole body'. In the majority of cases, it was assumed that the critical organ was the organ that received the greatest concentration of the radioactive substance. With regard to slowly-excreted, long-lived radioactive substances, there was the added complication that, if taken up repeatedly, they could build up an even greater body burden. The value of the body burden was then calculated to apply to a life-long exposure. The maximum permissible weekly dose was assumed to be

0.3 rem for all organs except for the genitals, the skin and the thyroid gland. The weekly dose of 0.1 rem applied to the genitals and 0.6 rem to the skin and the thyroid gland.

In addition to the values for the maximum permissible body burden, the report from Committee II also recommended the values for maximum permissible concentrations (or ‘MPC’ values) of the different radioactive nuclides in water and air for exposure over 40-hour weeks and for continuous exposure (168-hour weeks).

ICRP’s publication from 1955, the one that had been printed as a supplement to the *British Journal of Radiology*, included both the Commission’s own report and the Committees’ recommendations. In the 1958 report, the one that the editorial group wrote in Woods Hole and which was published in 1959 (Publication 1), it had not been possible to include any Committee report; the Committees, with the exception of Morgan’s Committee II, were not efficient enough.

This was the situation when ICRP met in Munich in summer 1959, a meeting in which all Committees also took part. The meeting took place immediately before the 9th International Congress of Radiology and our German hosts had arranged premises in the Bayer Group’s building. It was the first major ICRP meeting for which I had responsibility in the form of being Secretary, but luckily I was well assisted by Lars-Eric Larsson, who was a member of Committee III. Lars-Eric, big and dominant, ensured that everyone came to the right room and arranged numerous practical details, which enabled me to prepare the negotiations. I often experienced a feeling of giddiness and imminent disaster when Lars-Eric dealt with a number of important people as if they were schoolchildren who needed looking after. I shuddered when Lars-Eric’s loud voice reprimanded participants like Rolf Sievert and the explosive Bill Pochin. But Lars-Eric had an incredible ability to approach the edge of the abyss but always stop in time. Nobody took offence – quite the opposite; his inputs were appreciated and he became very popular over time.

On the first day of the Main Commission’s meeting, Lars-Eric came to me accompanied by a visitor. It turned out to be the Egyptian doctor Hussein Daw who worked at IAEA, responsible for the radiation protection area. Dr. Daw wanted to take part in the Commission’s meeting as an observer for IAEA. This was something completely new; until now, ICRP had not had any outsiders attend the meetings, and Sievert and Failla, Chair and Deputy Chair respectively, were not prepared to deviate from this tradition. I do not remember how they settled the sensitive problem of not alienating IAEA, but the meeting in Munich marked the introduction to both official relations with IAEA (and with WHO) and many years of cooperation with Hussein Daw.

We were all dismayed when, during the Commission’s meeting, Gino Failla explained that he wanted to leave ICRP and live a quieter life on moving from New York to Chicago where his young wife Patricia had obtained better research options. Many attempts were made to convince him to stay in the Commission even if he stepped down as Deputy Chair and Chair of Committee I, but Failla was adamant. It was then a matter of finding someone to succeed him as Deputy Chair. The Commission quickly agreed on Bill Pochin, which was a startling choice because Pochin was not yet a member of the Main Commission. However, it did show the extent to which Pochin’s competence and efficiency were appreciated.

At the Munich meeting, the Commission agreed on a number of clarifications and explanations of the recommendations in Publication 1. A few statements were also made, including with regard to the need for an extended holiday for people in radiological work: ‘The Commission considers that with the present maximum permissible exposure levels no special treatment of radiation workers with respect to working hours and length of vacation is needed’. These decisions were published in a number of radiological journals but were also included as an Appendix to Publication 2.

The ICRP dinner at the Congress of Radiology in Munich in 1959. From the left, the men at the table are Boris Rajewsky, Rolf Sievert and 'Bill' Pochin. Photo: Unknown.

Another Committee report was approved during the meeting, the one from Robert Jaeger's Committee III on protection against x rays. It was published in 1960 as ICRP Publication 3. In this connection, ICRP reports that were mostly in demand had become available, but the reports from Committees IV and V were still lacking.

At the time of the Congress of Radiology, Sievert had arranged a symposium on the risks of radiation. Here, Professor Bugnard introduced one of his mentees, the young doctor Pierre Pellerin.

In April 1959, UNSCEAR had invited ICRP and ICRU to a new joint study as a continuation of the one that had been published in 1957. It was now not a case of methods to estimate the genetically significant dose, but 'a careful study of the radiation doses received by patients, including not only doses to the gonads but exposure of significance for the possible induction of malignant disease', as well as 'the results of such a study should be correlated with the results of epidemiological investigations'.

So, once again, ICRP and ICRU set up a Joint Study Group, now consisting of the Chairs, Deputy Chairs and Secretaries of the Commissions as well as an additional ten people, among them David Sowby and Eric Smith. This time, the elected Secretary of the group was Lars-Eric Larsson, who did an excellent job.

The group found that it was significantly more difficult to state which information it was necessary to give when the aim was to correlate with epidemiological examinations. Where the risk concerned leukaemia, it was certainly possible to surmise that the dose to the active bone marrow was relevant, but it was not known whether it was the maximum dose or the average dose. For the latter case it was necessary to know the distribution of the active bone marrow in the skeleton, but also the distribution of the dose. However, the group stated a number of pieces of physical information which were needed concerning the source of the radiation as well as biological information concerning the patients. A quantity of interesting statistical information was also collected concerning the scope of different types of x-ray examinations and other medical exposures to radiation. The report was published in 1961 in *Physics in Medicine and Biology*.

Before 1962, ICRP's work method had meant that each Committee was expected to write a report with recommendations for radiation protection in the respects that fell within the Committee's area of responsibility. However, some of the Committees were very inefficient. The members were very competent but, in their ambition to find competent Committee members, the Commission had created Committees whose members, due to their competence, had such responsible tasks in their daily work that they did not have much time spare to draw up considered Committee reports. Not only that, Committees which were permanent in practical terms were under no pressure to finish writing the reports.

This had practical consequences which put me as Secretary in an embarrassing situation. It concerned primarily Committee IV, whose first Chair had been Professor Mayneord, followed by the very competent radiophysicist Harold Johns who was succeeded some time later by the British biophysicist *Gerard Neary* (1913–1972), who inherited the manuscript that his predecessor had drawn up. The publication was to give recommendations on protection against high-energy gamma radiation and particle radiation.

I found that the manuscript would be unsuitable for publication. I may not have dared to say this to Mayneord or Johns, partly because it would have been direct criticism of them as the persons responsible, but partly also because I did not feel I was in a position to criticise such big men. With Neary it was different. It was not his fault that the manuscript was poor. Neary, who managed the biophysics section at the Medical Research Council's research station for radiobiology outside Harwell, was also an incredibly nice, modest man who fully understood the problem. When I wrote to him on 26 January 1961 regarding the poor manuscript bequeathed by the old Committee, his answer of 6 February that my letter had made him despondent but that he recognised that I was right and reasonable in rejecting the report. Sievert and I had promised that he would not have to take any responsibility for his predecessor's results, but he was willing to discuss the problem with me. He wrote that he had already had serious reservations when he first saw the manuscript, partly due to the substandard presentation. What made the matter sensitive was that this was a draft report from a Committee consisting of the world's leading experts in the field, including Gray, Johns, Koch*, Mayneord, Tubiana and Wachsmann. But none of them had had time to adequately process the material.

Neary agreed that the most reasonable solution was to engage a consultant to analyse and complete the report. It would certainly only be mitigating measures, but one could perhaps still hope for a report 'purged of errors and incongruities which could serve as a temporary replacement until something which was better planned and had been more thought through became available'.

The person who was engaged as a consultant in March 1961 to save the document was Dr. Bernard Wheatley, who was now radiation protection physicist at the British Central Electricity Generating Board's nuclear physics laboratory in Berkeley. Wheatley's work was meritorious, which meant that an acceptable report could be published as ICRP Publication 4 in 1964. The other report with which ICRP had substantial difficulties was the one that would come from Committee V regarding the handling of radioactive substances and radioactive waste. Here, the Chair, Conrad P. Straub, had inherited the responsibility from the deceased Canadian Cipriani. We had seen a draft report during the meeting in Failla's editorial group in Woods Hole in summer 1958. We had found that the report was not ready to be included in the work that would constitute ICRP Publication 1 and which ended up only containing the Commission's own recommendations. We had shaken our heads at some strange formulations such as 'liquid accident', but this was set aside for the moment.

Since then, the report from Committee V had increased in significance in that the outside world was asking for guidance in the handling of radioactive waste and IAEA was waiting for fundamental recommendations from ICRP. This worried Sievert and me. It was clear to us that Straub's Committee

* Dr W. H. Koch was responsible for high-energy research at the U.S. National Bureau of Standards.

would not be able to put forward recommendations regarding the big waste problems that would arise within the nuclear power industry. If ICRP published Straub's report as it was, following necessary editorial inputs, and called it the 'Report on the handling of radioactive waste', the Commission would appear naive. The solution was to change the title to 'the handling and disposal of radioactive material in hospitals and medical research institutions' and not claim that we had a solution to the nuclear power industry's waste problem.

However, nor had Committee V succeeded in producing a report that was suitable for publication, despite the fact that the Committee had prominent members such as John Dunster, Henri Jammet, Greg Marley, Bill Pochin, Edith Quimby, David Sowby, Ed Struxness and Forrest Western. I once again ended up in the embarrassing situation that I, as Secretary of the Commission, had to advise against publishing it. As in the case of Committee IV's report, ICRP had to engage a consultant to produce a useable manuscript. The indispensable Mister Binks was now the one who had to save the situation so that a report, ICRP Publication 5, could be published in 1965.

In 1962, it was completely clear that Committees were unable to draw up their reports of their own accord. A need for more specialist reports than simply 'Report by Committee X' also began to arise. It was time to consider releasing the Committees with their highly-qualified members from writing the reports. The task could be transferred to Task Groups with well-defined assignments and a time limit for the work to then be disbanded. The task of the Committees would then be to examine and approve (or reject) the reports and to propose new Task Groups to the Commission (the Main Commission).

In order to make the change obvious, Sievert dictated that all five Committees be dissolved and replaced with four new ones which, unlike the old ones, would be numbered using Arabic rather than Roman numerals. Committees 1, 2 and 3 would have largely the same assignments as the old Committees had had, but Professor Bugnard, who would be the Deputy Chair of the Commission for the period of 1962–1965, proposed completely new assignments for Committee 4, i.e., to give recommendations on the way in which the Commission's fundamental recommendations would be applied in practice. The new Committee 4 would therefore mainly have members who were responsible for national radiation protection provisions.

Bugnard made no secret of the fact that his proposal was tailored to his protégé Henri Jammet. The outside world had followed the situation in France with interest, where Bugnard had had two protégés at one time, Henri Jammet and Pierre Pellerin. Jammet had found his place within the Atomic Energy Commissariat while Pellerin had been made responsible for radiation protection under the Ministry for Health. Before the distribution of the domain had been fully clarified, the two were thought to be bitter competitors who later went on to become good friends and conspirators when they had found their areas of responsibility. Within ICRP, Jammet was the one who drew the longest straw in becoming not only a member of the Main Commission in 1962, but also, with Bugnard's support, Chair of the new Committee 4. Pellerin only became an ordinary member of the new Committee 3.

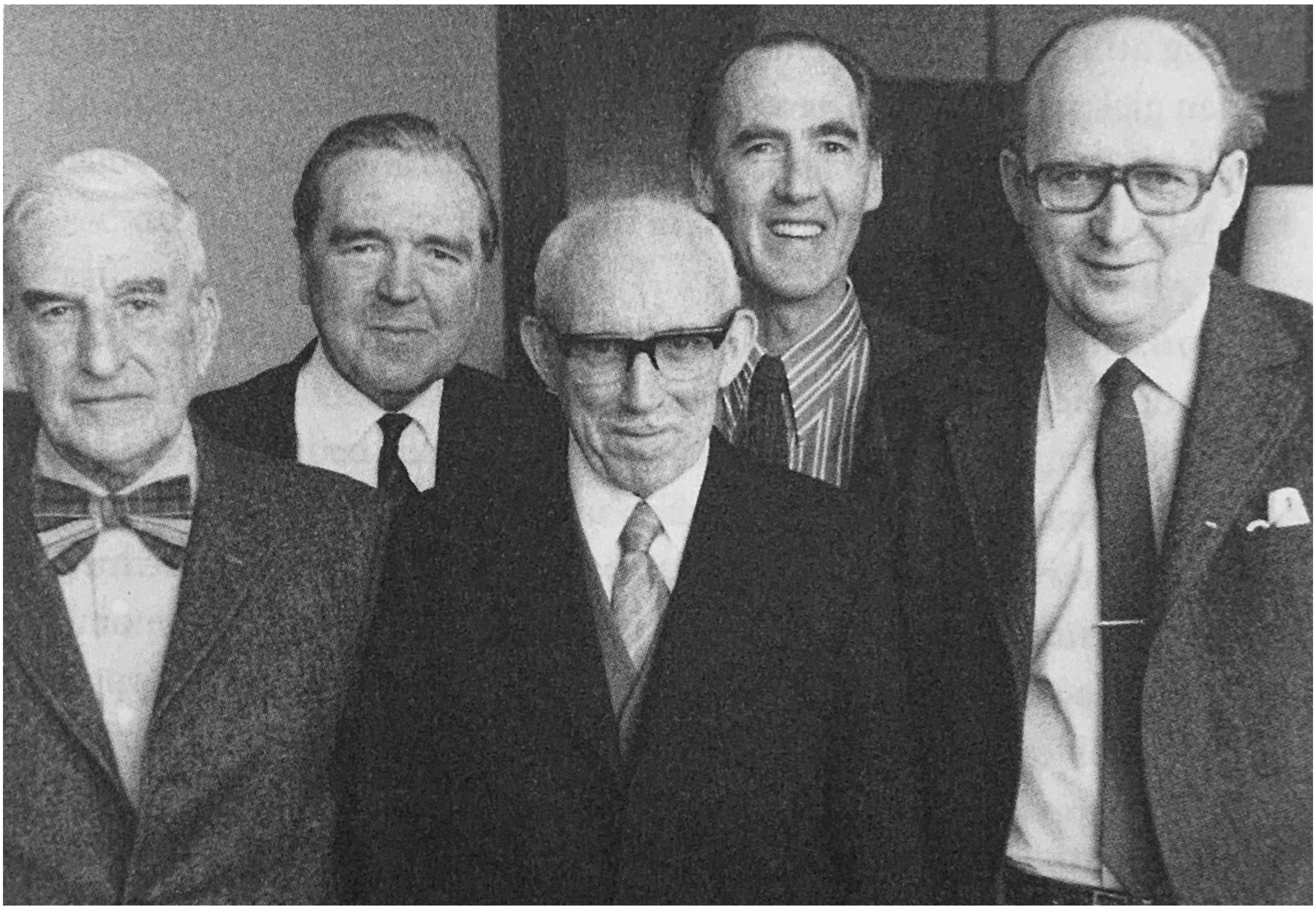
The reorganisation also meant that in 1961, Sievert had already decided to depart as Chair of ICRP before the new period which would begin at the 10th International Congress of Radiology in Montreal in late summer 1962. Discussions obviously immediately started as to who would succeed him. Sievert wrote to members asking for opinions. Geneticist Hermann Muller responded with a letter which was unexpectedly flattering to me. After having expressed regret that Sievert was unable to continue as Chair, Muller thought he did not know his colleagues at ICRP well enough to be able to assess their qualifications for the position of Chair. So, surprisingly enough, he continued to suggest me (!) as Chair in an embarrassingly complimentary manner. He continued:

> Of course, I understand that what he writes also has to largely reflect your judgement. You and he are the only members who know what is going on. Nevertheless, whatever the response may be, he may be able to continue enjoying important cooperation with you even after he becomes Chairman. And such cooperation between him and a Chairman who lives in another country would be considerably more difficult.

Appreciative words from a world-renowned Nobel Prize winner were encouraging, but Muller had of course overestimated my possibility to control the powerful people who were in ICRP. I would need to grow in stature and confidence for another sixteen years before I actually became Chair of the Commission, for which I had prepared myself by first of all spending eight years as Deputy Chair.

The person who Sievert favoured to succeed him was Dr. E. E. ('Bill') Pochin, who was surprisingly elected as Deputy Chair in Munich 1959 despite the fact that he was not yet a member of the Main Commission. With the new Chair in London, it was natural to also replace the Secretary. It was now thought that it was time for ICRP to have a paid Secretary who could devote all of his time to the Commission. Sievert was free to search for a suitable candidate, and I proposed David Sowby who, having been part of Failla's editorial group, was very familiar with the problems. I contacted David with Sievert's consent. I no longer remember exactly what happened, but David has described it (Sowby, 2001):

> in September 1961, during a session of UNSCEAR in New York, Bo Lindell invited Lars Eric Larsson and me to lunch. Afterwards, Bo told us that he intended shortly to give up the Secretaryship of ICRP. The current term of the Commission and its Chairman would expire in 1962, when a new Commission and a new Chairman would be elected. This would be a good time for the new Secretary to start. To this day I don't know how it was done, nor whether Bo and Lars Eric were working to a prearranged plan, but at some point in the afternoon I became aware that I had agreed to be Bo's successor, subject to approval by the Commission. It was virtually certain that the next Chairman would be Bill Pochin; if he were to take on the Chairmanship, he wanted the Secretary to be near at hand for easy consultation.



Five ICRP Secretaries From the left: Lauriston Taylor, Eric Smith, Walter Binks, David Sowby and Bo Lindell. Photo: Unknown.

In May 1962, ICRP met all its Committees in *Folkets Hus* in Stockholm under Sievert's Chairmanship and with Pochin as Deputy Chair and who, in reality at Sievert's request, acted as Chair of some important sessions bearing in mind that he would succeed Sievert during the summer. The election of David Sowby as Secretary Elect had now been approved by the Commission with Pochin's blessing, and

David had been asked to take part in the meeting as my assistant in order to settle into his new assignment.

When the Soviet member of Committee II, Schamov, turned up at *Folkets Hus* with an interpreter, it turned out that he had completely run out of money for food and hotel rooms. They had not prewarned ICRP of any need for assistance to cover their expenses. Lars-Eric Larsson, who helped me and David Sowby with the practical arrangements for the meeting, adopted his most protective fatherly mien and said: 'Now then, if the Soviet Embassy can't afford to assist its ICRP experts, *I'll* have to help instead!' So he brought his wallet and took out a few notes which he jovially offered to the Russians. This private capitalist intervention was more than the latter could endure – they refused the help and, whatever happened now, they would find their own solution to the financial problems.

The meeting in Stockholm was one of the most revolutionary in the history of ICRP. The old Committees I–V would be dissolved and the four new Committees 1–4 would be created from new. This also meant electing a new Committee Chair and members and electing the new Commission for 1962–1965. These decisions were actually made during the Stockholm meeting and would then only be formally confirmed at the meeting of the Main Commission which would be held in Canada in connection with the Congress of Radiology in Montreal during the late summer.

Three of the new Committees were quickly agreed, and it was also possible to agree on who would be the Chairs of the Committees, i.e.:

Committee 1 on the effects of radiation, Chair: John Loutit
Committee 2 on internal exposure, Chair: Karl Morgan
Committee 3 on external exposure, Chair: Eric Smith

With regard to the fourth Committee, the one that Bugnard proposed for recommendations regarding the application of the Commission's recommendations, they wanted to delay the decision until the meeting in Canada.

Eric Smith, who now became Chair of the new Committee 3, was not elected to the Main Commission, which would end up leading to a number of practical problems and discriminating somewhat against his Committee. The situation was not new, however - neither Neary nor Straub, who had previously been Chairs of Committees IV and V, had been members of the Commission. The election of Smith was still unexpected. It would have been natural for Robert Jaeger, who had been Chair of the old Committee III, to have continued as Chair of the new Committee as well. But, to his surprise, Jaeger was not re-elected to the Main Commission.

Jaeger's exit was an embarrassing example of the weakness of the really brutal voting procedure applied by both ICRP and ICRU at the time. According to the statutes at the time, prior to each new work period, which was three years then, at least two and no more than four members should be replaced. Three members had declined to be re-elected. If the Commission so desired, one additional person could be replaced if there was a new candidate who was preferred by the Commission. My notes three days after the meeting describe what happened:

> The election of new members to ICRP during the final afternoon session on Wednesday 16 May was very interesting. A Task Group consisting of Sievert, Pochin and Taylor had prepared a list of 8 names from those proposed and sent in by the Congress' [the International Congress of Radiology] Secretary General, Dr. Peirce* as copies of the national delegations' nominations, as well as from the additional proposals made by the members during the week. […]
>
> K. Z. Morgan complained that the list of the 8 names was too short and that the election ought to take place on the basis of all proposed names. Pochin had proposed me as a candidate and I was on the list of 8 names. This meant that I could not be

* Dr *Carleton B. Peirce* was Professor of Radiology at McGill University and head of the radiology clinic at the Royal Victoria Hospital in Montreal.

> Secretary during the voting; which became Dave Sowby's job instead. Lars-Eric and I waited in The Secretariat (room H) for the Commission in room 6 to issue a more comprehensive list for duplication. When Dave finally rushed along with it, it included 17 names. Lars-Eric came in as well to help count the votes in a wooden ballot box.
>
> There were three resignations (Holthusen, Watkinson and Jacobsen) but the statutes permit no more than four and prescribe at least two. The first vote was for the first [new] candidate who wanted to get in. Those present had [a total] of 11 votes and at least 7 were required [a majority among the 13 members of the Commission] for any candidate to get in. Those present placed dots by three names on their list and the dots were then added up.
>
> The first batch showed that I had received seven dots, followed by Jammet and Stewart with 6 dots and possibly Gopal-Ayengar* and Hug† with an equal number. I was therefore in. In the next batch, Jammet and Stewart were equal and both therefore got in. There was then a vote for a potential candidate who could possibly get in as number 4. This was Otto Hug from Germany. Candidates who also had a chance of getting in included Polvani and Gopal before they were outvoted in favour of Hug.
>
> Hug's name was then added to the list of names of the old members who had not resigned (10 names) and 10 out of 11 of these were voted for. This meant that Sievert, Taylor, Morgan and a few more had 11 dots each while Hug and Jaeger were at the bottom with 6 each. There then followed a long discussion on whether to vote *for* or *against* one of these names, and it was gradually agreed to vote *for* a name.
>
> The atmosphere was now pessimistic. The result was that Jaeger received 5 votes and Hug 6, which meant that Jaeger was out of ICRP. This was very embarrassing because there was no doubt that he had not expected this while everyone else was prepared for the result. Sowby could not bring himself to stay and follow what he called a 'death sentence'. Everyone felt rather uncomfortable. However, the next day, Jaeger was at work as usual as though nothing had happened.
>
> I would definitely not have been in first place had voting been for the person preferred. The votes were now instead split among the favourites while the third vote could be used on a name that seemed obvious.

Dagens Nyheter already carried an article about ICRP meeting on Tuesday 8 May with a big picture of Gordon Stewart, Pochin and me in conversation. It just so happened that this picture was observed by my old drawing teacher in *Nya Elementar*, Gustaf Nordlander, the man who raised my student grade in drawing from a to A in 1943 by reviewing my exam and thus enabled me to obtain a Technical Physics place at KTH. He lost no time in getting down to writing me a letter. After having expressed the hope that I really was his old pupil, Nordlander said in the letter that he was extremely interested in the problems that ICRP was dealing with. He continued:

> The following destinies which have affected me may explain this interest: during the summer I owned and lived on an islet in the Parish of Westljunga for approx. 30 years. Once Studsvik arrived there we sold islet and cottage. I sought out the finest cottage up the east coast, in a bay, quiet location, road link, private location and many other benefits. The cottage had several shortcomings but it was possible to remedy these. And along comes the Simpvarp project – one kilometre away from my cottage. I am being pursued by nuclear scientists!!
>
> All of us here on the coast now believe that our properties have fallen in value and may even be uninhabitable. Even if the risk of radioactivity is minimal, the appearance of a nuclear power plant in our neighbourhood has had a big psychological impact. The area and its products are avoided!
>
> It may change its name, but if you are in fact my old pupil, answer me with a few lines – how great is the risk?

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* *A. R. Gopal-Ayengar* was part of the Indian UNSCEAR delegation.

† *Otto Hug* (1913–1978), later Professor of Radiation Biology at the Ludwig Maximilian University of Munich.

My answer was that I would definitely remain living in my cottage if I had one and that Nordlander could absolutely forget about the risk of radiation. On the other hand, it was of course unfortunate that he should be affected by the presence of a major industry with everything it brought with it in terms of roads and traffic, change to the rhythm of life and disturbances for those who had chosen to live in peace and quiet and enjoy nature in seclusion.

The 10th International Congress of Radiology was held in Montreal from 26 August to 1 September 1962. During the Congress' closing ceremony on 1 September, the Swedish Academy of Sciences' gold medal for meritorious achievements within the radiation protection field was to be awarded. On 6 June 1962, the Academy had adopted statutes for this medal and the fund that would make its arrival possible. The fund had been started by a donation from the initiator, Academy member Professor Rolf Sievert. However, Sievert had not wanted his name linked with the medal. The intention had been for the medal to be awarded by Sievert but, as it turned out, he was prevented from taking part in the Congress.

On 25 August, the Secretary of the Academy of Sciences, Professor *Erik Rudberg* (1902–1980), wrote to Sweden's ambassador in Ottawa, *Oscar Thorsing* asking him to act as the Academy's representative at the prize-giving. The medals – there were two of them the first time – were to be taken to Canada by 'two of Prof. Sievert's younger Swedish colleagues'.

These two colleagues were Lars-Eric Larsson and myself. It was a task that came with responsibility because the medals were large and thick and made of pure gold. We wondered what would happen if a Customs officer discovered them, and we guessed that the correct explanation would be met with warranted suspicion. According to the statutes, proposals as to the recipients of the medals would come from ICRP whereupon the Academy of Sciences would make the final decision. On Sievert's advice, the decision on this first occasion was to award one medal to Walter Binks and one to Karl Morgan.

But the journey went well and we arrived in Ottawa on 18 August. ICRP's Main Commission was to meet there before the Congress under Bill Pochin's Chairmanship. Lars-Eric and I were invited by David Sowby to stay at his home while the ICRP meeting was on. He had a small villa by a tributary of the Ottawa River next to what we thought was Canadian wasteland, and he let us try and master the art of Canadian canoes on the river.

At the meeting in Ottawa, the election of Pochin as Chair following Sievert was confirmed, as well as the establishment of the new Committee 4 with Henri Jammet as Chair. Professor Bugnard was elected as Deputy Chair and Pochin was free to employ David Sowby as ICRP's first paid Secretary. An agreement had been reached with the Canadian authorities that Sowby would remain as Canadian civil servant for the moment with no change to his pay and pension rights, and that ICRP would 'hire' him by paying the Canadian government the corresponding amount. This arrangement led to considerable jealousy when Sowby moved to England at Pochin's request to establish ICRP's Secretariat there. Sowby's emoluments through this agreement that was reached became arrestingly beneficial compared to the salaries that were normally paid in England for equivalent work at the time.

The Commission's choice was later confirmed by the Congress of Radiology's International Committee. Such formal confirmation was needed because ICRP was still one of the Congress' Committees, but in reality the Commission's proposal had never changed. After the Congress I received a formal message from Pochin that I had been elected to the Main Commission as well as having become a member of Jammet's new Committee 4.

After the Congress of Radiology, Lars-Eric Larsson and I travelled to Washington to discuss with the US Public Health Service a possible research grant for a project which unfortunately never came off. We had recognised that the experiences from Japan indicated that the increase in the risk of leukaemia following a one-off instance of irradiation fell mainly during the period of 3–15 years following the irradiation. In such a case, we thought, the opposite should also apply, i.e., that an increase in the frequency of irradiation, e.g., x-ray examinations, could be proven 3–15 years before the start of the illness. Sweden's good logging practice when it comes to both the incidence of illness and x-ray examinations means that there could possibly be conditions to demonstrate a connection here. It did not seem impossible to be able to obtain an American grant for the study, but bureaucracy with the application documents deterred us and lack of time prevented us.

UNSCEAR's 12th session was held in Geneva in January 1963 with Dan Beninson as Chair. The Committee's next task was discussed once the 1962 scientific report had been published. The

Committee's proposal was discussed by the UN's Special Political Committee, whereupon the general meeting in November 1963 gave the Committee the task of continuing its activity. This led to UNSCEAR beginning its third scientific report during its 13th session in Geneva with Beninson still acting as Chair. This was completed at the Committee's 14th session in New York in July 1964.

UNSCEAR's report from 1964 was thinner than its predecessor, just 120 pages. It contained two scientific Appendices. One dealt with the radioactive environmental contamination from the nuclear weapons testing. The other was devoted to cancer caused by radiation. The powerful nuclear explosions in 1961 and 1962 were proven to have led to the doubling of the estimated global dose commitment from the nuclear weapons testing compared with the estimate in the Committee's report in 1962.

From the ICRP meeting in Stockholm in 1962. Three ICRP Chairs: Gordon Stewart (1969–1977), E. E. ('Bill') Pochin (1962–1969) and Bo Lindell (1977–1985). Photo: Sven Åsberg, *Pressens Bild*.

> The Institute was closely associated with the Royal Marsden Hospital, the leading cancer centre in London. The connections between the Institute and the Hospital were complex and mysterious, and I never succeeded in understanding the ramifications of the linkage. Physically,the two organisations shared the same sites, both in London on the Fulham Road and also at the then new site at Sutton, Surrey. Many staff members were — and are — attached to both organisations. By the time I arrived in September 1958 the Physics Department had moved out to the Sutton site on the southern fringe of London. It occupied one of six Victorian buildings that had once been used as a hospital for tuberculous children, but had been unoccupied for many years. The six blocks had been imaginatively designated by the letters A to F. […]

Dan Beninson at an UNSCEAR meeting in the 1960s. Australia's representative, Don Stevens, sits at his side. Standing on the far right is Professor Louis Bugnard. Photo: The United Nations.David Sowby had started as Secretary of the ICRP in October 1962, located in Sutton in Surrey, just south of London. In his memoirs, *Man Ages* (Sowby, 2002) he describes the area where the Institute for Cancer Research at the Royal Marsden Hospital had premises and the way in which ICRP could establish its Secretariat there:

> On my way back to Canada *[from the ICRP's May meeting in Stockholm]*, I stopped off in England to see the accommodation that the head of the Radiological Protection Service, Walter Binks, who was a member of the Commission, had offered to the new ICRP secretariat. The RPS was then located on the Royal Marsden Hospital site at Sutton, Surrey, which I've already described. It occupied most of Block E, and was soon to expand into D Block, which still lay unused and abandoned. Walter took me over to show me the space he proposed to let me have. On the way he explained that the building was currently in poor condition, but that by the time I returned in October it would be completely renovated. It was just as well that he warned me, because even so I was shocked at what I saw. Windows were open to the elements, and some of them had trees growing though. Inside, the building was as it had been when it ceased to be used as a hospital many years earlier. Most of the interior consisted of long dreary wards, each with a small coal fire at one end. Walter explained that the wards would be partitioned into offices and laboratories, and he showed me the small area at the end of one ward where he proposed to construct two rooms, one for me and one for a secretary. One needed a lot of imagination and faith to see what it might look like, but I have to say that Walter was quite correct, and when I came back that autumn the entire building had been radically transformed. […]
>
> [In October 1962 I came to London to start my post as Secretary of ICRP]. On my first day at work Walter Binks once again took me over to the new ICRP office in D Block. The place was transformed from what I'd seen five months earlier. I was half-

way through our allotted span of life when I found myself again in the place where, six months earlier, a dark forest had been growing through the windows. The rusty baths and the tiny fireplaces had disappeared. Instead, the building had been completely renovated, and included two rooms for ICRP at one end of a corridor.

The first room was for a secretary, out of which my office opened. I was very fortunate with the four secretaries I employed. The last of them, Andrea Price, came to work for ICRP in 1973 and stayed on until shortly after I retired in 1985. Andrea was extremely competent and conscientious. She had a great way of bossing 'her men', as she used to call the members — all done in the nicest possible way, of course. When Andrea and I were checking out possible places, such as hotels, for meetings she could charm the management into providing all sorts of extra services. Andrea was a loyal and willing colleague, and I came increasingly to depend on her. There was only one time in the year when I knew she wouldn't be available; this was during the tennis championships at Wimbledon. Immediately after lunch during those two weeks she'd disappear, to spend hours virtually stamped to her television set.

In May 1963, Jammet's Committee 4 had its first meeting, in Paris. From the time of this meeting, the Committee would play an important role in ICRP's policy. The Committee's task was to review the application of the Commission's recommendations. ICRP deliberately refrained from formulating detailed code of practice recommendations regarding the practical application of the basic principles. One exception to this rule concerned the radiation protection recommendations regarding x rays within medicine, because ICRP was expected to have special obligations vis-à-vis the radiologists. If ICRP did not give recommendations regarding practical measures here, the Radiology Congresses might well set up a new committee for this purpose.

However, it was important for ICRP to know that the fundamental principles recommended by the Commission could be applied in practice. Explanations might be needed for the application. ICRP therefore needed a committee whose members had experience of and responsibility for the practical application, both nationally and within other international organisations. It may be interesting to see Committee 4's initial members:

Henri Jammet, Chair
Dan Beninson (Argentina)
Gordon Butler (Canada)
Hussein Daw (IAEA)
John Dunster (The United Kingdom)
Bo Lindell (Sweden)
D. Méchali (France)
Carlo Polvani (Italy)
Pierre Recht (Euratom)
Conrad Straub (USA)
E.G., Struxness (USA)
Forrest Western (USA)

As ICRP had now issued Publications 1–4 and was well on the way with Publication 5, it was time to look to the future and establish a work programme for the new Committees. It was time to start preparing new recommendations from the Main Commission. A number of explanations, clarifications and additions had come about. While awaiting a more thorough review of Publication 1, there was an interim publication which consisted of Publication 1 with the explanations and additions incorporated. This became ICRP's Publication 6, which came out in 1964. It also contained a number of new and revised MPC values for strontium-90 and transuranic elements.

However, completely new recommendations were needed in the long term. The thoughts that Failla's editorial group from 1958 had put forward had become outdated. It was also increasingly obvious that the somatic effects of radiation were not always deterministic. People began to talk about 'nondeterministic injuries' before the concept of stochastic injuries was introduced.* The main dose limits that had been introduced to avoid deterministic injuries might not give adequate protection against the risk of cancer. If there were no completely safe doses of radiation, how were people supposed to handle the dose limits?

In 1963, Pochin had already decided to appoint an editorial group to start work with the new recommendations, the ones that would be published in 1966 as ICRP Publication 9. He would be Chair of the group which, alongside him, consisted of three people: John Loutit, David Sowby and me. We ended up meeting mainly at Pochin's site at University College Hospital Medical School where he was in charge of the Medical Research Council's Department for Clinical Research.

On 27 November 1963, Sievert sent his colleagues at ICRP a letter, stating that he intended to leave the Commission. He was satisfied with having roped me in and thought it was challenging having two members from the same Institute. Sievert had always taken care to assist his own, but his sense of what was correct always came out on top. He had never proposed anyone from his own institution for an assignment for which he thought someone else was more suitable. His colleagues sometimes felt that in doing this he was thwarting them, but that was not so. He was just trying to be fair.

Sievert's letter gave rise to a number of complaints, no doubt honourably intended. The first to get in touch were John Loutit, Bill Pochin, David Sowby, Bob Stone and Karl Morgan, who was a big admirer of Sievert. Taylor also made his voice heard after the New Year. Loutit wrote that he understood that Sievert was disappointed not to have succeeded in receiving the large donations to ICRP which he had imagined, but that Sievert had quite clearly set himself an impossible task. Pochin was more optimistic and wrote:

> I feel some confidence, as I hope to be able to discuss with you in January, with regard to the financial future of the Commission, and must strongly emphasise that if this becomes possible, it will be completely down to your enterprise to safeguard the Commission's financial strength for the next five years which can enable it to develop an organisation which, once it has come to fruition, ought to be far easier to maintain on a continuous basis.

Karl Morgan was thinking along the same lines. He wrote:

> You have no reason to apologise for having failed to secure firmer ground for the Commission. It was through your action only that the Commission obtained funds to facilitate its current activity, and I am sure that, thanks to you having facilitated this lifeline, ICRP will find ways to gain additional support and safeguard its existence as an effective and impartial body for the protection of mankind in his use of ionising radiation.

In January 1964, ICRP met at the Ciba Foundation in London, but Sievert now no longer attended. On 5 February, Pochin was able to announce that the Commission was inviting Sievert to be Honorary Chairman Emeritus. He wrote to him, saying:

> I hope that you are not only in a position to accept the invitation but will also attend each of the Commission's forthcoming meetings that you have time for, and take part in the meetings on the strength of your position.

At its London meeting in January, ICRP had set up a work group with Scott Russell as Chair and Committee 1 responsible in the first instance. The group's task was to 'to consider the extent to which the magnitude of somatic and genetic risks associated with exposure to radiation can be evaluated'. To

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* Deterministic injuries do not arise until the radiation dose has exceeded a threshold value. The greater the dose, the more serious the subsequent injury. For stochastic (random) injuries, there is no threshold value for the radiation dose and the seriousness of the injury is independent of the dose.

my surprise I was asked to participate in the work. The other members of the group were Richard Doll, John Dunster, Lorentz Eldjarn, *P. Coller*, Len Lamerton, John Loutit, Harold Newcombe, Bill Pochin, David Sowby and *A. C. Stevenson*. Epidemiologist Doll and geneticist Stevenson were asked to collect the information that was needed for the group's assignment.

The group met a couple of times in Oxford and I had the honour of staying with both John Loutit and Scott Russell. Scott Russell also invited me to sit at the High Table in his college, which meant that I needed to don a dinner suit for the event. Scott Russell may have thought that his task of acting as Chair of a group which included the Chair of the Commission (Pochin), the Chair of Committee 1 (Loutit) and the Secretary of the Commission (Sowby) was sensitive, but had adequate self-confidence to cope with the task. It truly was a high policy group that he controlled, but then it was also the first time that ICRP was tackling quantitative risk assessments.

Not everyone was up for putting his reputation on the line by making quantitative estimates, but Pochin was the main one to apply the pressure. He asked the group which share of the hereditary injury following irradiation could be expected within the first few generations compared with the total for all time. No-one in the group dared to state a figure. 'It must surely be more than 1 per cent?' ventured Pochin. Everyone nodded in agreement. 'But it surely can't be more than 50 per cent?' Everyone looked dismayed – of course not! 'So we can say that the figure lies between 1 and 50 per cent?' summarised Pochin. The group protested strongly. 'We have no data to be able to make such a quantitative estimate!' said geneticists, indignantly. And this is how the discussion went on, for hours.

The group's report was published in 1966 as ICRP Publication 8, entitled *The Evaluation of Risks from Radiation*. It was the first publication in ICRP's series of reports which contained only scientific background material rather than consisting of recommendations. The previous publications 1–7 had been encased in hard, brown folders. To mark the type of new publication, the folders were blue instead and were also not hard. This distinction between brown and blue folders was kept up for a good few years.

ICRP was now making quantitative risk assessments for the first time. The risk coefficient for leukaemia[*] was assumed to be 20 cases per million of irradiated people following a dose of 1 rad of x-ray or gamma radiation (in current units, 0.2 % per sievert). For thyroid gland cancer in children, the risk was estimated at between 10 and 20 cases per million at 1 rad. For other fatal forms of cancer, the likelihood was assumed to be just as great as for leukaemia so the total likelihood of death from cancer in today's units was estimated at between 0.5 and 0.6 % per sievert.[†] For the risk assessments, ICRP assumed a linear, no-threshold connection between radiation dose and risk (the LNT assumption). The report stated that the LNT assumption could overestimate the risk at low doses but, on the other hand, it was said that there was no data for a total follow-up period of those who had been exposed to radiation, which could affect the risk assessments in the other direction.

One innovation in Publication 8 was that the report proposed that, owing to the considerable uncertainty of the risk assessments, seemingly exact risk coefficients should not be stated but the order of risk concept should be used instead. The n^{th} order of risk would then refer to a risk that was greater than 10^{-n} but less than $10x10^{-n}$. A 0.4 % risk would thereby refer to a risk of the third order.

In Publication 8, the Gompertz diagram[‡] was also used to illustrate the way in which the mortality risk changes with age. A Gompertz diagram is log-linear, i.e., states the age on a linear scale based on the x-axis but the logarithm for the annual likelihood of death is based on the y-axis. The curves stating our total annual likelihood of death then usually become straight lines for ages beyond 30 years, which

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[*] ICRP usually referred to the likelihood of death, but in other publications on risk estimates, it is sometimes difficult to determine whether the author is referring to mortality or morbidity, possibly due to the fact that the early estimates concerned leukaemia where the difference was then insignificant.

[†] This estimate was made in 1964 and published in 1966. Note that the risk coefficient that I had assumed at a seminar in Geneva in 1960 for the total risk of death from cancer was 0.7 % per sievert (see Chapter 14).

[‡] The formula that forms the basis for the diagram was proposed in 1825 by the British insurance mathematician Benjamin Gompertz (1779–1865). It was modified in 1860 by another British insurance mathematician, Matthew Makeham (1827–1891), who added an age-independent term. In the modified form, an alternative name for the formula is 'the Makeham formula' or 'the Gompertz–Makeham formula'.

means that the likelihood increases exponentially with age (it doubles for approx.. every seventh year). The total mortality risk stated for British men in the diagram in Publication 8 was 1 % (i.e., a risk of the second order) at approx. 53 years of age; for Swedish men of the same age, it is now approx. 0.4 %, i.e., a risk of the third order. Many people are not aware of how great the natural annual mortality risk actually is, which means that they are overestimating the importance of small added risks which might not tangibly change their overall risk situation. The Gompertz diagram was later used by ICRP in its Publication 60 (1990).

At its meeting in London in 1964, ICRP also got Jammet's Committee 4 to appoint a Task Group with Forrest Western as Chair which had the task of reviewing ICRP Publication 6, i.e., the revised version of ICRP's recommendations from 1958, for the purpose of looking at any difficulties there may have been regarding the interpretation and application. This was Committee 4's main assignment. I ended up in the Task Group as well, along with Pierre Recht from EURATOM, Hussein Daw from IAEA and *David Méchali*. Méchali was a small, intelligent man who coped very well despite having lost an arm. Unfortunately, he did not speak English. Méchali was indispensable to Jammet and was Jammet's guarantee that the work group would function as expected. It was on this work group's recommendations that Committee 4 later acted. The group's function began when Committee 4 met once more in Paris at the end of May 1964.

On the final day of this meeting, we went out together to eat dinner and ended up at a small restaurant in Les Halles where we all ate an onion soup which disagreed with us somewhat. John Dunster and I shared a toilet at the hotel and had a troubled night. We both flew to London the next morning. A big, chauffeured official car awaited John while I took a taxi to my hotel. A few days' work with Pochin with ICRP's editorial committee now lay in wait.

19. FINALE

ROLF SIEVERT holds the Swedish record for pensionable age reached among government officials; he succeeded in remaining in service until almost the age of seventy ('an age which ought to correspond to at least eighty the way my body is'). He could point to many reasons for this: his international assignments as well as uncertainty as how his comprehensive imperium would be transformed following his departure. Another reason, albeit implicit, was that he wanted to assure himself that Rune Walstam and I could succeed him, and we had still not defended our theses until 1965.

Sievert's job actually consisted of several jobs, something that would become evident when he retired. His Professorship was a university job. His position as head of medical physics, primarily at *Radiumhemmet*, was primarily a matter for the hospital board, but because *Karolinska Sjukhuset* was a university hospital, it would be necessary to take a stand on the way in which the Professorship would be involved. The new Radiation Protection Institute was to have its own manager and become a state authority.

When Sievert's Professorship was declared up for grabs, Rune Walstam and I applied for it, but so did Rolf Björnerstedt, Gunnar Hettinger and Karl Johan Vikterlöf. I also applied, one year later, for the post of Assistant Professor in Medical Radiophysics in Gothenburg after Sven Benner, for the sake of security. Radiation biology was separated from radiophysics and radiation protection, and Arne Forssberg's job now became a Professorship.

Sievert was not the only pioneer who withdrew. Manne Siegbahn had certainly retired from his Professorship at the age of 67 in 1953, but he had remained as the person in charge of the Nobel Institute. On 1 July 1964, he also left this post and the Nobel Institute changed its name to the Research Institute for Atomic Physics. And in the USA, Lauriston Taylor retired from his job at the National Bureau of Standards on 18 December 1964. In 1965, Robert Thoraeus also retired and a tribute was made to him at a dinner for the Swedish Society of Medicine.

In July 1964, Sweden was visited by *N/S Savannah*, the first nuclear reactor-driven merchant ship. The name 'Savannah' had been chosen because this was the name of the first ship that travelled the Atlantic by steam engine and it was American. The enterprise was carried out in 1819. *N/S Savannah* did 20 knots and had a displacement of 22 000 tonnes. She was almost 200 metres long and could take 60 passengers and a 9 400-tonne load. The ship got permission to put into port at Gothenburg. Sievert was suspicious and wanted to ensure that no radioactive substances were released into Swedish water. Sievert monitored the visit with the help of Jan Olof Snihs and research assistant *Chris Wilson*, a young eccentric Scot with an awe-inspiring beard, although there were no mishaps. *Savannah's* virgin journey was meant to have marked the introduction of an era of reactor-driven merchant and passenger ships; this hope was not fulfilled, however.

The 1958 Radiation Protection Act regulated the special competence required for work with radioactive substances and, like the 1941 Act, the Act required you to have permission from the Radiation Protection Authority for such work. The Radiation Protection Act was primarily intended to protect the personnel and gave no instructions for the protection of the patients who were exposed to radiation during medical examinations or treatment. This meant that in 1961, the Radiation Protection Act had been supplemented with a circular from the Medical Board containing guidelines for the use of radioactive nuclides (commonly known as 'radioactive isotopes') within the healthcare system. The circular required the hospital at which there was an x-ray department and a clinical-chemical central laboratory to also have a local clinical Isotope Committee.

The arrival of the isotope circular was one of many reasons for the greater need for a radiochemist at the Institute of Radiophysics. Arne Hedgran chose from a number of interested parties and, in 1964, decided on a well-qualified Norwegian, *Ragnar Boge* (1933–1990) who had come to Gustaf Werner's Institute for Nuclear Chemistry in Uppsala to do research. This was a stroke of luck. Ragnar was happy at the Nuclear Physics Department and ended up becoming one of the Radiation Protection Institute's most valuable – and liked – colleagues. When he first started he made a great deal of effort to support the Isotope Committees and assess their reports.

On 16 March 1962, the board of *Karolinska Sjukhuset* had set up an Isotope Committee with five members. *Rolf Luft* (1914–2007), Professor of Endocrinology was elected as Chair. I became one of the members. The other members were pharmacist *Rolf Barkman* (1921–1985), who became the Secretary of the Committee and Assistant Chief Physician at *Radiumhemmet*, Assistant Professor Jerzy Einhorn, and the Chief Physician at the Central Laboratory of Clinical Chemistry, Bertil Swedin.

Karolinska Sjukhuset's Isotope Committee became a good reference for Isotope Committees at other hospitals, particularly owing to its close contact (through me) with the Radiation Protection Authority, initially the Radiation Protection Committee and from 1965 the National Radiation Protection Institute.

The Isotope Committee decided to divide the use of radioactive nuclides within the hospital into four groups. All use for radiation treatment would be located in *Radiumhemmet* and be under the leadership of radiologists and radiophysicists. The diagnostic use of activities that were high enough to need radiation protection monitoring and where the dosage could involve radiation protection problems should also be located in *Radiumhemmet* but could in special cases take place elsewhere, although this had to be in cooperation with *Radiumhemmet*. All scintigraphies were among the activities which came under such use.

The third group covered diagnostics using low activities. The Committee thought that it was in the interests of the patients and the doctors for such activity to be permitted with some reservation and for children and pregnant ladies only in exceptional cases. The fourth group covered animal experiments and laboratory trials without any patient being affected. Bearing in mind the risk of contamination, the Committee wanted such experiments and trials to be carried out using the lowest useful activities. This could require sensitive measurement instruments.

The Committee proposed that a central laboratory ('the isotope centre') be established at the military pharmacy and that this laboratory be responsible for purchasing the radioactive substances that were used at *Karolinska Sjukhuset*.

The third group also included the use of radioactive nuclides on voluntary trial subjects for the purpose of finding out the normal values for uptake and retention in the body. Particularly difficult considerations were involved when the trials were carried out on patients because the level of free will could then always be questioned. On the one hand it is not easy for a patient to say no when a doctor explains the purpose of an examination in research terms. On the other hand, many patients realise that the only possibility of finding out more about their particular illness is by examining the actual patient, even if the result only benefits other patients later on. On the other hand, it is bad practice if doctors use patients as readily-available research subjects in respects that do concern the patient's illness. Patients must be shielded from research that can equally well be carried out on completely voluntary trial subjects who are not patients.

On 17 November 1964 I wrote to Rolf Luft, Chair of the Isotope Committee, proposing that the Committee start discussing which doses of radiation could be considered acceptable for research on human beings. I proposed, with the support of discussions within ICRP, that in such cases the doses of radiation for each year should be less than those permitted for a week when it came to personnel who worked with radiation (using today's magnitudes and units, this corresponded to an annual effective dose of 1 mSv at the time). Better guidance was to come from ICRP in the near future and with the 'Declaration of Helsinki' (WHO, 1965). The Committee followed my proposal. Regarding trials on voluntary trial subjects, the level of free will also had to be indisputable and the participants had to have the opportunity and conditions to understand what the examination was about and any risks it could entail.

At the end of October 1965, *Karolinska Sjukhuset's* Isotope Committee and the Radiation Protection Institute arranged a conference for representatives of the country's other Isotope Committees. Detailed discussions were held during the afternoon following an introductory lecture in the morning. A commentary of the discussions was stencilled into 250 copies and sent out to the Isotope Committees on

15 January 1966. It gives a very good and interesting picture of the problems that were faced at the time and the conceivable solutions.

Kurt Lidén drew attention to the fact that the use of some of the most regularly-used radionuclides was no longer tenable. It concerned cobalt-60 in vitamin B-12 and iodine-131. For these there were alternative nuclides that gave much lower doses of radiation. I said in the discussion:

> The risk is still very small of course. It is not a case of our choosing to either use iodine-131 for some examinations or not actually do the examinations. The risk is so small that the benefit of the examinations obviously far exceeds the risk. There is another way of looking at it when there are other alternatives to choose from which give lower doses of radiation; you then of course choose those and in doing so work towards establishing the use of those alternatives.

Dr *Bertil Nosslin* ('inverse Nilsson', 1919–2014) in Malmö thought the risk could be rather substantial:

> When three cases of thyroid cancer* are seen in the space of one year from the iodine treatment for thyrotoxicosis†, a little more caution might be exercised. The occurrence of such a concentration may be a complete coincidence but you do still get cold feet.

However, Dr *Folke Edsmyr* (1926–1985) at *Radiumhemmet* thought the risk was small:

> At our isotope laboratory at *Radiumhemmet* we have performed follow-up examinations of 2300 people treated for thyrotoxicosis and toxic nodular goitres. There is an observation period of between 3 and 13 years for these. We have not found any people with thyroid cancer and nor have we found any leukaemia in the material. 13 years is of course a very short observation period, but it must mean something.

I then attempted to summarise what was known about the risks of thyroid cancer caused by radiation:

> When talking about the risks in connection with the irradiation of the thyroid gland, I should have made it a little clearer that the available data is slightly contradictory. The x-ray irradiation of children leads to a relatively high risk of thyroid cancer. The available materials concerning iodine for adults do not lead to such figures. There have been discussions as to whether it is because the doses of radiation may have been too high in these cases. That is one explanation. Another explanation is that the children are more sensitive to radiation. The expert committees I know of have still not reached agreement regarding this matter. When it comes to the risk of the foetus if the mother is given iodine, I personally do not know of any assessment that has been made of this.

When the discussion began on experiments on voluntary trial subjects, Professor of Veterinary Science Bertil Åberg had a firm idea:

> Mr Chairman, in order to get the discussion started, may I declare immediately in this context that I think the first criterion for a volunteer to accept a dose is that the volunteer needs to have understood what Professor Lindell recounted this morning. I am sure Professor Lindell agrees with me that there are a very large number of scientists in this field who, until now, have not understood the intention of the ICRP recommendations. How can one then expect the medical students who want to earn themselves fifty *kronor* to understand what they are actually doing? My view is that the only people who should be given doses on a voluntary basis are the scientists themselves. They should have well and truly acquainted themselves with the problems to their full extent. Other people are not exactly likely to be capable of comprehending

* Thyroid cancer.

† 'Hyperthyroidism', i.e., enlarged thyroid which overproduces certain hormones.

> what they are doing. I know this is unpopular and that there are many who say 'it is easy for someone who deals only with animals to say that we will not be able to obtain normal material', but I do not think it is ethically defensible to use other voluntary personnel except for those who really do understand exactly what the risk assessment in a context like this is.

I had my own comment on the matter of free will:

> I can shed a little more light on the matter by having spoken to some British people who have been involved in similar matters. They appear to have a certain work principle when it comes to assessing the level of free will, including not approving trials where the doctor goes to the patient and says: 'My dear Mrs. Smith, we would like to be able to examine you – it's not absolutely necessary, but it's extremely important to us.' There is no need for the patient to agree to this; she is not actually in a dependent situation. However, the patient believes that she is in a dependent situation, and this is enough. A notice is then instead put up stating that such and such an experiment will go ahead, that volunteers are needed and these are then asked to search for so and so. This system requires an active input by those who are volunteering – patient, student or personnel – and it puts a completely different slant on volunteering. If you then link this with information that the dose level from the Isotope Committee and other expert bodies is guaranteed to be low enough to make the risk negligible, I think we have sufficient guarantees - but otherwise we do not.

An account of the Isotope Committee's activity was published in *Läkartidningen* in 1966. Many cases could be tricky to deal with. One case was particularly difficult. For some reason, one of the hospital's x-ray diagnosticians, well qualified and ingenious, had been in contact with a young lady who was dying of acute leukaemia and who was a patient at *Radiumhemmet*. The radiologist, who felt deep sympathy for the patient, wanted to take drastic measures to save her life. He resorted to a completely unconventional method for which there was no good reason to believe that it would succeed – he injected Thorotrast, the radioactive x-ray contrast medium which was no longer normally used on people but which was still used within veterinary medicine. His hope was that the radiation from the contrast medium would kill the malignant cells.

The venture, which took place on a desperate basis with the best of intentions, contravened a number of important rules. Radiation therapy was reserved for *Radiumhemmet*, the patient's GP was not involved, there was no scientific reason to believe that the action would be any use… it was without any tried and tested experience whatsoever.

When the radiologist requested the Isotope Committee's permission to continue treatment, the Committee ended up in a difficult situation. No permission could be granted, but the radiologist had breached a number of provisions and ought to be reprimanded. In such a situation, solidarity with a medical colleague has a conscious or subconscious influence on your action. You do not want to be unfair and feel uneasy about telling a colleague off while at the same time realising that justice must somehow be done. I needed to convince some of the members to actually come to the meeting where the matter was to be dealt with initially.

The radiologist had been called to the meeting and was given the opportunity to explain himself and answer questions. It was decided that I and the hospital's pharmacist (who had been classmates with me in secondary school) would write the radiologist a proposed statement with a copy to the hospital's Board of Directors. I had insisted on the latter since I had the impression that the others wanted the matter to be shrouded in silence. This worried me because I was unsure as to whether the whole thing was a case of 'Lex Maria', an announcement from 15 January 1937 which obliged the board of a healthcare institution to report to the Medical Board and the police any instance of treatment having caused injury, or whether there was reason to believe that injury could possibly occur. The announcement had been named after some tragic events at the Maria Hospital in Stockholm in August 1936. Four people who were treated at the hospital for relatively banal afflictions died within a week because they had been given injections containing a disinfectant rather than the anaesthetic intended.

In the case that was now relevant, it would have been a clear case of Lex Maria had the treated lady not been dying (which meant there was no time for injury to make a difference to her life). However, I thought it would be a matter for the board to determine whether it wanted to report the event and that the Isotope Committee was not entitled to keep the case quiet.

It turned out that a number of versions of the letter to the radiologist that the other members of the Committee wanted me to accept either had the reprimand missing or someone had written on it that a copy should go to the Board. Although the letter would be going from the Committee and should normally be signed by the Chair and the pharmacist, who was Secretary, the latter did not want to sign it, but proposed that I should sign it instead. Luft asked the pharmacist if he had 'got the wind up' and urged him to sign the letter along with the Chair because the only alternative was for the letter to be signed by everyone. At a vote as to whether what was on the letter should say 'copy to the Board' I lost by four votes to one, but I dissented. Luft then called a new meeting at which I asked whether members considered it all to be a routine case or an important matter.

Everyone's answer apart from Einhorn's was that it was an important issue; Einhorn would have preferred to call it 'a question of another kind'. He proposed that an extract of the minutes be sent to the Medical Director justifying it as 'for the purposes of avoiding repetition'. The others agreed with this, but Einhorn suddenly changed his mind when he realised that this would bind the Medical Director if measures were to continue. In the end, everyone accepted my previous proposal and now voted unanimously for the letter to show 'copy to the board' and that Luft would discuss the principle matter with the hospital's director.

I was happy with this but at the same time felt like a dogmatist, which shows how easy it is to be infected by cronyism and to convince yourself that, despite everything, an irregularity is defensible and therefore does not need to be considered at the correct level.

At the start of the 1960s, primarily Sievert's institute in Stockholm and Kurt Lidén's institution in Lund were preoccupied with measurements of the remaining radioactive contamination from the big nuclear weapons tests. When it came to measuring the gamma radiation from the human body, the radon in the surroundings was a disruptive element. The maximum amount of radon that can be accumulated in the body corresponds to the approximate amount of radon in 50 litres of breathing air, which usually led to only an insignificant disruption. More disruptive was the amount of bismuth-214 (radium C) that could accumulate in the lungs after inhaling daughter products of the radon in the inhaled air, but the disruption was short-lived. However, at the Institute of Radiophysics, we feared that the quantities of radon that were in some drinking water could cause more troublesome disturbances. To investigate how things stood with this matter, we began to look at the radon levels in drinking water as well as to try and measure the length of time for which radon remained in the body after it had got there, courtesy of drinking water.

We found that the levels of radon in tap water in a number of communities from which we took samples could vary from less than 0.1 of a nanocurie per litre (the measurement limit at the time) to 33 nanocuries per litre (i.e., in today's units, from less than 4 becquerels per litre to approx. 1 200 becquerels per litre). The maximum values were found in Ängelsberg, where drinking water came from a deep bore well, but a number of larger communities (such as Avesta, Hedemora, Ludvika, Sollentuna, Uppsala and Örebro) had levels of radon of between 1 and approximately 2 nCi/l (40–80 Bq/l). It was a strange feeling to find that the vessel that I had used to take water from the tap in our kitchen in Sollentuna had become so radioactive that I could not use it for other samples for a good while to come.

It surprised us to also find measurable quantities of radon in some dairy milk. This led us to start taking samples from individual farms. Some farms in Bergslagen produced milk which showed radon concentrations of around 1 nanocurie per litre. We found that drinking water from deep bore wells there contained radon levels of approx. 40 nCi/l (approx. 1 500 Bq/l).

In autumn 1966, Bengt Håkansson was given the task of doing a field study of the level of radon in a number of wells in Vimmerby's dairy district, which gave similar results. However, it was difficult to explain how so much radon could get into the dairy milk.

When it came to examining the length of time for which radon from drinking water remained in the body we cooperated with Wilhelm von Döbeln, who wanted to try an organic plastic scintillator which he intended to use to determine muscle masses. To comply with Bertil Åberg's views on the responsibility of the scientists we did the experiment on ourselves. We were given water containing extra radon, with levels up to 100 nanocuries per litre (approx. 4 000 Bq/l), from Masugnsbyn in Lapland. It turned out that most of the radon disappeared rapidly from the body; only a few per cent remained after three hours. The secretion was then slower. Based on our measurement results, we were able to calculate the dose in the stomach at 200 millirems per microcurie of radon consumed (corresponding to approx. 50 microsieverts per becquerel). However, it later turned out that the greatest doses of radiation due to the presence of radon in tap water did not come due to having drunk water but as a result of having inhaled daughter products of the radon which had left the water in the kitchen and bathroom and which is found in indoor air.

The report that Wilhelm and I published about the radon in *Arkiv för fysik* (‘Archive for Physics’) was, along with my essay in *Health Physics* in 1960 on the dose commitment (Lindell, 1960), the most important part of my doctoral thesis. It was now no longer necessary to write an independent doctoral thesis; it was enough to collate a number of essays that had already been published and give them a cloak that emphasised their merits. Following significant hardships, both Rune Walstam and I were at long last ready to defend in spring 1965. Rune defended on 8 April with Berndt Waldeskog as faculty opponent. I defended on 10 April with Bengt Hultqvist as faculty opponent and David Sowby as the second opponent. For David’s sake I had asked for the defence to be able to take place in English. In the evening of that same day, Rune and I held a joint doctoral dinner at *Lärargården* on Lidingö. Unfortunately, Sven Hultberg was too unwell to attend; he died on 24 April.

A letter from Lars Gyllensten congratulated me on having ‘produced a thesis in spite of all national and international assignments, organisations and information that you have been overwhelmed with for many years’. At the conferment ceremony in ‘The Blue Hall’, a doctorate was also conferred on Barbro Westerholm[*] as well as veterinary medicine honorary doctorates on Bertil Åberg and, to my surprise, John Loutit.

At the end of April 1965, ICRP met with its four Committees in Stockholm. This was the first time the whole of the organisation had met in its new form which had been determined in 1962. The meeting took place in the new building which would accommodate the new Radiation Protection Institute but where Sievert still resided as head of his Institute of Radiophysics. Two of the Committees sat on the top floor, and it just so happened to be the two whose Chairs had difficulty staying on good terms. Karl Morgan’s Committee 2 sat in the seminar room while Henri Jammet’s Committee 4 sat out in the big laboratory hall, maybe no more than fifteen metres from there but with a closed door in between them.

The boundary between the two Committees was unclear. Morgan’s Committee would draw up recommendations regarding protection against what was known as ‘internal contamination’, i.e., radioactive substances that could get into the body. Jammet’s Committee, which would concentrate on problems with the application of the Commission’s recommendations, was in the process of drawing up what would be Publications 7 and 10 and concerned measurements for the purpose of determining doses of radiation in the event of internal contamination. Morgan, whose stubbornness was legendary, totally ignored what Jammet’s group was doing. Morgan irritated Jammet, who had a southern temperament. Cooperation was needed between the Committees to prevent the duplication of work, but it took place through representatives as though it were a matter of negotiations between belligerent powers. Jammet’s representative was David Méchali who, despite his language difficulties, was sent to Committee 2 time after time where Morgan’s closest aide, Walter Snyder, did his best to prevent a total breakdown in relations.

On 30 June, Sievert retired and his imperium was divided up into three main parts: the Institute of Radiophysics, now purely a university institution but with a duty for the Professor to take charge of the medical physics activity at *Karolinska Sjukhuset*, the new National Radiation Protection Institute and Arne Forssberg’s Institute of Radiobiology. No heads of the first two activities had yet been appointed, however. Sievert personally continued with his self-imposed preparedness report and a number of

[*] *Barbro Westerholm* (1933–) later became Director General of the National Board of Health and Welfare, Medical Director at *Apoteksbolaget* and a Liberal Party politician.

assignments for the Royal Academy of Sciences. He and his wife Astrid had moved to a small apartment in Solna, but the old official residence on the top floor of the Institute of Radiophysics is where he industrially sorted paper into innumerable piles on temporarily-erected tables.

The High Voltage Hall, Sievert's dashed hope, was no longer in use. Sievert's final big achievement was managing to sell most of the fixtures and fittings, some to those who had once donated them! The large condenser batteries were taken over by plasma physicist *Bo Lehnert* (1926–) and the fusion research. There was a makeshift extension in the hall which you entered from the long corridor, and this allowed for a couple of extra work rooms.

In 1965, a couple of new societies were established. On 28 March, the Nordic Association for Clinical Physics was formed in Copenhagen with *C. B. Madsen* from *Radiumstationen* in Århus as Chair. The Swedish board members were Vikterlöf and Walstam, who were also elected as Treasurer and Secretary respectively. In Stockholm, the Swedish Radiobiological Society was formed on 6 April in *Radiumhemmet's* lecture theatre. The invitation to the inaugural meeting had come from the less formal 'radiobiology club' with Torbjörn Caspersson as Chair. Arne Forssberg became Chair of the new Society, Arne Nelson its Deputy Chair and Dr. *Per Jakobsson* its Secretary. Other members of the Society's first board were *László Révész*, Åke Gustafsson, Börje Larsson and *Gunnar Ahnström*.

On 12–19 September 1965, ICRP's Main Commission met in Fiuggi outside Rome where the 11th International Congress of Radiology was to be held the following week. Pochin continued as Chair but Bugnard stepped down as Deputy Chair and was replaced by Gordon Stewart from Chalk River. The Commission now had a Russian member, Professor A. A. Letavet, but it continued to be heavily dominated by Anglo-Saxons. Of the Commissions thirteen members, eight were from Canada, England or the USA and none from Africa, Asia or South America. The international representation of the Committees was no better. The European Anglo-Saxon dominance was interrupted by just one Japanese and one Argentinian – Dan Beninson of Committee 4.

I, rather reluctantly, became Chair of ICRP's Committee 3 for protection against external radiation, but was comforted by the fact that Lars-Eric Larsson consented to being Secretary of the Committee. I still felt utterly out of my element – I would be steering a group containing veterans like Harold Wyckoff and Dale Trout and which still included the former Chair Eric Smith. The other two members were the head of Brookhaven's Radiation Protection Fred Cowan and *Jean Dutreix*, a well-known French radiologist. However, it was an amiable and knowledgeable group and I ended up being Chair thereof for twelve years.

However, the most important event at the meeting in Fiuggi was that the Commission approved ICRP's Publication 9, the new fundamental recommendations, exactly as the editorial group (Pochin, Loutit, Sowby and I) had drawn them up. Pochin was the main person to make his mark on the text. Publication 9 meant a distinct paradigm shift. It was now no longer the deterministic injuries and the threshold values for the dose of radiation which constituted the grounds for the radiation protection concept. It was not possible to count on a few completely safe doses. Paragraph 52 of the new recommendations would be of vital importance:

> (52) As any exposure may involve some degree of risk, the Commission recommends that any unnecessary exposure be avoided, and that all doses be kept as low as is readily available, economic and social considerations being taken into account. It should be noted that the dose limits are intended for planning the design and operation of sources leading to foreseeable conditions of exposure; the setting of 'action levels' for exposures from uncontrolled sources depends on other considerations.

The new recommendations contained a section on the concept of 'risk'. At the time, the Commission used the word 'risk' synonymously with 'the likelihood of injury'. A linear, non-threshold, dose-response dependency was assumed:

> The assumption is made that, down to the lowest levels of dose, the risk of inducing disease or disability increases with the dose accumulated by the individual. This assumption implies that there is no wholly 'safe' dose of radiation.

The Commission also said:

> On the assumption that the risk of radiation injury *[this was referring to 'leukaemia and other malignancies, and of hereditary effects', which are now called stochastic injuries]* is directly proportional to the accumulated dose, it follows that exposure from natural background radiation carries a probability of causing some somatic or hereditary injury, which would be present even without the radiation of man-made exposures. […]
>
> The commission also believes that the risk resulting from exposures received from natural background radiation should not affect the justification of an additional risk from man-made exposures, and this will be the case if the frequency of effects is proportional to dose so that risks due to different sources of exposure are simply additive.

The last point is very important. It was now realised that each source of radiation or activity could be assessed separately without needing to take into account other instances of irradiation. This is of great significance to the practical radiation protection activity.

It was now understood for the first time that different criteria had to apply to accidents and normal situations where the source of the radiation was under control. The dose limits that apply to the normal situations are not intended for accidents, where it is instead a matter of intervening with protection measures; the dose limits for normal situations cannot be used as 'action levels'. It was emphasised that the condition for intervention was that the protection measures would improve the situation, i.e., do more good than harm.

The new recommendations retained the term Maximum Permissible Dose (MPD) for irradiation in the work life. However, this expression was considered to be unsuitable for the limit values that were recommended for the public. For these, ICRP quite simply used the name Dose Limit (DL).

Regarding the MPD, the age-dependent dose limit was abandoned and a general limit value of 5 rem (50 mSv) per year was recommended now for whole body irradiation. 1/10 of the MPD was recommended for the DL. Ever since then, time after time there have been discussions concerning the justification of permitting doses of radiation for radiation workers which are ten times the size of those for the public. People forget that the limit for the workers was the original limit and that in 1965, ICRP's problem was choosing an appropriate limit for the public. The opinion was that this limit had to be lower than the MPD for a number of reasons: the dose to individual members of the public cannot be measured in each individual case in the way that it can for the radiation workers; members of the public include individuals who are children or whose health is not as good, and individual members of the public are irradiated for a longer period of time than people who work with it.

A few years would pass until it was realised that the distinction between 'the public' and 'people in radiation work' was not completely logical. The radiation workers will be exposed to the same irradiation as the general public after their working hours have finished. It is not a question of different categories of people but of different categories of irradiation: 'general irradiation' and 'irradiation at work'. The fact that the dose to individual members of the public cannot be measured directly but has to be estimated on the basis of knowledge of the various characteristics of the different sources of radiation emphasised the importance of source-focused rather than individual-orientated assessments. The Commission wrote:

> The dose limitation for members of the public is a more theoretical concept *[than the MPD]*, intended to provide standards for the design and operation of radiation sources so that it is unlikely that individuals in the public will receive more than a specified dose. The effectiveness of this is checked not by observing individuals but by assessments through sampling procedures in the environment and statistical calculations, and by a control of the sources from which the exposure is expected to arise.

In ICRP's Publication 1, the Commission had presented an illustrative apportionment of the genetic dose. Publication 9 instead gave an estimate of conceivable contributions from different sources of radiation. The doses of radiation from medical examination and treatment was completely dominated by contributions that were five times as high as those from all other sources of radiation put together (with the exception of natural radiation).

The Commission finally had the following to say about the length of the working period and extended holidays:

> The commission considers that with the present maximum permissible exposure levels no special treatment of radiation workers with respect to working hours and length of vacation is needed.

At the Congress of Radiology the week after ICRP's meeting in Fiuggi, the Academy of Sciences' gold medal was awarded to Val Mayneord. Bob Stone was more tired than usual. He told Lars-Eric and me that he knew a man who had a parotid tumour and who saw the cancer as a clock that was ticking away the time he had left. We realised that he was talking about himself. Elis Berven was also having symptoms from his cancer and asked Lars-Eric to help put a dressing on one arm. He said, apologetically: 'I've known many people who've aged with healthy bodies while their intellect had failed them, but here am I now with full intellect but with a body that's failing me. It's much worse ….'

In February 1964, Karl Morgan had started to devote himself seriously to attempting to create an international radiation protection society, an equivalent of the American Health Physics Society of which Morgan was Chair. He discussed this with a number of radiation protectionists all over the world. He is said to have written a thousand letters to create contacts and determine the interest. Sievert was one of his contacts, but Sievert was initially apprehensive. However, one person who did take up Morgan's proposal with life and soul was the Belgian Professor *Samuel Halter*, Director General within the *Ministère de la Santé Publique et de la Famille*, strange-sounding name that it was. Halter had similar plans of his own. Deliberations with Halter and others took place in London on 1 February 1964 and the result was reported at a Health Physics Society board meeting on 14 February. The Board then set up a Temporary Committee with Morgan as Chair to continue the investigation. The thinking was now along the lines of an international society with national or regional societies as members; the problem was that very few proper radiation protection societies, apart from the Health Physics Society, had yet been formed. The British UK Society for Radiological Protection was early, however - it had already been formed in 1963.

There was now space for initiative here on the part of the determined Sievert. He succeeded in getting 45 names (including his own) on a circular that proposed the formation of a Nordic Society for Radiation Protection and convened an inaugural meeting in spring 1964. The meeting took place in Stockholm with Sievert as Chair on 10 June 1964 and was attended by 53 people, 8 of whom were from Denmark, 5 from Finland, 1 from Iceland and 5 from Norway. The society was formed and was indeed named the 'Nordic Society for Radiation Protection' (NSFS, Nordiska Sällskapet För Strålskydd). Sievert was also elected as Chair of the Society's Board and the membership fee was set at 10 Swedish kronor. Physicist *Stig David Johansson*, employed in 1960 by Hedgran's Nuclear Physics Department, became Secretary.*

In summer 1966, the British Hospital Physicists' Association did a round trip led by Alan Jennings for recreational and study purposes, and then also visited Sweden and Stockholm. The visit to the Institute of Radiophysics and the Radiation Protection Institute were thought to be productive. The slightly eccentric Carl Gösta Rylander aroused attention when, during a lunch, he was singled out by a fly perching on his sandwich. He nodded and proceeded to take a big bite of sandwich and fly. He then said with the air that is otherwise usually characteristic of the British: 'Yes, it's the best season for them now.'

On 15–17 September 1966, the Nordic Association for Clinical Physics met at Hangö.

At the international level, Morgan's Advisory Committee met on 11 June in Gatlinburg, just south of Knoxville in Tennessee, where they decided to call a *pro tempore* general assembly meeting in Paris in

* It may be of interest to see the names of the 45 who signed the convening letter: A. Aarkrog, J. Ambrosen, S. Benner, E. Berven, L. Carlbom, T. Caspersson, B. Chr. Christensen, F. Devik, K. Edvarson, R. Eker, L. Eldjarn, A. Forssberg, L. Fredriksson, *K. Garder*, H. L. Gjørup, P. Grande, *K. Gussgard*, L. Hannerz, *S. Hauge*, A. Hedgran, M. Helde, *E. Hoff-Jørgensen*, *G. Jenssen*, E. Juel Henningsen, J. Koch, K. Koren, *K. Kristensen*, L.-G. Larsson, H. Levi, K. Lidén, B. Lindell, L. Lorentzon, K.-G. Lüning, J.K. Miettinen. S. Mustakallio, A. Nelson, P. Oftedal, G.Petersen, J. Rydberg, K.E. Salimäki, J. Schultz-Larsen, R. Sievert, *E. Stedje*, T. Westermark and B. Åberg.

November 1964. This decision was accepted by the Board of the Health Physics Society in Cincinnati on 15 June, and after this initial help from the Americans, the international society was left to its own devices.

The Paris meeting brought together 45 people who represented 15 countries or regions with potential radiation protection societies. The Nordic Society was represented by Sievert, Grande, Koren, Salimäki and me. Sievert invited us to dinner and questioned the taciturn Salimäki in particular, whom he did not know as well as Grande and Koren.

A decision was made in Paris to form an international radiation protection society which was named the International Radiation Protection Association, abbreviated to the IRPA, and established fundamental statutes. A Provisional Executive Council was set up with Morgan as Chair. Sievert was elected to this Board but succeeded in having me accepted as a substitute, which was something that nobody else had.

The members of the Provisional Executive Council were:

> K. Z. Morgan (USA), Chair; P. Caldirola (Italy), Deputy Chair; P. Bonet-Maury (France), Secretary; P. Courvoisier (Switzerland), treasurer; W. S. Snyder (USA), publicationsresponsibilityig; P. Spaander (The Netherlands); A. Benco (Italy); *Y. Feige* (Israel); K. Koren (Norway); *K. Becker* (Germany); *A. M. Marko* (Canada); R. M. Sievert with B. Lindell as substitute (Sweden); S. Halter (Belgium); *G. Zedgenidz* (The Soviet Union); R. Maushart (Switzerland, Germany, Austria); F. Yamasaki and Y. Nishiwaki (Japan); *B. A. J. Lister* and W. G. Marley (The United Kingdom); F. Duhamel and H. Jammet (France); *H. H. Abee* and *W. T. Ham* (USA), and *J. Solanas* (Venezuela).

The Provisional Executive Council met in Los Angeles on 18 July 1965 and then again in Paris on 16 December.

On 3 October 1965 I drove to Lund with Arne Hedgran to take part in a WHO conference concerning the radiation protection of patients. We continued to Copenhagen where a Nordic conference on medical examinations of personnel in radiation work was held. A summary of the discussions in Lund was published by WHO's European office in 1966 and contains a great deal of valuable information.

Representatives of the Nordic Society for Radiation Protection at the IRPA's *pro tempore* general assembly in Paris in November 1964: Bo Lindell (Sweden), Kauno Salimäki (Finland), Kristian Koren (Norway) and Per Grande (Denmark). Photo: Unknown.

Mass examinations were discussed and, for the first time in a wider context, the mammography was mentioned, which had shown promising results in a number of experimental applications. Individual fluoroscopy examinations were assessed by category. Professor Olle Olsson, who held one of the general lectures, is quoted as having said:

The propensity of doctors to use this [i.e., fluoroscopy] is based on two erroneous ideas, the first of which is that illnesses can be precluded by using fluoroscopy (which is incorrect), and the second being that the dose of radiation is negligible, which is also incorrect.

There were substantial discussions about whether the doctor who is actually responsible for the patient should determine whether there was a need for an x-ray examination, or whether this should be determined by the radiologist. Some radiologists were of the view that they were not 'photographers' who were carrying out commissioned work. In the end, it was agreed that the referring doctor ought to consult the radiologist regarding the need for a certain examination. Most thought that the radiologist should be able to refuse to do an examination if he or she thought that this was not in the best interests of the patient.

On Wednesday 27 October 1965, a tribute dinner was held for Sievert at *Lärargården* on Lidingö. Both early and more recent colleagues were present. Sievert took *Nanna Svartz* (1890–1986) into dinner. Hannes Alfvén gave a speech where he reminded everyone of the interesting time when, under Sievert's management, the Swedish physicists had attempted to mobilise their wealth of ideas for the benefit of the Defence. He recalled Sievert's great interest in butterflies and how he used to annoy him by calling him the 'Macrolepidologist', or the big butterfly collector.

On 1 November, I became head of the new Radiation Protection Institute. It was the Secretary of State at the Ministry of Health and Social Affairs, Lars-Åke Åström, who rang me. We had known each other very well since the ILO meetings in Geneva. Åström got straight to the point:

> You are at the top of the list of nominees for the Professorship of Radiophysics after Sievert, but he wants you to become head of the new Radiation Protection Institute. Which job would you like?

This was flattering; getting two top job offers on the same day is not a regular occurrence. But, which one did I actually want? The Professorship of Radiophysics was more independent and might be more interesting, but I was not sure that I was the appropriate person for it. The directorship of the Radiation Protection Institute involved more work and the aspect of leadership to which I might also not be suited. Yet I had already become acquainted with the international radiation protection work and would have many competent colleagues. I did not hesitate long. 'The Radiation Protection Institute,' was the answer I gave right there and then during that telephone conversation.

It turned out to be a Professorship in any event. Sievert, who was afraid that the government would appoint an administrator with no radiation protection knowledge as head of the new Institute, had advised the Ministry of Health and Social Affairs time after time to make sure that it was someone with some expertise. To guarantee this, he had convinced the Ministry that it would be good if the head of the new Institute could be given the title 'Professor and Manager', which would underline the need for the person to have the qualifications to be a Professor.

It was of course down to Sievert that I got the job. He had been very eager for it to happen, perhaps too eager in my opinion. He was also anxious for me to replace him as Swedish representative of the UN's Scientific Committee, UNSCEAR. In order to show me off to the Ministry for Foreign Affairs, he managed to agree a time for a meeting with Torsten Nilsson, who had been Minister for Foreign Affairs since 1962. We were told to wait in a corridor in the old Parliamentary House. As was so often the case when he had no reason for being so, Sievert was very troubled before the expected meeting. When the Minister for Foreign Affairs finally came and Sievert was to put forward his case, he was nervous and stammered. Torsten Nilsson placed a calming hand on his arm and looked at me. 'Well, I already know Lindell from when we were together in Geneva,' he said and hurried on. So that was that.

On 15–23 November 1965, UNSCEAR held its 15th session in Geneva, the last one attended by Sievert. The delegation had its usual members present with Torbjörn Caspersson as Sievert's substitute and Arne Nelson, K.-G. Lüning and me as advisers. It was obvious that Sievert was saddened by the prospect of losing frequent contact with friends and colleagues in other countries. He valued his friendships very highly.

This time, UNSCEAR's meeting concerned the completion of the report that was to be published in 1966. The report was to be comparatively thin and contained only three scientific Appendices:

A. Radiation from natural sources
B. Environmental contamination
C. Genetic risks of ionising radiation

The Chair of the Committee for this session was the Australian D. J. Stevens with the Indian Dr. A. R. Gopal-Ayengar as Deputy Chair.

The radioactive fallout was still causing problems. At the CEA works in Strängnäs, the only manufacturer of x-ray film in the Nordic countries, head of research Arne Lundh was complaining about disruptive dots on film which were caused by radioactive particles in the protective paper.

Radiation protection optimisation was as yet an unknown concept, but the optimisation principle began to be suggested in other, sometimes unexpected areas. In December 1965, the Swedish Tourist Association's periodical discussed the extent to which costs could reasonably be laid down for the mountain rescue service to save a human life. At a meeting of the Swedish Association of Technologists, the later editor of *Teknisk Tidskrift*, *Bertil Håård* (1921–1999) gave a talk on the importance of giving human life its 'own' value when performing cost-benefit calculations. Håård said that if you only gave life a 'utility value', you would find that the most economical way of building road curves would be to make them lethal and ensure that no-one survived injuries because the healthcare expenses would be greater than the value of human life. It was vital to consider a life to be worth more than its utility value, said Håård, which made an impression on me as the listener.

In 1965, David Sowby's essay *Radiation and Other Risks* was also published in *Health Physics* (Sowby, 1965). It really was a pioneering work which compared different types of risk. The popularity of risk comparisons has since re-emerged, but they have often been misused. It may be interesting to point out that the mortality risk from smoking cigarettes (i.e., the likelihood of a death occurring too early as a consequence of the increase) is, roughly speaking, one in a million for each cigarette smoked, but this says little about the acceptability of other risks. That which we accept or do not accept is actually not a risk of a certain magnitude but the phenomenon that gives rise to the risk. This means that a number of other factors come into the picture, such as the use or enjoyment of this phenomenon, the possibility of influencing it and much more.

In December 1965, the Secretary of the Nordic Society, Stig Johansson, wrote to the Secretary of the IRPA, Bonet-Maury, and submitted an application from the Society to become a member of the IRPA. On 9 December, Sievert wrote to Morgan and complained that the IRPA's statutes and admission rules were unnecessarily finicky and complicated.

At the tribute dinner for Rolf Sievert on 27 October 1965. Elis Berven gives a speech and Professor Nanna Svartz keeps an eye on Sievert while he is listening. Photo: Sven Löfveberg.

The Nordic Society's first big conference was now approaching, which was meant to be held in Stockholm in February 1966. Sievert began the preparations in December of the previous year, when he ordered from the Royal court jeweller H. C. Bolin 5 Chair clubs, one for each of the Nordic countries, silver plate inscribed, for 303 Swedish kronor per club. The inscription read:

AD • PROTECTIONEM • CONTRA • RADIATIONEM
DANIA • FINLANDIA • ISLANDIA • NORVEGIA • SUECIA

And just before Christmas, Sievert asked Minister for Health and Social Affairs *Sven Aspling* (1912–2000) to open the Society's meeting when the time came.

Secretary of the Society Stig David Johansson's days were busy. Stig David was Radiation Protection Inspector at Arne Hedgran's Nuclear Physics Department and was the immediate person responsible for checking radioactive sources of radiation such as level indicators, substance detectors and similar within the industry. He had great pedagogical experience and was very diligent in what he did. He tended towards perfectionism and jealously guarded his territory. You could be certain that what he did would be done well and correctly, but at the same time it was clear that a corresponding level of achievement throughout the Radiation Protection Institute's areas of responsibility would have required more resources than those which were available.

Stig David set about his preparations for the Nordic Society's first big meeting with great zeal and ambition. His interest covered all aspects, not just the scientific programme but also accommodation, food and entertainment. He even tasted the meals that were intended to be served at the dinners!

The meeting took place on 6–9 February 1966 with 150 participants and all five Nordic countries were represented. 35 lecture and 2 panel discussions were held during the Conference. Proceedings from the Conference were compiled by Kurt Lidén and the editor of *Acta radiologica*, Erik Lindgren (Lidén, 1966).

A selection of the most interesting lectures shows which matters were relevant in 1966 and who the primary players were:

Asker Aarkrog from Risø spoke about the levels of strontium-90 in the Danish environment. Olli Castrén about gamma spectrometry measurements of caesium-137 in Finnish milk, Lennart Hannerz talked about caesium-137 in fish and plankton in Mälaren, Thorleif Hvinden about caesium-137 and strontium-90 in air, precipitation, land and agricultural products in Norway, Kurt Lidén and *Monica Gustafsson* reported the levels of caesium-137 in different population groups in Sweden, *Gunnar Lindblom* gave an account of data from fallout measurements in Sweden, Attilio Magi and Gun Astri Swedjemark reported the quantity of caesium-137 in Swedish dairy milk, Anneli Salo talked about strontium-90 and caesium-137 in run-off water and drinking water in Finland, Lars Ehrenberg and *G. Eriksson* spoke about dose dependency of the mutation rate in the rad area in the light of experiments with taller plants, *Gustav Notter* (born 1919) and Rune Walstam spoke about radiation-induced cataracts (grey cataracts) following the radiation treatment of children, *Lennart Devell*, *L. Venner* and Bertil Mandahl in Studsvik spoke about measurements of the internal contamination of personnel, Aulis Isola and *O. Ojala* reported the genetically significant dose from x-ray examinations in Finland in 1963, Knud Kristensen spoke about radiopharmaceuticals, Attilio Magi and I described the Radiation Protection Institute's new laboratory for whole-body measurements, Matti Suomela described the whole-body counter at the Institute of Radiation Protection in Helsinki, and H. L. Gjørup described a food sensor for preparedness purposes.

Four months later the *Fachverband für Strahlenschutz* [Radiation Protection Association] was formed as one common organisation for Germans, Austrians and Swiss. However, German-speaking members of the Health Physics Society had already formed a central European section of the HPS in 1964. This was the section that became a 'Fachverband', or 'Association' in 1966 in order to be able to become a member of the IRPA. The driving forces were Peter Courvoisier from Switzerland and Rupprecht Maushart from Germany.

In June 1966, the IRPA's Interim Board met in Sterling Forest with Merril Eisenbud as host. Sterling Forest is an eighty km^2 recreational area in Bear Mountains approx. 50 km north-west of Manhattan where New York University has research laboratories and a conference facility (Onchiota), which is where we met. Here, a very vigorous Japanese Professor, Yasushi Nishiwaki, was noted in the discussions, the same man who had been active in Japan following the radioactive contamination of the 'Lucky Dragon'.

On 3 September, the Interim Board met for the final time, this time in Rome, and the IRPA's first Congress opened on 5 September, a big event. Dr. P. Caldirola was President and Carlo Polvani Secretary General. However, a very efficient lady, *Dr. Lia Forti,* took charge of the practical Secretariat work. The IRPA could now formally be inaugurated with 15 affiliated societies and a total of 5 000 members from 55 countries. Its first Ordinary Board was set up with the following members:

K. Z. Morgan (USA), Chair; Y. Nishiwaki (Japan), Deputy Chair; W. G. Marley (the United Kingdom), Deputy Chair of Congress affairs; P. Bonet-Maury (France), Secretary; P. Courvoisier (Switzerland), Treasurer; W. S. Snyder (USA), head of publication; D. Beninson (Argentina); L. Bozoky (Hungary); J. R. Horan (USA); B. Lindell (Sweden); A. M. Marko (Canada), and C. Polvani (Italy).

The lectures from the IRPA's first Congress were published in the Proceedings, which were compiled by Walter Snyder into two thick volumes. Many interesting lectures had been collated. One in particular interested me. Dr. *Mogens Faber* from the Finsen Institute gave an account of the result of a follow-up of 1 000 Danish patients who had been given intravenous injections of the x-ray contrast medium Thorotrast from 1936–1945. The lecture was of particular interest because I knew that this examination had taken place but that it had been surrounded by great secrecy. Berven and Sievert had each received their 'confidential' report but the Danes had appealed for secrecy with regard to the patients concerned (see Chapter 2). Faber's conclusion now was that the number of tumours in the group examined did not differ significantly from that which was normally expected but that there were a few extra cases of liver tumours and leukaemia.

In Sweden, Thorotrast had been used primarily at the Seraphim General Hospital for cerebral angiographies, i.e., examinations of the blood vessels in the brain. Professor Erik Lindgren was Chief Physician and head of the x-ray department there from 1949–1970, i.e., after they had stopped using Thorotrast. Lindgren, who was concerned about the consequences of the previous use of Thorotrast, took the initiative of following up the patients who had been examined. As it happened, Lars-Eric Larsson and I, along with a young doctor by the name *Rune Blomberg*, cooperated with Erik Lindgren and began going through the information that existed on how much Thorotrast had been injected and to whom it had been given. The next step was to find out what had happened to these patients. Our primary aim was to see whether they had got cancer from the Thorotrast that had accumulated in the liver.

And this was the context in which *Margareta Rydell* was employed as an assistant. Margareta ended up being my Secretary and had the valuable ability to think independently. She later became Executive Officer within the Radiation Protection Institute's preparedness activity.

As a doctor, Dr Blomberg was the necessary key to the cancer registry. This registry had been established in 1958 as a national registry of all cases of tumour disease and was facilitated by the doctors' duty to report. At the start of the 1960s there were no computers in the modern sense, but there must have been tabulating machines to process information that had been stored on punch cards. The central cancer registry contained tens of thousands of punch cards covering people who had been affected by cancer. If the card were pushed into a mechanical sorting machine with the instruction to sort out the patients who were on our list and at the same time had liver cancer, our task would have been easily solved. However, the friendly ladies who were responsible for the registry became scared. The punch cards were stored in alphabetical order, you see, and to run them through the sorting machine would disrupt that order. The cards were run in the machine on an annual basis to produce the information that was needed for the annual reports, but otherwise not at all. We attempted to get someone to listen to our view that punch cards did not need to be stored in any particular order but were intended to be sorted in a machine – but no such luck. Instead, we received the result of the latest run, an incredibly long 'accordion' printout which we had to read through ourselves in the search for liver tumours.

From 1932–1942, 814 patients had been injected with Thorotrast at the Seraphim General Hospital where examinations of the brain's blood vessels were particularly important, given the General Hospital's important position with brain surgeon *Herbert Olivecrona* (1891–1980) as the impetus behind it. No other x-ray contrast medium was used in these years. Other contrast media were also used from 1943–1947, but it was stated that Thorotrast had been used in 94 cases while there was no information on the contrast medium in 210 cases. So, at least 908 and no more than 1118 patients had been given intravenous Thorotrast injections. Among these patients we found six cases of liver tumours. 35 patients had also been given an unsuccessful injection so that a clump of Thorotrast had remained beneath the skin of the throat which had led to worrying injuries in at least half of the cases. Our observations were published in *Acta radiologica* in July 1963. Erik Lindgren was almost relieved that we had not found more tumours; he had been afraid that the consequences were worse.

With the help of the Radiation Protection Institute's 'whole-body counter' I was later able to measure the gamma radiation from some patients who had Thorotrast in their bodies. Particularly diabolical were a few cases where Thorotrast had accumulated in the palates of people who had been injected with Thorotrast for some indefensible reason. These patients were aware of what had happened. I was more worried about the ethical problem of whether we ought to inform those who we knew had Thorotrast in their bodies but were not aware of it. I did discuss the matter; I do not remember whether it was with the Radiation Protection Committee or its successor, the Radiation Protection Institute's Board, but it was agreed that there would be no point giving information to those affected. There was nothing that could be done to reduce the risk; the information would therefore only lead to unnecessary worry. I wondered whether silence would not take away the patient's opportunity of obtaining compensation. The answer was that the injections had been given *lege artis*, i.e., in accordance with the level of science and tried and tested experience at the time, and that the likelihood of any compensation being awarded was therefore very small. I decided to go along with this; I had great respect for the integrity and judgement of board members such as Yngve Samuelsson and Gösta Dahlberg. I regret not having discussed the matter again later on when full information was given to patients as a matter of course, but the Radiation Protection Institute had no responsibility with regard to the matter; those who were directly responsible for information were the doctors at the Seraphim General Hospital where the examinations had taken place and the overall Medical Board, as well as its successor the National Board of Health and Welfare.

The job as Professor and Manager of the new Radiation Protection Institute was not set up until 1 November. I received a number of congratulatory letters and telegrams, obviously from Sievert but also from Elis Berven, Sigvard Eklund in Vienna, Olof Lagercrantz (who hoped that the appointment would not prevent me from writing for *DN* occasionally), Jan Cederlund from Borås as well as Kurt Lidén (who hoped for close and reliable cooperation). On 2 November I withdrew my applications for the Professorship of Radiophysics at *Karolinska Institutet* and Assistant Professor in Medical Radiophysics in Gothenburg.

When I started as head of the Radiation Protection Institute, my greatest concern was the importuning 'atomic age'. OKG had ordered its first reactor and Waterfall also appeared to want to invest in light water reactors. Soon there would be large nuclear power plants in Sweden and the Radiation Protection Institute would be responsible for the radiation protection requirements. However, at the Radiation Protection Institute there was no-one apart from Arne Hedgran who knew anything about reactors. Before coming to the Institute in 1960, Stig David Johansson had certainly been a close colleague of AKK's MD *Gunnar Lindström* (1918–1990) and had been involved in assessing stations and reactor systems but, due to 'his general unwillingness to compromise on what he thought was right', he was never given the role that Hedgran had initially intended. Abiding by what you think is right is often a good characteristic, but if it concerns a number of day-to-day problems rather than a greater matter of principle, this does not exactly assist the cooperation. Johansson ended up concentrating on other problems rather than that of nuclear power – sources of radiation within the industry.

As for me, I actually had no choice; I was forced to familiarise myself with the problems and give nuclear power radiation protection matters the maximum priority. This meant that I had little time to concentrate on x-ray checks, but this activity had functioned well for more than twenty years so I assumed it should be able to manage without me. This could have been a mistake.

Matts Helde used to tell vivid stories about inspection trips in Värmland and other areas where the roads were steep and slippery. It sometimes appeared as though the adventure aspect and the desire for discovery were the be all and end all of the trips. After a while I looked at their objective. The supervisors were justifiably proud of their inputs throughout the country and thought they were doing useful things when they were able to correct errors, but I thought it was an inefficient way of putting things right. After their trips I thought they should write better reports about the errors they had discovered so that the Radiation Protection Institute could send information on the experiences to all radiologists and thereby making the experiences beneficial to everyone. However, the radiation protection inspectors, who were practical but not particularly fond of writing, thought that writing reports was a waste of their time. They saw demands for this as a bureaucratic decision, particularly if it came from a new manager who had never done any inspection trips himself.

At the ICRP meeting in Fiuggi in September 1965, I had wondered to what extent the exaggerated fear of radiation that was expressed in the 1960s could be explained by the lack of knowledge regarding the nature and effects of ionising radiation (I now know that there were other reasons as well). What did people learn at primary school? I contacted the obliging Ingemar Wizelius at *Dagens Nyheter's* cultural desk and asked whether *DN* would be at all interested in an article on the matter. Wizelius' immediate answer was: 'I will of course be delighted to try and collect all available primary school physics books and send them over upon your return.'

I received a pile of textbooks (seven in all) to read through and was pretty alarmed at what I saw. My reading through them resulted in an article in *DN* on 11 February 1966. I wrote:

> For those who were brought up in times when a printing error in a textbook was a sensation and a factual error blasphemous, there is a worryingly substantial number of actual errors in the new physics books. Some of them can doubtless be put down to pressure but too many can be attributed to lack of knowledge on the part of the authors.

In my opinion it was strange that the school authorities had not invested more in the course literature rather than leaving the initiative to the publishers, all of whom appeared to be intent on producing textbooks. This had unfortunate consequences:

> The Swedish textbook market may not be particularly small, but each portion could have been a significantly larger piece of the pie had it not been shared among so many. While the publishers are anxious to publish a textbook, they are unwilling to take any major risks. The typical example is that a younger teacher writes a text for a small fee which the publishing company makes competitive by also paying an older teacher whose name is known to review the book. In the most fortunate cases, the older teacher spends a lot of time on the review but, if the results are anything to go by, this unfortunately does not happen nearly often enough.

In many of the books I found what I referred to in the article as 'silly, jaunty jargon'. I continued:

> You cannot help but get the impression that the subject matter is changing here. For example, you no longer find physics in the shape of natural science in many of the physics textbooks; instead, you find that the authors are writing *about* physics rather than as though they are *involved in* physics. At best, it all turns into cultural history with a new forum for much criticised 'infantile' language and lecturing. Terms and phenomena are lined up in what must seem to the pupils like an infinite quantity with no obviously logical connection. It often seems as though the aim is to teach as little as possible about as much as possible.

Following this outburst I looked in particular at what had been said about radiation and radioactive substances. I found that several authors had been tempted to describe radioactivity and ionising radiation as something that was mysterious and strange. Phrases such as 'x rays are dangerous to living beings' and 'gamma rays are dangerous to all life' were not accompanied by any further explanations or mention of radiation as a life-saving tool in medicine. I was able to list countless factual errors.

One of the authors of the textbooks very much resented this. It was a 75 year-old retired lecturer who sent me a cutting of a complimentary review of the book that named him as co-author.

In January 1966, I, Kay Edvarson and Rolf Sievert gave expert statements on who was the most suitable person to succeed me as premier physicist at the Radiation Protection Institute's specialist laboratories. There were only two applications for the job: Stig David Johansson and Jan Olof Snihs. We said that both were sufficiently competent for the job but thought that Snihs was the most suitable, which was later verified by his solid inputs.

On 21 January, a meeting of the subject representatives of radiophysics was held in the Radiation Protection Institute's assembly hall. The meeting was in preparation for a conference arranged by the University Chancellor's Office to be held on 10 March. At the conference it was determined that a an institute of radiophysics ought to be an independent institute and that the subject of radiophysics ought to come under the medical faculty, where it should include the radiological institution group, and that the subject should also have a seat and vote in the mathematics and natural science faculty and be able to belong to an institution group there. During the discussion it emerged that the conditions could be very different in different places. Börje Larsson pointed out that the Gustaf Werner Institute in Uppsala differed from other university institutions through its unique research options. Sievert pointed out that the Institute of Radiophysics in Stockholm was in a unique position through its connection with the Radiation Protection Institute. Börje Larsson thought that standardising the subject at the different university sites would not necessarily be the correct solution. People ought to be able to utilise the different conditions at the seats of learning to that provide for different needs within the field of radiophysics.

On 25–29 April, the 1966 Academy of Sciences' National Committee for Radiation Protection Research arranged a big symposium in Stockholm concerning 'radioecological concentration processes'. A great deal of trouble was taken with the arrangements. An Organisation Committee had been set up with Sievert as Chair and Kurt Lidén as Deputy Chair and with me and Arne Nelson as Secretaries. The Committee also included Lars Carlbom, Lars Fredriksson, Lennart Hannerz and Bertil Åberg. We had strict requirements regarding the readability of the contributions, and Sven Löfveberg (who helped me with the practical arrangements) and I had considerable clashes with the authors to make the contributions acceptable. Bertil Åberg, who had undertaken along with *Frank Hungate* at the Northwest Laboratory in Richland operated by Battelle to complete Proceedings, had stated the requirement that the manuscript should be ready for printing by the end of the symposium. They succeeded in getting Pergamon Press to publish a neat, thousand-page book as early as the following year entitled *Radioecological Concentration Processes*.

The symposium was a big success. This was the first time a number of Soviet scientists, all of eighteen people, within the field of radiation protection were allowed to participate freely in an international symposium. Bureaucracy was not without its problems. The Soviet scientists who came were not exactly those who had registered, and nor was their arrival always at the expected time, by the expected mode of transport or to the expected place. We were not able to say exactly how free they were, and we suspected that some were political bureaucrats. This suspicion was reinforced by one interesting episode. When on the first day Sven Löfveberg came down into the foyer, four Russians were seated in front of the reception looking lost. 'Don't you have anywhere to go? wondered Sven, speaking to them in English. 'Come with me, I have a car outside! I can show you parts of Stockholm that you would never see on an ordinary sightseeing tour.' One of the group spoke English and explained to the others what Sven had said. And so he took them on a long, worthwhile trip around the city. However, when they returned, a Russian lady was standing in the foyer crying miserably. She had thought that the four had defected and was probably afraid of being punished for inadequate supervision.

The Russian lecturers were *R. M. Alexakhin, V. L. Anandin, V. A. Knizhnikov, V. V. Kovalsky, N.T. Kwaratskhelia, A. N. Marey, V. I. Maslov, M. K. Melnikova, A. A. Moiseev, M. A. Nevstrueva, F. I. Pavlotskaya, G. G. Polikarpov, Yu. A. Polyakov, N. A. Timofeeva, E. B. Tyuryukanova* and *I. N. Verkhovskaja*.* Of these, Polikarpov was the best known at the time. Alexakhin and Moiseev would later become part of the ICRP Committees and Alexakhin became a member of the Main Commission in 2001.

The National Committee had promised to reimburse the Russians' travel expenses. Polikarpov asked that the monies be deposited to be used to purchase books from *Nordiska bokhandeln* (the Nordic Bookstore), a service which we were able to provide a good while later.

Several of the Soviet contributions discussed the uptake of strontium-90 and caesium-137. We did not understand how the concentrations could be so high. The Kyshtym disaster of 1957 was still being kept secret from outsiders.

* I list the names in the way they had been registered for the symposium, which in the majority of cases meant English transcription.

In April 1966, Rune Walstam was appointed as Professor of Radiophysics after Sievert, thereby also becoming the first supervisor of medical physics which, until now, far-fetched as it might seem but for practical reasons, had been administered by the new Radiation Protection Institute.

In summer 1966, Sievert lost three of the people with whom he had enjoyed close cooperation. On 28 May, instrument maker Ragnar Scheer had an unfortunate fall down the stairs and died. Scheer had meant a great deal to Sievert when it came to realising the different types of ion chamber that he had designed in his early career. On 15 June, Elis Berven died following an agonising final few years, possibly a victim of the radiation that he had used so ingeniously to save the lives of others. George de Hevesy died on 5 July.

In June, the 27th Congress of the Nordic Society of Medical radiology was held in Oslo under the Chairmanship of Professor Erik Poppe. The lectures were divided into three sections: diagnostics (55 lectures), therapy (28) and physics (7).

In the physics section, *Kenneth Magnusson* from the Radiation Protection Institute talked about patient and personnel doses during x-ray work using image intensifiers, and Jan Cederlund gave an interesting presentation on information capacity in an x-ray beam. Within the therapy section the Swedish radiobiologist László Révész spoke about radiation biology principles which he expected to be able to lead to an important development in radiation therapy. Rolf Wideröe reported opinions on the irradiation of tumours with electrons, and Erik Poppe presented treatment results for 468 patients with bladder tumours who had been irradiated using the Brown Boveri betatron in Oslo.

On 6–17 June 1966, UNSCEAR held its 16th session in New York. This time, Sievert was no longer involved and I had succeeded him as the Swedish representative. The meeting was an ‘interim meeting’ and in the Swedish delegation, the only adviser I had was geneticist K-G. Lüning. The task in hand was that of finally approving the 1966 annual report where one of the three Appendices dealt with genetic* risks. The rest of the meeting was devoted to preparatory discussions about the continued work. The Chair of the Committee at this session was the Indian Dr. A. R. Gopal-Ayengar with Canadian Dr. Gordon Butler as Deputy Chair.

After UNSCEAR’s meeting, I travelled with the family by car to the northern part of New York State to visit John Hursh in Rochester. John was to spend a sabbatical year in Europe, half of which time was spent at the Radiation Protection Institute, and we discussed the strategy of the visit. He was to do research with Jan Olof Snihs at the specialist laboratories and would be assisted by Jorma Suomela. The research concerned the uptake of lead into the body when it entered the stomach and intestines.

We returned to Sweden on *M/S Gripsholm*. Flying was not yet an automatic option as it is today.

John Hursh and his wife came to Stockholm from Rome in the late summer and it turned out that she had bought a Porsche *en route*. I had arranged a room with cooking facilities at the Wenner-Gren Centre within a short walking distance of the Radiation Protection Institute. John’s cooperation with Jan Olof and Jorma proved to be very fruitful for both parties.

On 1 October, Gustav Weber stepped down as head of Elema-Schönander but remained within the company as Chair of the board.

During autumn 1966, the possibility of making uranium production in Ranstad more economical by producing building material from the tailings was discussed. However, Professor Jan Rydberg pointed out that all the radium is left in the tailings and that radium levels of 75 microgrammes per tonne of concrete could therefore be expected, i.e., close to ten times as much as in concrete made of cement and sand. Rydberg is understood to have said:

> The risk is that the people who live in these homes will receive ten times as high a mutation frequency. That is not to say that genetic injuries occur. The risk of a certain individual suffering genetic injuries is quite small, but when it comes to large numbers of people, a proportionately high number the genetic injuries may also occur.

* It is now preferable to say hereditary risks because genetic risks in a wider sense also cover risks of impact on the reproduction mechanism of somatic cells.

On 11 October 1966, the Swedish Radiobiological Society held its annual meeting at the FOA's premises in Ursvik. Because I drove a car, Arne Forssberg asked me to take along a young doctor who was doing research with him. It was the first opportunity I had had to form a closer acquaintanceship with *Bernhard Tribukait* (1928–), who went on to become the Radiation Protection Institute's radiation protection medic and later to succeed Forssberg as Professor of Radiobiology.

On 21 October I wrote to all the country's active medical physicists and appended a questionnaire on the medical physicists' opinion regarding a number of matters concerning the Radiation Protection Institute's involvement in measurements of x-ray therapy devices, including telegraph device and accelerators. The intention was to find out whether such measurements could be delegated to medical physicists, which, if the answers are anything to go by, they considered to be reasonable and desirable. This would afford the Institute's standard laboratory considerable work relief.

On 2 November, the Radiation Protection Institute sent the press an invitation to information days. The reason was that we were concerned about any misunderstandings and all the lack of knowledge that was displayed in the newspapers. Unfortunately, there was also lack of knowledge amongst ourselves. It turned out that those who came to the information days were science journalists who were interested and not that lacking in knowledge. However, those who were responsible for the mistakes in the daily press were news journalists and the people who set the headlines. For them, the radiation issues were a very small part of the overall amount of things they had to write about and they had neither the time for nor the interest in the information offered.

On 15 November 1966, Sievert held a lecture at the Joint Occupational Safety Council (a joint work environment body for the Swedish Employers' Confederation and the Swedish Trade Union Confederation); it would be the last one he would hold. A couple of weeks later he would have an operation for an intestinal tumour.

At the end of November I received the following letter from Dale Trout:

> A couple of weeks ago I was in San Francisco visiting Dr. Robert Stone whom I suspect is at death's door at the university hospital. A few weeks ago he had a parotid gland tumour which was treated but his condition has deteriorated rapidly. When I visited him was he lucid, which I understand is not always the case. I have told Laurie Taylor but thought you might not have heard about it.
>
> I was sad to hear about Dr. Berven's death. He has been a good friend for many years and I will miss him. One of my big privileges in life is to have known Dr. Forssell, Dr. Berven and Professor Sievert. They were dedicated men and I am sure that the whole world is aware of it.

A pioneering generation was on the verge of disappearing.

I November 1966, *Lars Persson* (1936–), who was head of division at the National Council for Nuclear Research at the time but who would later come to the Radiation Protection Institute, sent out an invitation to a physicists' conference to be held by the Nuclear Research Council and the Natural Science Research Council (NFR) in the Wenner-Gren Centre on 24 November. The conference was valuable and attracted many participants. The way in which physics was organised in Sweden was discussed with Dr. Funke as the opening speaker. Hugo Atterling opened a discussion on Swedish accelerators. Professor *Gösta Ekspong* (1922–2017) opened up the discussion on CERN's 300 GeV (gigaelectronvolt) accelerator project. The summarising discussion, led by the FOA's Director General *Martin Fehrm* (1910–2001), concerned prioritisation matters. Many physicists were worried that very large amounts had been invested in large accelerator projects. Fehrm was also Chair of the NFR at this time.

At the end of November it was time to appoint a head of the Radiation Protection Institute's Department for Radiation Protection Medicine. The only person to apply for the job was Dr. of Medicine Bernhard Tribukait. Tribukait, who was born in Dresden in 1928, was already a registered German doctor by 1954 and had defended his doctoral thesis at the University of Göttingen in 1956. When he came to Sweden in the same year, he was able to concentrate on research to defend another thesis in 1963, this time for a Swedish doctorate. The specialist whom the Radiation Protection Institute appointed to examine Tribukait's qualifications, Dr. Bertil Swedin, found that 'his competence and skills for the post applied for as head of the Department for Radiation Protection Medicine were evident'.

On 1–2 December 1966, Lars-Eric Larsson and I visited Kristian Koren in Oslo to discuss problems with industrial radiography.

The weather was depressing with snow and slush. I was tired on arriving home from Oslo on the Friday evening of 2 December. Marrit did not want to burden with sad news in the evening, so I was still unaware that a man who was our best friend had suddenly died of a heart attack; she waited until the next morning to tell me. His death hit me hard and I rang *Karolinska Sjukhuset* in the morning in a depressed state to hear what had happened to Rolf Sievert. However, the doctor I spoke to said that the operation had been successful and that Rolf was well.

> In the afternoon, Astrid Sievert rang.
>
> 'Rolf is dead,' she said briefly.
>
> I was aghast. 'But that's impossible! I've just recently spoken to his doctor who said he was well!..?'
>
> 'Rolf is dead,' repeated Astrid, impatiently and irritated. 'I should know. He had a clot in his lungs after the operation. I hope you tell his friends and colleagues, Bo. You must arrange a commemoration of course.'
>
> Factual and efficient.

The practical arrangements softened the painful blow of receiving the news of two sudden deaths on one and the same day. A number of colleagues had to be informed without delay: telegrams to Eker and Koren in Norway, Juel Henningsen and Grande in Denmark, Mustakallio and Salimäki in Finland, Pochin and Sowby in England, Bugnard in France, Jaeger in Germany, Taylor in the USA … the Academy of Sciences. Time and place for a commemoration. Obituaries in the daily press and the periodicals.

Kristian Koren (who was Chair of the Nordic Society for Radiation Protection at the time) rang at 9.35 on the Monday morning. He was on the way to England for a meeting with John Dunster but had spoken to Eker. Koren asked me to arrange a wreath from the Nordic Society.

An hour later, Pochin rang wanting further information on burial and the commemoration. He wanted to come to Stockholm.

At 13.30, Per Grande rang bringing salutations from Juel Henningsen. I was now able to say that the burial would take place on Tuesday 13 December and that a commemoration would be held at *Radiumhemmet* prior to this. The main purpose of the commemoration was to save Astrid Sievert from having to bother with official duties at the time of the burial.

At 14.30, *Gösta Larsson* (1905–) - editor of *Statskalendern* (the Swedish 'Official Directory' of organisations and employees) and Secretary of the Board of the Academy's research stations in Upper Norrland - rang from the Royal Academy of Sciences. He promised to inform the Academy of the commemoration and also to ring Bengt Hultqvist in Kiruna.

A week later, I picked up Bill Pochin from Arlanda and drove him to his hotel. It was a dark, gloomy, slushy day and visibility was very poor, and it was undeniably a great mark of respect that Pochin had taken the trouble to undertake the uncomfortable journey to Stockholm in honour of Sievert.

The commemoration was held in *Radiumhemmet's* big entrance hall and was opened with Vivaldi's Largo Allegro of Cello Sonata in E minor. This was followed by a number of short memorial addresses. My own was concluded with:

> There is no need for this commemoration to be framed into a testimonial to the importance of Rolf Sievert's life work. Let us instead literally just remember him. Let us not forget the man behind his accomplishment. His accomplishment lives on in scientific essays, in buildings and laboratories, in organisations and his dealings. It lives on with us and around us, manifest and efficient, and will always influence us. But the man behind the accomplishment has gone.
>
> Let us not allow ensuing sorrow to prevent us from keeping the memory of him alive. Let us instead remember Rolf Sievert as the man he was when he was bubbling over with life, when he used his enormous energy and appetite for life to swoop on new information, full of suggestions and initiative, cigar in hand and glint in eye with his warm, generous heart still beating for all his friends.

An oration was held at the Academy of Sciences by the Academy's President, Professor of Mathematics *Otto Frostman* (1907–1977). His closing remarks were as follows:

> Sievert was an affectionate and emotional man, something of an enthusiast who spread friendship around him in the circles in which he wanted to work. The jovial and conciliatory surface concealed a substantial firmness of character and precision of work. A talented musician, he was heavily captivated by the master of precision Johann Sebastian Bach, and could spend many happy hours at his beloved Tvartorp at Rejmyre in Kolmården interpreting him on his organ. The Academy's loss is emphasised by the fact that Sievert was willing to work in its best interests until the very end: not only was he Chair of the Board for Upper Norrland's research stations and a member of the Executive Committee, but also Chair of the Committee that the Executive Committee set up at his initiative to review the Academy's activity, undertakings and obligations.

Some of the dedications had arrived while Sievert was able to appreciate them. The documents from the first meeting of the Nordic Society for Radiation Protection were published in 1966 in a supplement to *Acta radiologica*, dedicated to Sievert and with an appreciative prologue by his five Nordic colleagues Eker, Juel Henningsen, Lindell, Mustakallio and Petersen. The supplement was edited by Kurt Lidén and Erik Lindgren. The well-known publication *Health Physics* had also dedicated an edition to him in the same year.

In Budapest, the little Hungarian László Bozoki wrote an affectionate dedication in *Fizikai Szemle*. He concluded by saying:

> The results of Professor Sievert's activity have been well-known in Hungary for a very long time and have been very much accredited. The physics laboratory at the Eötvös Lóránd Radium and X-ray Institute, opened in 1936, adopted his – what was at this time the most up-to-date – condenser chamber method before many other countries did, and through this method received internationally-recognised results and further developed the method in the 1950s. Sievert's influence is noted in various areas of Hungarian research into radiation protection, such as in the development of cobalt-60 units with full radiation protection, further measurements in different types of living accommodation, whole-body measurements, etc.
>
> Sievert's name has always been highly esteemed in Hungary and he has always been considered the most prominent figure and great master in medical radiation physics; his institution and life were and will remain our role model in the future.
>
> His sudden death was a great shock, particularly to those who had been privileged enough to know this huge personality, his characteristic temperament and must-have cigar - but he was primarily characterised by incomparable philanthropy beyond all national borders.

I personally wrote a eulogy in *Health Physics*, which ended with:

> Rolf Sievert was a very generous man, not only when he was sparing no efforts to entertain his guests, but also in tolerance and understanding. His behaviour was colored by his impulsiveness. He was anxious to help and encourage not only his closest friends but any individual, with no discrimination, who happened to be embraced by his wide sphere of activity and interest. His modesty with regard to his own capacity and learnedness sometimes misled people to underestimate his skill and knowledge, and his special liking for testing theories by provoking opposition to opinions which he did not really share himself frequently confused casual listeners. At these occasions his lips carried a faint smile that only his nearest friends learned to detect and interpret.
>
> In his latest years Rolf Sievert often said that his long professional life had taught him that out of his experience of scientific research, successful ideas, administrative efforts, international meetings and hard work, only one precious thing had crystallized – friendship. He valued his ties of friendship across so many borders more dearly than anything else. To the many friends with whom these ties are now broken it is a privilege to have known and hence to be able to remember this great man.

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CHAPTER 14. NATIONALLY AND INTERNATIONALLY
Bain 1997, Boag 1960, Both 1988, Brun 1996, Duns 2000, FAO 1962, Fisc 1997, Gold 1997, Hine 1962, Jay 1954, John 1964, Kath 1974, Kuro 1989, Lind 1961a, Lind 1961b, Lind 1999, Lind 2000a, Love 1995, Mall 1994, Marc 1997, Maus 1991, Mill 1956, Niel 1998, Njøl 1999, Petr 1996, Polv 1984, Rand 1975, Sowb 2001, Sowb 2002, Tayl 1979, UKAE 1977, UKMe 1956, Unne 1984, UNSC 1962, Wake 2000.

CHAPTER 15. SALTHOLM AND NORDIC COOPERATION
ICRP 1955, Lind 1972, Lind 2000a, Niel 1998, Unne 1984, UNSC 1958.

CHAPTER 16. NOVAYA ZEMLYA AND CONTINUED COOPERATION
FAO 1962, Lind 1972, Lind 2000a, Medi 1966, Stra 1962, Stra 1963, UNSC2000.

CHAPTER 17. RADIOECOLOGY, RADIOLOGY, AND RADIOLOGICAL PROTECTION
Cign 1996, From 199 5, Gyll 2000, Lind 1972, Lind 2000a, Siev 1975, Sowb 2001, Unne 1984, USCo 1957.

CHAPTER 18. INNOVATION WITHIN ICRP

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Professor Rolf Sievert (1896-1966) was a pioneer in medical physics, radiological protection, and radiation research. He was a founder member of ICRP, the International Commission on Radiological Protection, and participated in the creation of UNSCEAR, the United Nations Scientific Committee on the Effects of Atomic Radiation. During his last years, he contributed to an effective Nordic collaboration on radiological protection issues. He is one of the few Swedish scientists who have been honoured by having a physical unit named after them. The *sievert* (Sv) is the unit for radiation dose (weighted for radiation type and tissue sensitivity).

The title, The *Labours of Hercules*, refers to Sievert's achievements. This book is a sequel to the two earlier volumes of Bo Lindell's history of radiation, radioactivity, and radiological protection, *Pandora's Box* and *The Sword of Damocles*. It is aimed at persons with a general interest in radiation and requires no previous knowledge. The book deals with the years 1950-1966, which was Sievert's most intense period of activity. This period saw the early development of nuclear power in many countries, and discussions about nuclear weapons proliferation.

The early 1950s was also when the shape and function of the DNA molecule were clarified, constituting one of the most significant scientific discoveries of the century. Radiation therapy against cancer improved thanks to new, effective appliances such as 'cobalt cannons' and accelerators.

In the Pacific, the United States tested 'superbombs' causing global radioactive fallout. In the Urals, where the Soviet Union produced plutonium for their nuclear weapons, great releases of radioactive substances caused extensive damage. Radioactive fallout from nuclear weapons testing caused much anxiety and was a significant factor behind more profound collaboration between neighbouring non-nuclear countries, such as the Nordic countries.

Bo Lindell's personal narrative provides an eminently readable account for all of these events, and for the activities within ICRP and UNSCEAR where he was initially an active participant under Sievert's guidance, and later on became one of the leaders of these organisations.

Professor Bo Lindell (1922-2016) had a degree in engineering physics and a PhD in radiation physics. Having worked closely with the radiation-protection pioneer Rolf Sievert, he took over as Director of the Swedish Radiation Protection Institute in 1965. He retired from that position in 1982 but remained an emeritus adviser until 2008. Lindell was Scientific Secretary and then Chairman of ICRP and the Swedish delegate to, and for a time Chairman of, UNSCEAR.

Lindell wrote this book series, his magnum opus, in Swedish. Aided by generous grants, the Nordic Society for Radiation Protection (NSFS) proudly presents this translation into English.