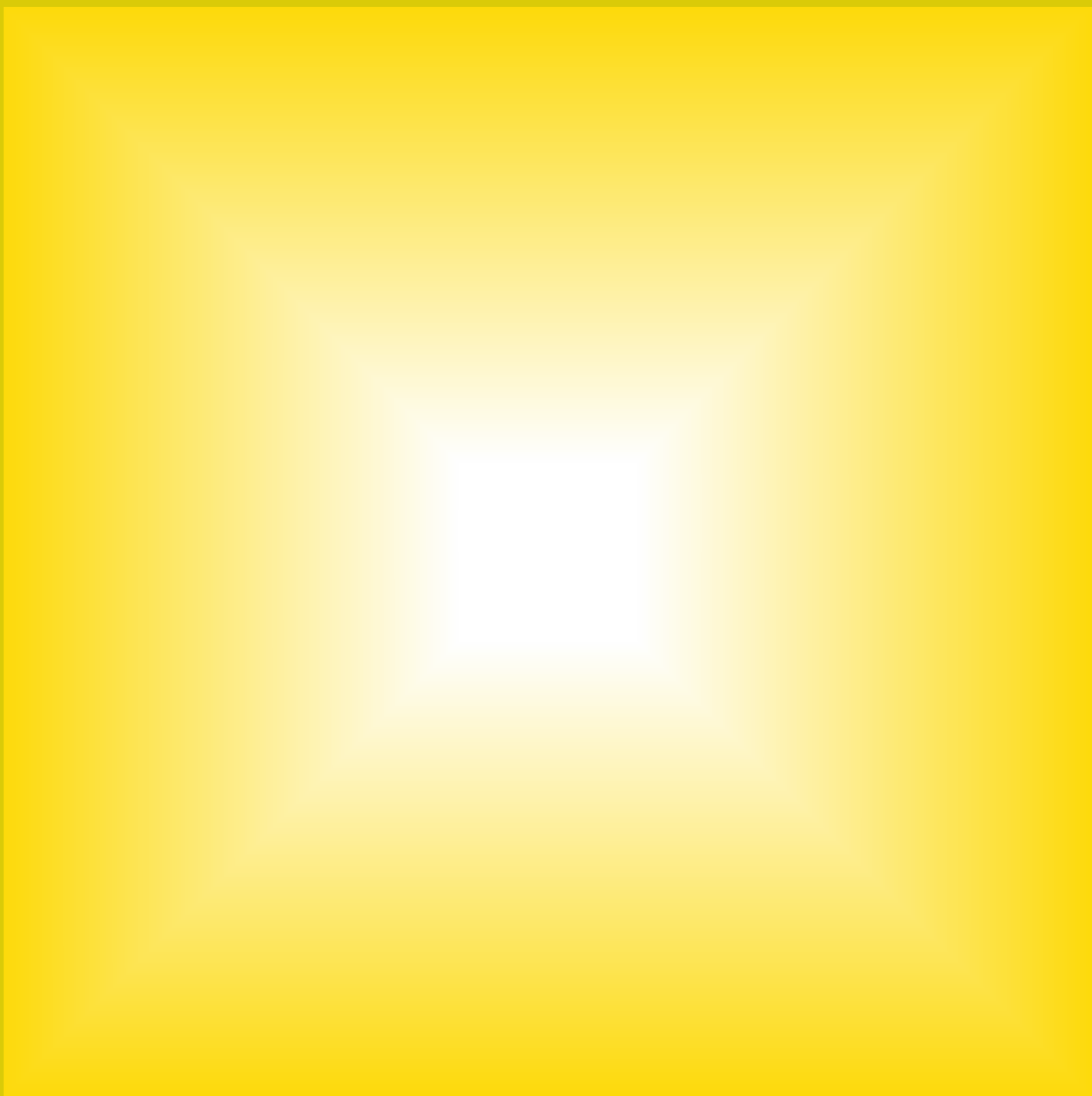
Bo Lindell

The history of radiation, radioactivity, and radiological protection

PART 1. THE TIME BEFORE WORLD WAR II

PANDORA'S BOX



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Pandora's Box

The History of Radiation, Radioactivity, and Radiological Protection

PART I. THE TIME BEFORE WORLD WAR II

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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The translation was ably and enthusiastically performed by Helen Johnson and efficiently and smoothly administered through Snabböversättare Sverige AB.

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And above all, the NSFS of course wishes to thank Bo Lindell himself. His continuous, unswerving, and enthusiastic support for the project until his passing in 2016 was a great inspiration

FOREWORD TO THE ENGLISH EDITION

This is the first book in a series of four volumes, describing how man discovered radiation and radioactive substances, how we learned to use this knowledge for good and evil purposes, and how in due course we also learned how to protect ourselves against radiation risks. This first part leads up to just before World War II, when it was found that some heavy atomic nuclei could be split in a process releasing very large amounts of radiation. The second book describes the rapid developments during the 1940s and the advent and use of atomic bombs. The third book deals with the period from around 1950 to 1966 and Rolf Sievert's most important achievements, and the establishment of civil nuclear energy production. Finally, the fourth volume concerns the time from 1967 until around 2008, a period of considerable expansion of radiation science in many more countries than before.

In parallel with the narrative concerning technical developments and the acquisition of new knowledge, these books describe the evolution of radiological protection activities, both internationally through vast co-operation and within Sweden, thanks to pioneering contributions by Rolf Sievert and his department.

The book series is not a scientific textbook, nor strictly a history of technology. My intention has been to tell readers about the picture of developments that I have personally obtained through many years of work in the area, rather than to *create* a picture through research and source critical work. I have not had time for such things. Instead, I have used reasonably accessible reference literature in order to provide substance around things with which I am broadly familiar.

While the primary aim is to tell a story, the fascinating story about radiation, I also wanted to make the books useful as refence sources that the reader may wish to return to. Therefore, I have included some more detailed information than what is strictly required for the story as such. In order to facilitate such look-up use, the original Swedish version of the books had extensive subject and name indexes, laboriously compiled with the assistance of my Swedish publisher. However, the availability and accessibility of computerised search functions requiring no specialist knowledge is now such that such indexes are no longer necessary, since electronic versions of the English editions are being made available cost-free.

In some chapters, the details include some mathematical expressions. Readers should not let themselves be intimidated by these expressions. They do not constitute any advanced mathematics. While they are not essential in order to keep up with the general narrative, I would advise readers to try to follow the mathematical reasoning – this might lead to some aha experiences.

So as not to make the reading too dry, I have included some light anecdotal material. Personally, I am convinced that it is necessary to get an insight into the life and conditions of the principal characters in order really to understand the development.

The books were written in Swedish – this seemed self-evident to me, since I am a Swede and had Swedish readers in mind. Readers from other countries may feel that in these books there is a preponderance of persons and events from the Nordic countries. This is true, and in part reflects my own contacts with many of these persons, but it is also a fact that much of the early development took place in the Nordic countries and in particular through Rolf Sievert and the people around him.

Relatively early on, some people asked whether I would consider writing in English instead in order to reach a wider audience. That did not seem right at the time, since I was writing primarily for a Swedish audience. I wanted the books to be accessible for lay persons who might be deterred if the books were not just thick and on a subject matter perceived by many as 'difficult', but on top of that indited in another language.

At the same time I am of course keen for my books to be widely read, and admittedly the many requests for an English version are very flattering.

Therefore, I am extremely pleased and grateful that the Nordic Society For Radiation Protection has initiated and carried out the huge project of translating all of the four books into English. The project was possible only thanks to the very generous support from the Society, from its international parent organisation IRPA, from all of the Nordic regulatory authorities for radiological protection, and from the Nordic nuclear safety research organisation. I am deeply grateful to these organisations for their support. The excellent translation by Helen Johnson, and the conscientious fact-checking by the editorial project group commissioned by the Nordic Society for Radiation Protection, complete the picture of a successful project. My heartfelt thanks go to all involved in the project.

Finally, let me repeat here some words of thanks from the Swedish edition of *Pandora's Box.* There, I mentioned how grateful I am for the assistance provided by many colleagues at the then Swedish Radiation Protection Institute/Authority (in particular Ulf Bäverstam and Jack Valentin) and from the directors of the Institute (Gunnar Bengtsson and Lars-Erik Holm), and from the late Professor Rune Walstam. These friends of mine have saved me from some of the worst faux pas, and the Institute also kindly supported the publishing of the Swedish edition. My Swedish publisher, Atlantis, and their editor Björn Wahlberg contributed many improvements. And last but not least I gratefully appreciated the incredible patience of my wife Marrit (now regrettably deceased) during my work with this book.

Sollentuna, February 2015

Bo Lindell

1. THE MYSTERIOUS ISLAND

> 'Are we rising again?'
> 'No. On the contrary.'
> 'Are we descending?'
> 'Worse than that, Captain! We are falling!'
> 'For Heaven's sake heave out the ballast!'
> 'There! the last sack is empty!'
> 'Does the balloon rise?'
> 'No!'
> 'I hear a noise like the dashing of waves. The sea is below the car! It cannot be more than 500 feet from us!'
> 'Overboard with every weight! ... Everything!'
> Such were the loud and startling words which resounded through the air, above the vast watery desert of the Pacific, about four o'clock in the evening of the 23rd of March 1865.[1]

These were also the words that were used by *Jules Verne* (1828-1905) in the introduction to his novel *The Mysterious Island*, which was indisputably an adventure novel, but also an account of man's knowledge of natural science into the bargain. These were times of precision. Nothing was impossible for the scientist (within the boundaries of natural law, that is), and the laws of nature were now seen as definitive. Law and order most definitely had the upper hand where natural science was concerned; but then exactitude does have its own degree of romanticism. The wording of the quoted introduction continues as follows:

> Few can possibly have forgotten the terrible storm from the northeast, in the middle of the equinox of that year. The tempest raged without intermission from the 18th to the 26th of March. Its ravages were terrible in America, Europe, and Asia, covering a distance of eighteen hundred miles, and extending obliquely to the equator from the thirty-fifth north parallel to the fortieth south parallel. Towns were overthrown, forests uprooted, coasts devastated by the mountains of water which were precipitated on them, vessels cast on the shore, which the published accounts numbered by hundreds, whole districts leveled by waterspouts which destroyed everything they passed over, several thousand people crushed on land or drowned at sea; such were the traces of its fury, left by this devastating tempest. It surpassed in disasters those which so frightfully ravaged Havana and Guadalupe, one on the 25th of October 1810, the other on the 26th of July 1825.

Exactitude somehow has the power to reduce these horrors to the howl of a storm in the dark of night beyond the realms of your secure, cosy room where, in front of a log fire flickering away in the open stove, you can have the satisfaction of shuddering when faced with the most shocking descriptions - provided they turned out well in the end.

This was the sign of the times. Mankind had yet to master nature - the fury of the elements could be appalling, storms wreaked havoc, ships foundered, earthquakes brought down that which had been erected by the human hand, and infectious diseases caused an incalculable number of deaths. However, as it happens, mankind had just finalised the key that would open the door to an anticipated future in

[1] Lindell used a translation of the French original into Swedish from 1904. Excerpts quoted here are from the translation into English by Agnes Kinloch Kingston, 1875.

which nature would become a willing slave. They thought they had found the Holy Grail which constituted a definitive understanding of the forces of nature; they thought they knew the laws of nature and that they could use them to their own advantage: mankind now held a key to knowledge in its hand.

There was no doubt that the future looked bright. The Earth had existed for billions of years and mankind for millions of years. Development had stood still for millennia – apart from a few occasional inventions such as the wheel and the art of creating fire, that is - but now, all of a sudden - over the past few centuries, over just a few generations - the secrets behind the laws that controlled nature had been surmounted.

Well, perhaps not everything had stood still thus far. There was of course a cultural heritage of increasing knowledge within mathematics and astronomy - but *what* a speed for development to have rushed towards the finishing line, faster and faster as mankind drew closer! Not only that, mankind had now almost reached the finishing line. What incredible luck to have been born during a time of so many revelations!

The development of natural science had in fact formed the basis for the accomplished art of engineering. It was only logical that the leader of the men whom Jules Verne saw fit to leave foundering on the mysterious island when their air balloon lost its buoyancy was an engineer. He can be seen as the symbol of specialist science, and his friends idolised him. He was clearly the one who had all the answers and it was equally obvious that there was an answer to all conceivable questions.

> 'Is not our engineer alive? He will soon find some way of making fire for us!'
> 'With what?'
> 'With nothing.'
> What had Pencroft to say? He could say nothing, for, in the bottom of his heart he shared the confidence which his companions had in Cyrus Harding. The engineer was to them a microcosm, a compound of every science, a possessor of all human knowledge. It was better to be with Cyrus in a desert island, than without him in the most flourishing town in the United States. With him they could want nothing; with him they would never despair. If these brave men had been told that a volcanic eruption would destroy the land, that this land would be engulfed in the depths of the Pacific, they would have imperturbably replied: 'Cyrus is here!'

Nature's resources, which were now at the disposal of mankind as never before, still seemed to be inexhaustible. The relevance of or necessity for environmental conservation was such you could have let a small child roam free in a forest full of berries with little concern. Nature groaned beneath the weight of berries and fruits - both metaphorically and literally speaking - and all mankind needed to do was pick them.

Most natural phenomena have an intrinsic resilience and tolerance. If I pull a tree branch it gives way, and if I pull harder it gives even more. When I release the branch it assumes its original position. The overall damage is proportional to the impact, and there is no long-term damage provided the impact is minor. If on the other hand I pull the tree branch very hard, it breaks.

Provided the human impact on nature was moderate, the damage was also moderate and reversible. All natural life consists of a chaos of impacts and mankind has come to be seen as part of nature in terms of both having an impact and being impacted on. Experience has shown that nature was able to tolerate what mankind was doing. You could smile indulgently at those who were concerned about any possible intolerance. Nobody doubted experience. Any reaction was similar to that of the old man who indignantly grumbled: 'Behave, you rascal you! Why shouldn't you have the suit? It was good enough for my grandfather and my father and for me so why shouldn't it be good enough for you as well?'

I've experienced a similar comment myself. In 1951, I took a trip up a Swiss mountain by mountain rail whose design looked more as though it belonged in a technical museum than in full service as a means of transport. The line rose steeply and I was alone with the driver in the rail car. Pacified by the deluding thought that there was no particular likelihood of an accident happening on the particular occasion that I happened to be on it, I wasn't actually seriously thinking about any risks but, since I needed a topic of conversation and safety was an issue that could be of imminent relevance, I asked the driver if he was ever afraid that the car would topple. His response was a pitying look and:

'Since you ask, this car's been running every single day since 1883 without anything happening, so it's quite obviously safe!'

In spite of good previous experiences, neither nature nor technology should be pushed too far. However, Cyrus Smith and his friends on the mysterious island, like their social contemporaries in the 1800s, were not in a situation where it was easy for them to push nature too far. There were of course some exceptions on the mainland, such as the destruction of forest land and the development of cities with poor hygiene which replaced the order of nature with loess, rats and epidemics. But, in principle, mankind was merely scratching the surface of nature's abundance. The damage that was caused increased gradually in proportion to the impact. Nature was nature. Not in stationary balance - that had never been the case - but still as good as unaffected by the activities of humankind.

On Lincoln Island, the name that the stranded people gave the mysterious island, its new residents built a society that was a technical paradise by uninhibitedly utilising the natural resources and applying the latest engineering discoveries with imaginative flair and brilliance. There was just one restriction, one that was serious enough because it meant that their society was doomed in the long term: there was no woman on the island. There was no space for women in the lust for adventure. The adventurers were more like big kids, and popular science still did not cover all divisions of biology; Jules Verne had written his novel in the mid-Victorian era of 1875.

And still nature proved superior. Lincoln Island was of volcanic origin and doomed to destruction. The author's way of ending the adventure was a terrible disaster that he made happen on 9 March 1869:

> The sublime horror of this spectacle passed all description. During the night it could only be compared to a Niagara of molten fluid, with its incandescent vapors above and its boiling masses below.

Here, as is often the case, Jules Verne predicted a dramatic event that was soon due to occur. He did so on the basis of his knowledge of previous disasters such as the Tambora volcano in Indonesia in 1815. Its eruption polluted the air to such an extent that there was no proper summer in Europe the following year and there was snow in Massachusetts during the summer. Another disaster had occurred with the volcanic island of Thera, although not in the form of a volcanic eruption. There was instead a violent steam explosion when water had flowed in. This was the destiny that Jules Verne had prepared for Lincoln Island. The fact that the description in his novel was not an unlikely event was proven in the summer of 1883 when Krakatoa was destroyed.

This island, which in practical terms disappeared at the time of the disaster, lay in the Sunda Strait between Java and Sumatra. It was known to be volcanic and that there had been a volcanic eruption in 1680, but everything had remained calm for more than 200 years since then. The volcano had been giving warning signs since as early as spring 1883. And so it erupted, with the power of an explosion that lasted for two days in August of the same year. It led to sea water gushing into the volcano and being converted into steam, and this meant that the major disaster did actually occur; the steam caused dynamite-like destruction.

When Jules Verne foresaw this event eight years before it occured, he got Cyrus Smith to explain to the sceptical seaman Pencroff what would happen to Lincoln Island:

> 'Well, then, I saw that these fissures widen under the internal pressure from within, that the wall of basalt is gradually giving way and that after a longer or shorter period it will afford a passage to the waters of the lake which fill the cavern.'
>
> 'Good!' replied Pencroff, with an attempt at pleasantry. 'The sea will extinguish the volcano, and there will be an end of the matter!'
>
> 'Not so!' said Cyrus Harding, 'should a day arrive when the sea, rushing through the wall of the cavern, penetrates by the central shaft into the interior of the island to the boiling lava, Lincoln Island will that day be blown into the air—just as would

> happen to the island of Sicily were the Mediterranean to precipitate itself into Mount Etna.'
>
> The colonists made no answer to these significant words of the engineer. They now understood the danger by which they were menaced.

It may be added that Cyrus Harding had in no way exaggerated the danger to be apprehended. Many persons have formed an idea that it would be possible to extinguish volcanoes, which are almost always situated on the shores of a sea or lake, by opening a passage for the admission of the water. But they are not aware that this would be to incur the risk of blowing up a portion of the globe, like a boiler whose steam is suddenly expanded by intense heat. The water, rushing into a cavity whose temperature might be estimated at thousands of degrees, would be converted into steam with a sudden energy which no enclosure could resist. So, I've understood that these cracks are increasing through the internal pressure, that the basalt wall is gradually cracking and that sooner or later it will allow the sea water to pass through, thereby filling the cavern.

This was exactly what had happened with Krakatoa. The thunder from the explosions was heard not just on the Philippines and in Australia but also as far away as Hong Kong and Madagascar, the latter at a distance of almost 5 000 kilometres. A tidal wave rising to more than 30 metres in height washed away the development along the Sunda Strait. It generated breakers that swept across the Indian Ocean towards Africa and across the Pacific Ocean towards South America. The waves continued across the Atlantic and were even observed in the English Channel. 36 000 people were killed along the Sunda Strait and nearby coastal areas.

The pressure waves in the air generated sudden changes in the barometer height across the world and were transmitted 4-5 times around the Earth before they were too weak to be registered. Volcanic ash was still being hurled into the stratosphere and diffused or spread by winds all around the globe. This led to strange optical phenomena which enabled the most magnificent sunsets to be viewed throughout the world for many months after the explosion. Closer to Krakatoa, yet over an area the size of one million square kilometres (more than double the area of Sweden), the tropospheric spreading of the volcanic ash caused substantial air contamination that darkened the sky. The quantity of substances that were spread at the time of the eruption was calculated to be approximately 18 cubic kilometres.

Similarly to the fictitious obliteration of Lincoln Island, the Krakatoa disaster was not a volcanic eruption in the normal sense but a boiler explosion of gigantic proportions. It obliterated Krakatoa and surrounding islands and left behind a vacuum of more than 75 square kilometres down to 200-300 metres below sea level. Sea now lay where there had once been land.

Human beings are extremely small compared with such forces - and what a sensation it was to be able to read off the effects in Europe and, not only that, see the explosion dust from a disaster that had occurred on the other side of the globe!

More than 70 years later, a Swedish professor sat at his desk in the Radiophysical Institute building at *Karolinska Sjukhuset* (Karolinska Hospital) in Solna studying the old scientific reports on the observations following the annihilation of Krakatoa. It had occurred to him that perhaps it could be possible to find experiences from this that would make it possible to better predict how Sweden could be affected by the radioactive substances that, in 1954, started to spread from unbelievably powerful nuclear blasts on the Marshall Islands in the Pacific Ocean around 6 000 km east of Krakatoa and just slightly further west-north-west of Lincoln Island had the latter actually existed.

This professor was one of the pioneers of radiation protection and he plays a major role as my narrative continues. He was posthumously and internationally honoured in that his colleagues decided to name the unit for the radiation dose after him.

His name was Rolf Sievert.

2. LIGHT

And God said 'Let there be light': and there was light. (First Book of Moses, 1:3)

There are four elements, said the aristocratic philosopher *Empedocles*, who was born on the island of Sicily almost five hundred years before Christ. In so saying, he gave his perspective on the matter of the alternative conceptions of the world that had been proposed by the somewhat older philosophers *Heraclitus* and *Parmenides*.

'All things flow,' asserted Heraclitus. 'Fire is the only the ethereal substance that forms everything we see in nature by means of perpetual change.'

'All things stand still,' declared Parmenides. 'That which exists is eternal; thinking and being are the same; everything emanates from the primary opposites of light and darkness.'

'The primary opposites are the conflicting forces,' said Empedocles. 'Love and hate, the force that joins and the force that divides. Everything is made of four elements: earth, water, air and fire. Love holds them together and hate divides them.'

Fire and therefore light was one given element:

'And God saw that light was good; and God divided light from dark'. (First Book of Moses, 1:4)

'And God said: "Let there be lights in the firmament of Heaven to divide the day from the night, that they may be unto signs, seasons, days and years, and let them be lights in the firmament of Heaven to shine upon the Earth." And it was so'. (First Book of Moses, 1:14-15)

The sun shone and the moon shone so that mankind could see, and it 'was delightful to the eyes'. Mankind saw, said Empedocles, by means of his eye carrying a spark of fire and transmitting visual rays similar to tentacles.

'Nonsense,' thought *Aristotle* (384-322 BC) one hundred years later. 'If that were the case, what would prevent mankind from being able to see as well at night as he can during the day?'

The Roman poet *Titus Lucretius*, who was born around one hundred years before Christ, had another explanation. He thought that particles or 'pieces of bark' which contained information on the structure and appearance of the objects were constantly emitted from the surface of every object. These pieces of bark travelled through the air at great speed and generated a sensory impression when they encountered an eye. Lucretius' 'pieces of bark' were certainly not in the style of bark canoes but were presumed to be so small that they were imperceptible until they had penetrated an eye. The fact that they could still convey an image of an entire object was presumed to be because they were emitted from every part of the surface of the object.

Lucretius' image of the particles imparting sensory impressions originated from the perception of the structure of the matter proposed by the Greek philosophers *Anaxagoras* and *Democritus* (both close contemporaries of Empedocles). Since Lucretius referred to these doctrines in his influential didactic poem *De rerum natura* ('Of the Nature of Things'), we have the opportunity to understand them more deeply. Let us listen to Lucretius as he explains Anaxagoras' theory of the *homeomeria* of matter (quoted from Professor Alfred Liljeström's account in part I of *Uppfinningarnas bok* (The Book of Inventions), page 178):

> An example of his primary meaning when using the term homeomeria is that bone is formed from the minutest of small bones, meat from the smallest and finest of meats, blood from a quantity of tiny united drops of blood; he conceives that gold is formed from small grains of gold, that the Earth is concreted from particles of earth, fire from particles of fire, water from particles of water, and he includes all stuffs in

> this mode of formation, while on the other hand not conceding vacuums or an end to divisibility of the stuffs; and in this respect he appears to me to err in the same way as all of the aforementioned philosophers (Heraclitus, Empedocles, etc.). Allow me to add that he is embracing principles that are far too fragile, if indeed they can be called principles, to equip the element with the same nature as the stuffs themselves so that they are subject to tiredness and death and are protected by nothing against destruction.

Lucretius had a greater comprehension of Democritus' doctrines. According to Lucretius, Democritus believed that the world was made of substances and vacuums. The substance can, as Anaxagoras theorised, be divided, but not into the smallest possible pieces. If division continues, you end up with particles that can no longer be divided, and Democritus called these *atoms* (the Greek word *atomos* means indivisible). In his endeavour to explain what an atom was, Lucretius said:

> For the same atoms that form the sky, the sea, the lands, the rivers and the sun also form the grains, the trees, the living beings, but in varying mixtures, combinations and movements, in the same way as among these verses* you see a diversity of words; yet it is easy to see that the content and sound of verses and words do differ. Such is the power of letters when their order alone is changed.

Is it not impressive to hear this discerning explanation and pedagogic simile from a man who lived 2 000 years ago? Lucretius' theory on particles that convey sensory impressions from object to eye formed the basis for the modern perception of light. It was not difficult to ascertain that such particles, just like the rays from a light source, had to move in a linear manner. This basic principle gradually enabled the derivation of a number of optical laws. The Arabian researchers in particular lent many shrewd thoughts to the geometric properties of rays of light. There may have been less knowledge in times gone by but certainly not less capacity for thought. The famous Arabian physicist *Alhazen* (or *Ibn al-Haytham*, 965 to around 1040) says in his work called '*Book of Optics*' from around the year 1000 (quoted from Liljeström):

> When sunlight or moonlight or the light from fire penetrates a room through a narrow opening and the room is dusty or if dust eddies in the room, the light entering through the opening becomes clearly visible through the dust that is interspersed with the air, and it also becomes visible on the floor or on an opposite wall in the room. And you find that the light from the opening travels in straight lines to the floor or to the wall opposite the opening. And if, to compare, you hold a straight stick next to this visible light, you find that the light travels in the direction of the straight stick. If on the other hand the room is free from dust so that the light is seen only on the floor or on the opposite wall and you place the straight stick between the opening and the visible light or hold a taut thread between them and place an opaque object between the opening and the light, the light becomes visible on this opaque object but disappears from the place where it was previously seen. If you move this opaque object backwards and forwards in the direction of the stick, you will always find that the light is visible on the object. From this it is evident that the light travels in straight lines between the opening and the place where it is visible.

The more exact nature of that which was emitted from an object and generated a sensory impression in the eye or of that which was cast through the opening and lit up objects in the room remained a mystery for a long time. However, it was presumed, like with Lucretius, that something had to be cast from the light sources. If the 'particles of light' had different speeds in air, water and glass, this might explain why rays of light change direction, were 'refracted' when they passed between media of varying optical density. The refraction phenomenon was described in the 1st century BC by the Greek

* Lucretius' original text was written in hexameter.

astronomer *Cleomedes.* He had observed that a ray of light that passes from a denser substance such as water to a thinner one such as air is refracted in the direction of the interface. Rays of light from objects on a seabed will therefore be perceived by the eye as though they have come from a greater height. The sea therefore looks shallower than it actually is.

The French philosopher and mathematician *René Descartes* ('Cartesius', 1596- 1650), who was enticed to Stockholm in 1649 by Queen Kristina (and died there the following year as a victim of the cold Swedish weather), endeavoured to find a mathematical law for the refraction of light. The connection between angle of incidence and angle of reflection of the light had previously been studied by *Ptolemy* (in the 2nd Century AD) and by *Johannes Kepler* (1571-1630) in his book called *Dioptrik* ('Dioptrice') in 1611. The Dutchman *Snellius* (actually *Willebrord van Roijen*, 1580-1626) in fact formulated a mathematical link before Descartes published the same formula in 1637 in a dissertation with the rather immodest title of *Discours de la méthode pour bien conduire sa raison et chercher la vérité des sciences. Plus la dioptrique, les météores et la géométrie, qui sont des essais the cette méthode.* However, Descartes provided not only the formula but also an explanation.

The formula stated that the ratio between the sine of the angle of incidence and the sine of the angle of refraction is independent of the angle of incidence and is a constant that is characteristic of the material. If the light penetrates a substance from a vacuum (and in this case air is a good approximation compared with fluids and solid substances), the ratio is called the *refractive index* of the substance. Descartes' simple explanation of the phenomenon was that light travels at different speeds in these two media.

Now, if light were a flow of small particles moving at different speeds in different substances, one natural question to ask would be how fast the speed could be. It was obvious that it had to be much greater than the speed of sound. The latter had been estimated by the French researcher *Pierre Gassendi* (1592-1655) who, at the start of the 1600s, observed a canon and measured the time between the flames and the sound of the shot. His first measurement result indicated that the speed of sound was almost 500 metres per second. *Vincento Viviani* (1622-1703) and other researchers at Accademia del Cimento ('the Academy of Experiment') in Florence repeated the experiment. They estimated the speed to be 352 metres per second, which was not bad since the correct value is 340 metres per second. Did light also have a speed that could be measured? A number of experiments were performed but with no results. Mankind had no idea what speed of light actually was.

However, even if the conditions for measuring the speed of light did not seem to exist on Earth, maybe they could be found in outer space. The Danish astronomer *Ole Römer* (1644-1710) realised that the short distances for observation that were available on Earth would be insufficient to determine very high speeds. He then drew the ingenious conclusion that the speed of light could perhaps be estimated by observing some of Jupiter's 16 moons. The orbit of one of Jupiter's moons could be determined quite accurately by observing when it was obscured while passing behind Jupiter, as of when it left the sun, and was thus no longer illuminated. Such observations could be made when the Earth was closest to Jupiter and when Jupiter was furthest away. The difference in distance between the light signals from the moon of Jupiter would then become the orbital radius of the Earth. Römer found that it looked as though the orbit of Jupiter's moons varied according to the distance between the Earth and Jupiter although the orbit had to be presumed to be constant. In 1675, Römer was able to use the times observed to calculate the speed of light as being approximately 300 000 kilometres per second. That was well done because we now consider the speed of light in vacuum to be 299 792 kilometres per second!

Light travels incredibly fast through air and in vacuum, but does it travel faster or slower in solid substances? This was a question that would be debated for a long time because it was not easy to make measurements of this on Earth. And the question was controversial.

Descartes had presumed that particles of light that passed an interface between two media retained their speed component in a tangential direction unchanged but changed their speed component perpendicular to the surface. In order for the theory to be correct, the speed of light in something such as water needed to be greater than in air since the refractive index of water is greater than one.

The French mathematician *Pierre de Fermat* (1601-1665) had another theory. He thought that particles of light chose the fastest route that led to the goal ('Fermat's principle'). In order for it to be correct, the speed of light had to be slower in water than in air. So, who was right - Descartes or Fermat?

It was a question that was as yet unanswered when *Isaac Newton* (1643-1727) was considering the laws of optics at the end of the 1600s. Not all particles of light could be identical, thought Newton. If light were refracted in a prism, you could obtain a spectrum of colours. Newton realised this must be because the particles of light were of different types and travelled at different speeds and were therefore refracted differently. However, if this were not the case, there must already be particles of light corresponding to the different colours to start with in the ordinary 'white' light before it encountered the prism!

This conclusion was not a completely obvious one at the time. The fact that the white sunlight was refracted in prisms so that different colours could be shown at different angles of incidence was an old observation. It was also a well-known fact that the same phenomenon lay behind the appearance of a rainbow: a spectrum had 'all the colours of the rainbow'. In the 1200s, Polish Dominican monk *Vitello* showed in his dissertation called *Optik* ('Optics') that the rainbow appeared when drops of water not only reflected but also refracted sunlight. In the 1600s, Descartes had discussed the occurrence of the rainbow in detail and was fully aware that different colours of light were refracted in a prism to different extents and that different colours of light originated from white light at the time of the first refraction. However, Descartes realised, reasonably enough, that the different colours did not exist before the first refraction and did not appear until afterwards. Newton, on the other hand, thought that the particles that generated different colours were there all the time, but that 'colour' was not a property of light, instead being a sensory impression on the part of the observer. So, the sensory impression of 'white' was something which the rays could accomplish only by means of joint capacity. Newton himself wrote:

> And if at any time I speak of Light and Rays as coloured or endued with Colours, I would be understood to speak not philosophically and properly, but grosly, and accordingly to such Conceptions as vulgar People in seeing all these Experiments would be apt to frame. For the Rays to speak properly are not coloured. In them there is nothing else than a certain Power and Disposition to stir up a sensation of this or that Colour.

And so Newton advocated the *emission theory*, which held that light is conveyed by some types of particles. When Newton began to experiment with the refraction of light in 1666, Römer had not yet determined the speed of light, and Newton himself was still no great authority. But when in 1704 he summarised his optical discoveries in the masterpiece called *Opticks; or a Treatise of the Reflections, Refractions, Inflexions and Colours of Light*, he had long since published his *Principia* ('Principles') and explained the motions of celestial bodies. His words now carried a great deal of weight. He definitively expressed the emission theory in his optics.

With the help of the emission theory, Newton succeeded in explaining all optical phenomena that were known at the time, and the theory was long heralded as the one generally accepted. However, it did conflict with Fermat's principle. Correctness could be determined only through experiments that had been impossible to perform thus far.

It took until 1850 for someone to pull off that feat. The person who succeeded was Frenchman *Léon Foucault* (1819-1868), who is best known for his experiments to demonstrate the rotation of the Earth using a 64 metre-long pendulum suspended in the cupola in Pantheon in Paris. Foucault used a method that had been developed in the previous year by his compatriot *Hippolyte Fizeau* (1819-1896) while the first endeavours were taking place to determine the speed of light in air on Earth.

Fizeau's method was cunning. Using mirrors, he sent the same beam of light twice towards the ring gear on a rapidly rotating toothed wheel. Between the two passages of light, the light had been sent for as long a distance as possible on the ground and been reflected back by a mirror. If the toothed wheel rotated quickly enough, the following could be presumed: the light that was allowed to pass through the space between two cogs at regular intervals returned to the ring gear at the precise point where the wheel had turned enough for a cog to be able to block the passage. If you knew the number of cogs, the speed of rotation and the distance covered by the light, you could calculate the speed of light.

Foucault modified the method together with Fizeau by replacing the toothed wheel device with a system that instead used a rapidly rotating mirror. This meant that he could use a shorter distance. The experiments that were performed to compare the speeds of light in air and water showed that the speed of light in water was only 3/4 of the speed of light in air. In order for Descartes and Newton to have been correct, the ratio would need to have been 4/3. Foucault's result was a cruel blow to the emission theory.

The latter had long had a competitor in the theory that the light is a wave: the *undulatory theory* (cf. the 'waving' of hair), which presumed that the air, outer space and the gaps between the particles of matter were filled with an elastic substance that could propagate waves through mechanical vibrations. The light could be spread through waves in this substance as sound is spread through waves in air or some other matter.

The existence of this invisible, ubiquitous substance was proposed by Dutchman *Christiaan Huygens* (1629-1695) in a report to the twelve year-old French Academy of Sciences in 1678. Huygens' theory was not published until 1690, however, in his *Traité de la lumière* ('Treatise on Light'). In this, he called the wave-carrying substance 'ether', after the Greek's naming of the clear air above the clouds, *aither*.

If you presumed that light was a wave in the ether, it was easy to derive the laws of refraction in exactly the same way as with Fermat's principle. This lent strong support for the undulatory theory. However, Newton was a great authority and that was sufficient for the emission theory, or the *theory of emanation* as it was also known, to prevail throughout the 1700s.

However, sceptics were raising their voices. At the start of the 1760s, prominent Swiss mathematician *Leonhard Euler* (1707-1783) wrote the following sceptical words (translated from the French by Henry Hunter, 1802) in an instructional letter to the Prussian princess of Anhalt-Theseu:

> [Your Highness],
>
> However strange the doctrine of the celebrated *Newton* may appear, that rays proceed from the sun, by a continual emanation, it has, however, been so generally received, that it requires an effort of courage to call it in question. What has chiefly contributed to this, is, no doubt, the high reputation of the great English philosopher, who first discovered the true laws of the motions of the heavenly bodies: and it is this very discovery which led him to the system of emanation.
>
> *Descartes*, in order to support his theory, was under the necessity of filling the whole space of the heavens with a subtile matter, through which all the celestial bodies move at perfect liberty. But it is well known, that if a body moves in air, it must meet with a certain degree of resistance; from which *Newton* concluded, that, however subtile the matter of the heavens may be supposed, the planets must encounter some resistance in their motions. But, said he, this motion is not subject to any resistance: the immense space of the heavens, therefore, contains no matter. A perfect vacuum, then, universally prevails. This is one of the leading doctrines of the Newtonian philosophy, that the immensity of the universe contains no matter, in the spaces not occupied by the heavenly bodies. This being laid down, there is between the sun and us, or at least from the sun down to the atmosphere of the earth, an absolute vacuum. In truth, the farther we ascend, the more subtile we find the air to be; from whence it would apparently follow, that at length the air would be entirely lost. If the space between the sun and the earth be an absolute vacuum, it is impossible that the rays should reach us in the way of communication, as the sound of a bell is transmitted by means of the air. For if the air, intervening between the bell and our ear, were to be annihilated, we should absolutely hear nothing, let the bell be struck ever so violently.
>
> Having established, then, a perfect vacuum between the heavenly bodies, there remains no other opinion to be adopted, but that of emanation: which obliged *Newton* to maintain, that the sun, and all other luminous bodies, emit rays, which are always

particles, infinitely small, of their mass, darted from them with incredible force. It must be such to a very high degree, in order to impress on rays of light that inconceivable velocity with which they come from the sun to us, in the space of eight minutes. But let us see whether this theory be consistent with Newton's leading doctrine, which requires an absolute vacuum in the heavens, that the planets may encounter no manner of resistance to their motions. You must conclude on a moment's reflection, that the space in which the heavenly bodies revolve, instead of remaining a vacuum, must be filled with the rays, not only of the sun, but likewise of all the other stars which are continually passing through it, from every quarter, and in all directions, with incredible rapidity. The heavenly bodies which traverse these spaces, instead of encountering a vacuum, will meet with the matter of luminous rays in a terrible agitation, which must disturb these bodies in their motions, much more than if it were in a state of rest.

> Thus *Newton*, apprehensive lest a subtile matter, such as *Descartes* imagined, should disturb the motions of the planets, had recourse to a very strange expedient, and quite contradictory to his own intention, as, on his hypothesis, the planets must be exposed to a derangement infinitely more considerable. And so we see an unfortunate example of human wisdom which, in its endeavour to avoid contradiction, often - precipitates to even greater contradictions.[2]

This was what Euler wrote in his 18th letter to the princess. In his 26th letter, he continues the comparison between light and sound waves:

> Luminous bodies must be compared to musical instruments actually in a state of vibration. It is a matter of indifference whether this be the effect of an intrinsic or of a foreign power: it is sufficient for my purpose that sound is emitted. Opaque bodies, as long as they are not illuminated, must be compared to musical instruments not in use, or, if you will, to strings which emit no sound till they are touched.
>
> The question, then, being transferred from light to sound, is resolved into this, whether it be possible for the string of an instrument, in a state of rest, when brought within the sphere of activity of the sound of instruments in a state of vibration, to receive, in certain circumstances, some agitation, and emit sound, without being touched ? Now this is confirmed by daily experience.

The Prussian princess most certainly had the benefit of a good outlook on the achievements in science and philosophy through Euler's letters. In his 28th letter, Euler gives the following summary:

> The ignorance which prevailed respecting the true nature of colours, has occasioned frequent and violent disputes among philosophers; each of whom made an attempt to shine, by maintaining a peculiar opinion on the subject. The system which made colours to reside in the bodies themselves, appeared to them too vulgar and too little worthy of a philosopher, who ought always to soar above the multitude. Because the clown imagines that one body is red, another blue, and another green, the philosopher could not distinguish himself better than by maintaining the contrary; and he accordingly affirms that there is nothing real in colours, and that there is nothing in bodies relative to them.
>
> The Newtonians make colours to consist in rays only; which they distinguish into red, yellow, green, blue, and violet; and they tell us that a body appears of such and such a colour when it reflects rays of that species. Others, to whom this opinion seemed absurd, pretend that colours exist only in ourselves. This is an admirable way to conceal ignorance; the vulgar might otherwise believe that the scholar was not better acquainted with the nature of colours than themselves. But [Your Highness]

[2] The last sentence is omitted in Henry Hunter's translation.

> will readily perceive that these affected refinements are mere cavil. Every simple colour (in order to distinguish from compound colours) depends on a certain number of vibrations, which are performed in a certain time; so that this number of vibrations, made in a second, determines the red colour, another the yellow, and then the green, another the blue, and another the violet, which are the simple colours represented to us in the rainbow.
>
> If, then, the particles of the surface of certain bodies are disposed in such a manner, that being agitated, they make in a second as many vibrations as are necessary to produce, for example, the red colour, I call such a body red, just as the clown does; and I see nothing like a reason for deviating from the common mode of expression. And rays which make such a number of vibrations in a second, may, with equal propriety be denominated red rays; and finally, when the optic nerve is affected by these same rays, and receives from them a number of impulsions, sensibly equal, in a second, we receive the sensation of the red colour. Here every thing is clear; and I see no necessity for introducing dark and mysterious phrases, which really mean nothing. The parallel between sound and light is so perfect, that it hits even in the minutest circumstances.

This was written in 1762 while the emission theory still reigned supreme. It is worthwhile noting that Euler, without daring to speak of waves, still realised that particles of light were characterised by specific *frequencies*, and this meant that he came very close to the modern way of thinking early on.

When, later on, it became more acceptable to view light as a wave, people initially started to talk about the *wavelength* of light although the frequency does actually give a better description (as Euler wrote to the princess).

Each wave - be it waves over the surface of a sea, sound waves through the air or, as we will eventually see, light - can be described by means of its frequency (f) and wavelength (λ). The product of these two is the velocity of the wave ('v' for 'velocity'):

$$f \cdot \lambda = v \qquad (2.1)$$

For sound waves in air, v = 340 metres per second. For an 'A' sound, which has approximately 440 vibrations per second, the wavelength is $\lambda = 340/440 = 0.773$ metres, which is important for an organ builder to know. The speed of light is usually designated by $c = 3 \cdot 10^8$ metres per second (300 000 kilometres per second).

The wavelength is the distance between two consecutive crests. It is easy to understand when we are dealing with *transverse* mechanical waves where the motion in the medium that conveys the wave principally takes place perpendicular to the direction of the wave. True transverse mechanical waves can take place only in solids. The waves over the surface of water can be similar to a transverse wave, however. It is the wave rather than the water that moves forward, but each particle of water takes a circular path. You can still distinguish crests and troughs and thus calculate a wavelength.

It is not as easy to observe wavelengths in a *longitudinal* mechanical wave. Such a wave can also occur in liquids and gases and involves the particles conveying the wave turning in the direction of movement of the wave, which creates a series of pressure increases alternating with pressure reductions. When a source of the wave oscillates at a constant frequency, the wavelength in air, for example, is the distance between adjacent zones of maximum pressure (or minimum pressure). When sound from a vibrating violin string reaches us through the air, a longitudinal wave conveys the air pressure changes to our ear and sets receptors in motion so that we gain an impression of sound there.

The primary objection to the undulatory theory for the propagation of light was that it required a medium that could either vibrate more or less transversally or, as in the case with sound through the air, transfer pressure changes. It had not been possible to show any such medium, i.e., an 'ether', and it was also difficult to picture it.

However, Newton saw other difficulties as well. You could refract the light using a burning glass and show that the collected light also brought heat. With the theory of emanation, it was not difficult to understand that being bombarded with quantities of particles of light could have such an effect. But a *wave*??

And if light were a wave motion as with sound, ought it not also to be spread and bend around corners just like sound? No, the rays of light must consist of small bodies that are emitted from the luminous substances 'since such bodies in a similar medium will move forward in straight lines without bending in shadow consistently with the nature of light.

In 'Query 29' of his *Opticks,* Newton says:

> Nothing more is requisite for producing all the variety of Colours, and degrees of Refrangibility, than that the Rays of Light be Bodies of different Sizes, the least of which may take violet the weakest and darkest of the Colours, and be more easily diverted by refracting Surfaces from the right Course; and the rest as they are bigger and bigger, may make the stronger and more lucid Colours, blue, green, yellow, and red, and be more and more difficultly diverted.

However, Newton was wrong when he did not believe that light, like sound, could bend around corners. He had not understood the explanation of the observations that were made by an academic at the Jesuit College in Bologna, *Francesco Maria Grimaldi* (1618-1663). In *Physico-mathesis de lumine, coloris iride*, posthumously printed in 1665, Grimaldi gave an account of experiments in which he had observed light that had been able to pass through a small aperture in a screen and had then produced a circle of light on a wall behind the screen. If he placed a needle in the beam of light, he saw, not as might have been expected in the shape of a sharp shadow of the needle over the lit surface, but a number of darker and lighter streaks that made the shadow broader than expected. Grimaldi himself realised that the explanation was the deflection of light (*diffraction*), and he did not preclude the possibility of the light being a wave. Similar observations on the fine structure of the outlines of shadows had been made by *Leonardo da Vinci* (1452-1519) as early as the end of the 1400s.

The fact that this bending of light which was easy to ascertain (and which Newton had also observed) but not easy to explain compromised the theory of emanation in the 1700s. In 1818, the French Academy of Sciences arranged a competition on the subject of the bending of light. One of the contributions to the competition was written by a 30-year-old engineer by the name of *Augustin Fresnel* (1788-1827). He theorised - against the competition rules - that light was a wave, and gave an in-depth elucidation of the bending phenomenon plus the fact that light did generally seem to propagate in straight lines. The best thing of all was that his explanation could easily be tested through an experiment, which did actually take place. Fresnel's theory proved to be viable. We now had a phenomenon which could be explained by the undulatory theory if not the theory of emanation.

Some other phenomena remained inexplicable, however, including the polarisation of light. In 1669, Danish doctor *Rasmus Bartholin* ('Erasmus Bartholinus', 1625-1698) had already described the strange properties of a transparent form of calcite called Icelandic spar. If you view an object through an Icelandic spar crystal, the ability of the crystal to diffract the light in two different ways enables you to see two images. One of the strongest endorsements for the undulatory theory was that Huygens had already been able to explain the double refraction of calcite by viewing the light as a wave. However, he had not succeeded in explaining why the double refraction had not occurred in a second calcite crystal that was aligned completely parallel with the first. At the start of the 1800s, French physicist and artillery officer *Étienne Louis Malus* (1775-1812) also observed that some objects did not appear as two images through a calcite crystal. He found that if rays of light were reflected in glass at an angle of less than 55 degrees, they lost the property of double refraction.

With Huygens' theory of light being a wave, it was not possible to imagine waves with properties which meant that their capacity for refraction was affected by the position of the refracting substances in the room. Huygens saw light as a longitudinal pressure wave like sound. Such a wave is characterised solely by its frequency, intensity and direction. It is presumed that all vibrating particles are pushed in the direction of the wave.

It had not occurred to Huygens that the waves of light could be described as transverse waves, i.e., be propagated by a medium that vibrates perpendicular to the direction of motion of the wave in a similar way to the waves over a surface of water. A true transverse mechanical wave such as the standing wave in a vibrating string can be imagined to consist of horizontal and vertical vibrations.

If light were a transverse wave, it would be possible to have vibrations in all conceivable directions perpendicular to the direction of propagation of the wave. It could then be conceivable, thought Malus, that the light would lose all directions of vibration apart from one single plane when reflected against glass or when refracted in a calcite crystal. He proposed that light with such properties be called *polarised*. The light had been given a specific relation to its surroundings and it would be refracted differently in a calcite crystal, depending on the way in which the crystal was held in relation to the light's vibration plane.

Demonstrating the polarisation of light made its wave motion even more difficult to understand. If the wave were transverse rather than longitudinal, it could not be propagated through gases or liquids (the almost transverse waves over the surface of water do not consist of a wave motion through the water but of positional changes in the surface of water). If light were a transverse wave, it had to be presumed that it would be propagated through vibrations in an elastic solid, and it defied common sense to believe that ether could be a solid. They had to be satisfied with ascertaining that light sometimes behaved *as if* it were polarised.

If light were a wave motion, this could mean that different waves of light could affect one another, sometimes reinforcing one another and sometimes actually extinguishing one another, just like the waves over the surface of water. Such interplay between waves of light was described in 1801 by English universal genius *Thomas Young* (1773-1829) – the man who found out the Rosetta Stone mystery which enabled Champollion to successfully interpret the hieroglyphic inscriptions – as follows:

> Suppose a number of equal waves of water to move upon the surface of a stagnant lake, with a certain constant velocity, and to enter a narrow channel leading out of the lake. Suppose then another similar cause to have excited another equal series of waves, which arrive at the same channel, with the same velocity, and at the same time with the first. Neither series of waves will destroy the other, but their effects will be combined: if they enter the channel in such a manner that the elevations of one series coincide with those of the other, they must together produce a series of greater joint elevations; but if the elevations of one series are so situated as to correspond to the depressions of the other, they must exactly fill up those depressions, and the surface of the water must remain smooth; at least I can discover no alternative, either from theory or from experiment.
>
> Now I maintain that similar effects take place whenever two portions of light are thus mixed; and this I call the general law of the interference of light.[3]

This statement corresponded to the observations made by Jesuit Professor Grimaldi approximately 150 years previously. In the latter's posthumous work, 'Proposition XXII' consists of the startling statement: 'An illuminated object may be obscured if you add additional light to the light that is illuminating the same'.

When Grimaldi had studied the bending of light behind a small aperture in a plate, he had made an interesting observation. The spot of light obtained on a wall a long way beyond the aperture did not necessarily become brighter if it coincided with a similar spot of light from an adjacent aperture. He may have expected light plus light to lead to reinforced light, but Grimaldi actually found that light plus light could also lead to darkness. An example of Young's thesis on the interference of light was formulated much later. In 1807, Young showed in his *Lectures on Natural Philosophy* that it was easy to demonstrate this interference by using a sharp knife to make two parallel slits in a screen. If you then illuminated the screen from one side and positioned another screen in a dark area behind the slits, you would see alternating dark and light bands on the screen beyond. Young was easily able to explain

[3] Quoted from Miscellaneous Works of the Late Thomas Young, ed. G. Peacock, 1855.

this by the fact that the incoming waves of light had widened after passing through the slits as if each of the slits had been a source of the wave. This spreading of light means that the screen beyond is illuminated in that it is encountered by the waves of light from each slit. However, the light has taken different paths to different parts of the screen, and a path difference arises for the light from both of the slits except for to the points on the screen which are in the centre of the dividing line between the slits. The path difference means that the waves sometimes fortify one another’s effect and that they sometimes extinguish one another. You will therefore obtain alternating light and dark bands on the screen.

If the incident light is white, bands representing different colours are displaced in relation to one another because the light components corresponding to different colours have different wavelengths.

At the start of the 1800s, Thomas Young was able to describe light as a wave and thereby explain phenomena such as bending (diffraction) and interference. However, in order for his explanation to be viable, light had to be a transverse wave, which required the whole of the universe to be filled with a solid, elastic medium, an ‘ether’, which could propagate the waves. This seemed unreasonable, and the nature of the wave motion therefore remained unknown. It was not until the 1880s that an insight was gained into the connection between light and the as yet little researched phenomenon of electricity and magnetism.

3. ELECTRICITY AND MAGNETISM

Electricity has been a known phenomenon since ancient times. *Thales* from Miletus (640-550 BC) described the way in which amber acquires the strange capacity to attract light objects such as feathers and fluff if you rub it. Thales took this as proof of the *hylozoistic*[*] view that the matter was alive and possessed by demons. For him, this belief was further strengthened when he later experimented with magnetic stones.

In the year 191 BC, *Antiochus III* (who also went by the name of *Megas*, i.e., 'the Great') was defeated by the Romans at Thermopylae. One year later, he was decisively defeated at Magnesia and had to surrender large parts of Asia Minor, including mineral deposits containing black iron ore (ferrous ferrite, $Fe\ (FeO_2)_2$) of the same type as that found in the Swedish iron mines. The ore was initially called *Heraclean* stone (*Lithos Heracleia*) because the city of Magnesia used to be called Heracleia. People later started to call the iron ore 'magnetite' after the city's new name, and this is why the capacity of this iron ore to attract iron objects is called 'magnetism'. Had Heracleia never changed its name, we might now have been calling magnets 'Heracles'!

The Chinese knew long before the birth of Christ that a magnetic force could transmit itself so that something such as an iron needle that was rubbed against a piece of magnetite would itself become magnetic. They also knew that such a needle always aligned itself in a north-south direction if you hung it up so that it could swing freely. The Chinese therefore knew about the *compass,* and it is known that they used this on their ships as early as the 300s. This use spread to the Arabic sailors on the Indian Ocean and from the Arabs to the Venetians. In Europe, the use of the compass is mentioned among sailors in the 1100s, including in *Alexander Neckam's* 'On the Nature of Things' (*De naturis rerum*).

Christopher Columbus (1451-1506) navigated using a compass, and his equipment included a number of extra compass needles and a magnetic stone to magnetise the needles. On his great journey in 1492, he also discovered magnetic variation.

However, Queen Elizabeth the 1st's court physician, *William Gilbert* (1544-1603), is the person who can actually be seen as the pioneer where the scientific study of electricity and magnetism is concerned. He found that many substances other than amber, such as diamonds, sulphur and resin, had the capacity to attract light objects after being rubbed, but that the metals did not have the capacity despite the fact that a couple of them - iron and nickel - can be made magnetic. Gilbert had thereby made a distinction between what we now call insulators and electrical conductors.

Gilbert understood that the magnetic forces were of a different nature from the force exerted by amber after it had been rubbed. He therefore called the latter force *vis eléctrica* after amber's Greek name *e'lectron,* thereby creating the word 'electricity'.

In his masterpiece from 1600, *De magnete, magneticisque corporis, et de magno magnete tellure* ('On the loadstone and on magnetic bodies and on the great magnet, the Earth'[4]), Gilbert described the Earth as a great magnet. However, of the 115 chapters in the book, just one was dedicated to the electrical phenomena - magnetism was the issue that was of immediate interest.

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* From Greek *hyle* = matter and *zoe* = life.

[4] English translation: Paul Fleury Mottelay, 1893

Already in 1269, Frenchman *Petrus de Maricourt*, commonly known as *Peregrinus* (the pilgrim), had described the way in which the influence of magnets appeared to emanate from two poles which, bearing in mind the properties of the compass, could be called the 'north pole' and the 'south pole'. Peregrinus showed that identical poles (i.e., north-north or south-south) repelled one another while different poles (north-south) attracted one another.

In 1733, Ludvig XV's superintendent of Jardin des Plantes, physicist *Charles François de Cisternay du Fay* (1698-1739), commonly known as *Dufay*, discovered that there were also two types of electricity. He called them vitreous electricity and resinous electricity, after the materials you needed to rub in order to produce the electric forces. He found that identical types of electricity made the electric bodies repel one another but that the bodies were drawn to one another if they had different types of electricity.

In 1785, both the electric and the magnetic forces were given a single mathematical description by French engineer and officer *Charles de Coulomb* (1736-1806). He was able to show that the force (F) that operates between two electrically-charged bodies *or* between two magnetic poles is both directly proportional to the quantity (Q) of electricity or magnetism in each body and inversely proportional to the square of the distance(s) between the bodies:*

$$F = k \cdot Q_1 \cdot Q_2 / r^2 \qquad (3.1)$$

where the proportionality constant k has the value of $9 \cdot 10^9$ $N \cdot m^2 / (As)^2$ so that, with the charges stated in coulombs (C) or ampere-seconds (As) and the distance in metres, F, will be stated in newtons (N). This law proved to be identical in form to Newton's formula from 1687 for the gravitational force between two planets, if Q_1 and Q_2 are the planetary masses at a distance r from one another.

Not just the gravitational force but also the electric and magnetic forces could now be described using one (and the same) formula, and the fact that forces could have an effect at a distance over a vacuum was equally remarkable in all three cases. Many explanations were proposed but none of them was particularly convincing.

Roman Titus Lucretius, who was quoted in the previous Chapter, stated in his theory of the nature of things (*De rerum natura*) that light consisted of 'pieces of bark' which constantly flowed towards the eye from the surface of objects. In the same way, he presumed that there was a flow of magnetic 'seeds' from the magnetic stone and, as he described it (according to Liljeström) 'through repeated collisions displace the air between the stone and the iron. When this space between the stone and a piece of iron is liberated, the particles of iron are drawn into the vacuum whereto they will hurtle with great animation but, because of this, the whole piece of iron will follow that motion, since there is no body whose particles are so tightly bound to one another as the cold, solid iron. It is therefore no surprise that no molecules from the iron could fly into the vacuum but that the whole piece of iron comes along'.

In the 1600s, Descartes added further to Lucretius' theory and primarily saw a magnet as a device that sucked in particles at one end and spat them out at the other. Inside the magnet, the particles flowed in a number of parallel tubes; magnets could thus be likened to a bundle of straws. Outside the magnet, the tube widened to become a flow tube with a broader cross section but the number of particles that flowed in each tube was always thought to be the same.

In his letters to the princess of Anhalt-Theseu, Euler showed that the magnitude of the magnetic force was dependent upon whether the magnetic flow was concentrated as by the magnetic poles or widespread as when at a distance from the magnet. The more 'flow tubes' there were per unit area where the flow proceeded, the greater the force ought to be since more particles then collided with a surface in the path of the flow.

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* English chemist Henry Cavendish (1731-1810) appears to have made the same discovery fourteen years before Coulomb but had not published it.

The influence between electric charges or magnetic poles was tangible evidence that an object was electric or magnetic. The fact that other objects such as fluff or a piece of iron could also be attracted without being electrical or without being magnetic respectively was explained by the fact that they became electrical or magnetic through ‘influence’.

Using Coulomb’s formula (3.1), it is possible to calculate the force (*E*) which at a distance *r* from an electric charge of *Q* ampere-seconds (As) would influence a body with a unit charge (1 As). The force is

$$E = \mathrm{k} \cdot Q / r^2 \qquad (3.2)$$

With the value of the proportionality constant k as stated in (3.1), the force will be expressed in newtons (N). Since this influence is to be found everywhere in space, it is said that there is an ‘electric field’ and that the force is the electric *field strength* (E). A great deal has taken place with physical quantities and units since Coulomb’s time, and we will see later on that the electric field strength is now stated in volts per metre.

The electric field strength (depending on which electric charges are around and about) at each point is thus quite simply the force that would influence a hypothetical, unit-charged object should it happen to be present. So long as the hypothetical object is missing, no influence is exercised and the electric field strength is then not felt although it is still present. We stipulate that the field strength is a property that exists in space around the charged objects.

Correspondingly, you can calculate the force that would influence a magnetic unit pole in vacuum at a distance of r metres from a magnetic pole with a given pole strength and call it the *magnetic field strength* (H). However, this method of defining a magnetic field strength is of less practical interest because no ‘magnetic charges’ corresponding to the electric ones have been found. As we are about to see, it is more worthwhile linking magnetic forces with electric charges in motion.

The electric field strength at a given point is not usually caused by a single electrically-charged object but by electric charges in many places in the surroundings, nearby and at some distance away. However, with the help of Coulomb’s law, it is possible mathematically to add the force contributions from all charges and thereby calculate the total field strength.

It is usually also not a matter of calculating field strengths in vacuum but in air or inside solids made of different materials. Such calculations must take into account the capacity of the material to be penetrated by the electric and magnetic fields. This can be done using a calculation method that was pioneered by British physicist *Michael Faraday* (1791-1867), who is considered to be the father of modern electrotechnology.

Faraday was born into a home that was not well off and his father was a blacksmith. He learned the ropes of bookbinding as an apprentice from an early age which, luckily enough, was what gave him his initial contact with books to an extent which would otherwise not have been possible. While collecting extracts from natural science literature, he performed experiments under his own steam.

In 1812, Faraday obtained permission to listen to a lecture by *Sir Humphry Davy* (1778-1829), who was then at the top of his tree as a chemist and experimenter. Faraday wrote a report on the lecture and sent it to Davy. The latter was interested in the young apprentice bookbinder and employed him as an assistant, although the first few years were spent principally as a valet on a long European journey.

Upon his return, Faraday was able to carry out his own experiments. He soon showed that he had a unique skill and imagination. In 1823, he was elected as a member of The Royal Society. During 1831-1851, he performed a number of experiments that were published in the Royal Society’s documents under the title of *Experimental Researches in Electricity.*

Faraday’s lack of mathematical schooling meant that he was never drawn towards immersing himself in the depths of the theoretical analyses that could have been thought of after each new discovery. Instead, he drew the most astute conclusions through intuition and planned experiments that no-one else had imagined.

When Faraday was pondering the nature of electric and magnetic influences, he paid particular attention to the graphic demonstration of the magnetic field that is obtained if you place iron filings on a sheet of paper above a magnet. The filings then adopt the positions that appear to directly illustrate the flow tubes imagined by Euler. You could also say that they illustrated the pathways according to which magnetic poles would be forced to move had they been brought into the field of force.

Faraday saw the flow tubes or lines of force as something that existed in an actual, physical sense. The different magnetic poles were linked through lines of force that moved through the matter between them or through vacuum. From each magnetic pole it was possible to imagine as many lines of force or flow tubes as the strength of the magnet's pole expressed in unit poles. In the same way, it was possible to imagine that a line of force or a flow tube emanated from each electric unit charge which, according to Faraday always had to culminate in an electric unit charge of the opposite sign. Faraday thought that positive and negative electric unit charges were always connected through flow tubes.

The British physicist *William Thomson* (1824-1907), who later became *Lord Kelvin* and Professor of Mathematics in Glasgow, found that Euler's theorem whereby field strength should be proportional to the number of flow tubes per unit area needed to be modified where it did not concern vacuum. Thomson thought that the electric field strength ought to be related to the number of flow tubes per unit area (the *electric flux density*, D) through the expression

$$E = D / (\varepsilon \cdot \varepsilon_o) \qquad (3.3)$$

For the magnetic field strength (H), it should correspondingly apply that the relation with the *magnetic flux density* (B) can be written as

$$H = B / (\mu \cdot \mu_o) \qquad (3.4)$$

The constants* in these expressions were the *dielectric constant* ($\varepsilon \cdot \varepsilon_o$) and the *magnetic permeability* ($\mu \cdot \mu_o$).

One example of the consequence of Thomson's formulae is that it is possible to calculate the properties of a capacitor, e.g., two parallel metal plates at a short distance from one another. If one of the plates is given an electric charge, you can measure an electric voltage between the plates. Assume that the voltage is U = 100 volts. If the distance between the plates is 2 cm, the electric field strength (E) at each point between the plates is 100/0.02 = 5 000 volts per metre.

Electrically non-conductive materials usually have the value of ε between 2 and 6. Presume that we are supplying a material with ε = 5, which completely fills the space between the plates. According to Faraday, since we have not changed the electric charge, the same number of electric flow tubes must still pass between the plates and the flux density (D) remains unchanged. On the other hand, ε has increased from approximately 1 for the air to 5 for the insulating material. According to Thomson's formula (3.3), the field strength has then been reduced to 1/5 of the original value and is thus 1 000 volts per metre. The electric voltage between the plates that are at a distance of 2 cm from one another has then also been reduced to 1/5, i.e., to 20 volts.

William Thomson was struck by the observation that many physical phenomena such as the flow of fluid and the convection of heat could be described using a mathematical tool, *vector calculus.* It looked as though it could also be used to describe the electric and magnetic fields. Some quantities, such as temperature, are *scalar*, i.e., they can be fully described by a numerical value. Others are on the other hand vectorial, such as velocity. Velocity does not just have a numerical value, it also has a direction that can be indicated using an arrow in a diagram. If you get the direction of the arrow to indicate the direction of velocity and make the length of the arrow proportional to the magnitude of the velocity, the arrow becomes a *vector* that represents the velocity.

In a similar way, the electric and magnetic field strengths can be indicated by vectors that state both the magnitude and the direction of the field strength. The art of working with vectors was developed

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* The constants are usually divided into dimensionless *relative* values, ε and μ, and absolute values for vacuum, ε_{oo} and μ_o. The size and dimension of the latter depends on how the electrical and magnetic quantities D, E, B and H are defined. For the sake of practical application, it is the dimensionless, relative values of ε and μ that are of interest. They are now usually called *relative permittivity* and *relative permeability.*

into a special branch of mathematics and played a very significant part in the elaboration of the theory of electricity.

Coulomb's formulae were empirical, mathematical expressions that described the electric and magnetic forces without building on any physical picture or explanation. Faraday, on the other hand, attempted to use his lines of force to show that the influence was determined by the properties of the matter lying in between (which in vacuum was presumed to consist of the mystical ether). According to Faraday, this matter was influenced bit by bit through the molecular near-field effect ('influence') until the influence finally reached over large distances. In 1845 at the age of just 21, Thomson wrote:

> Mr. Faraday's researches on electrostatical induction, which are published in a memoir forming the eleventh series of his Experimental Researches in Electricity, were undertaken with a view to test an idea which he had long possessed, that the forces of attraction and repulsion exercised by free electricity, are not the resultant of actions exercised at a distance, but are propagated by means of molecular action among the contiguous particles of the insulating medium surrounding the electrified bodies, which he therefore calls the dielectric. By this idea he has been led to some very remarkable views upon induction, or, in fact, upon electrical action in general. As it is impossible that the phenomena observed by Faraday can be incompatible with the results of experiment which constitute Coulomb's theory, it is to be expected that the difference of his ideas from those of Coulomb must arise solely from a different method of stating, and interpreting physically, the same laws: and farther, it may, I think, be shown that either method of viewing the subject, when carried sufficiently far, may be made the foundation of a mathematical theory which would lead to the elementary principles of the other as consequences. This theory would accordingly be the expression of the ultimate law of the phenomena, independently of any physical hypothesis we might, from other circumstances, be led to adopt.

Thomson's application of vector calculus to the field concepts led to great gains by means of greater clarity. Thomson also used the potential concept that was introduced as a mathematical tool by mathematicians *Carl Friedrich Gauss* (1777-1855), *Pierre Simon de Laplace* (1749-1827) and *Siméon Denis Poisson* (1781-1840) at the end of the 1700s and the start of the 1800s. In an electric field, the *electric potential* is the work that is required to move an electric unit charge from a reference point (usually an earthed wire) to the point where the potential is stated. Since the force at each point is expressed by the electric field strength, measured in volts per metre, the potential (V) can be stated in volts. The electric 'voltage' (U), which is also stated in volts (V*), is the difference in potential between two points. The *potential difference* is therefore a better name for 'voltage'.

The electric *current density* is a quantity that we have not yet discussed but which is of vital importance to the continued line of argument. In 1729, British physicist *Stephen Gray* (1666-1736) began to publish a number of experiments into the electrical phenomenon in the Royal Society's *Philosophical Transactions*. He had shown that an ivory ball hanging on a 26-foot thread from a glass tube was electrified when the glass tube was electrified by being rubbed. The electric property was transferred through the thread.

The study of the mobility of electric charges was facilitated when the art of collecting electric charges in what we now know as *capacitors* was learned. The first electrical capacitors of significance were manufactured by the German *E.G., von Kleist* (1700-1748) in 1745. He found that a glass bottle covered with an electrically conductive material on the inside and the outside had the capacity to bind an electric charge. Nobody had thought of placing two metal plates close to one another in air. In von Kleist's bottle, a short distance was combined with an insulating material between the metal layers. The field strength could already have been high in air: E = U/d volts per metre, if d is the distance

* The reader should not be confused by the fact that *the quantity symbol* for the electrical potential happens to be the same as the *unit abbreviation* for volt, i.e., 'V'.

between the metal plates in metres and U is the voltage in volts, but with Thomson's formula (3.3), the flux density (D) would be six times greater if the space contained glass rather than air. This follows from the fact that $D = \varepsilon \cdot \varepsilon_o . E$ and that ε is likely to be = 6. Since, according to Faraday, the flux density is proportional to the number of unit charges held, there was a capacity to bind a large electric charge at a given reasonable voltage on the conductive layer in the glass bottle.

When von Kleist's observation was repeated by Dutch researchers and was thereby published for the scientific world, his capacitor bottle became known by the name of *the Leyden jar.*

The Leyden jar was demonstrated at the French court where no fewer than 180 people were asked to form a long chain by holding one another by the hand, whereupon the persons at either end had to touch the inside and the outside of the bottle that was charged with electricity from having been rubbed. The bottle was felt to discharge itself pretty tangibly and convincingly right through the chain of people.

The British doctor Sir *William Watson* (1715-1787) repeated this experiment in 1747 but he replaced the chain of people with a four kilometre-long iron wire. In so doing, he built not only on the experiences of the French court but also on his compatriot Stephen Gray's experiment using considerably smaller amounts of electricity at the end of the 1720s. Dr Watson attempted to estimate the time it took for the electric phenomenon to propagate along the iron wire, but it happened so quickly that it was impossible for him to register any measurable time period.

Following the discovery of the properties of the Leyden jar, playing with electricity became popular and experiments were also performed to use electricity for medical purposes. Already in 1747, a locksmith in Genoa was said to have been cured of a paralysis in his right arm through two months of electric shock treatment.

Much superstition and ignorance abounded, however. The electrical experiments would be closely associated with the fuzzy concept of animal magnetism which, at the end of the 1700s, was spread by German doctor *Franz Mesmer* (1734-1815) after he became doctor of medicine in 1766 in Vienna with a dissertation on the influence of the planets on the human body. Mesmer thought that magnets could be used to cure a variety of diseases, but that the magnets could also be replaced by the mesmerist's own hypnotic power. This mystical animal magnetism is called *mesmerism* after Mesmer.

The book-based film *Gustavianskt* [The Gustavian Age] (Ingvar Andersson et al.) quotes the following from an unnamed Swedish newspaper in 1803:

> The benefit of electricity
>
> A 58-year-old man in the parish of Kuddby whose posture reveals the likelihood of a stroke rapidly became paralysed last spring, with no previous illness, all the way down his right side with no warmth, feeling or power, as if completely dead from head to toe.
>
> He was sent the following day to priest A.G. Dahlin in the parish of Häradshamar, where he was received to be electrified. Upon the first electric impulses, strong enough to melt metals and make holes as if drilling through the thickest of pasteboard, he was completely without feeling.
>
> But, following several repetitions thereof, etc., on the first day he finally got back enough feeling and strength to be able to move his foot and lift his hand to his mouth, on the second day put weight on his foot and lift his hand above his head, on the third day to walk slowly on flat ground, on the fourth day to walk upstairs, on the fifth day to get into his carriage by himself and control his horse, on the sixth day climb alone over fences. In the end, he became so healthy that he was able, after three quarters of a year, to perform all of his household chores as before.
>
> J.P. Wallensteen

It was clearly a common thing for the priests to compete with the medical profession, but the development of medical treatment in the olden times was also running parallel to that of the church. The Swedish Medical Society was only formed in 1807. The following bulletin from Steninge and Heda vicarage on 6 July 1807 is therefore equally unlikely to surprise you (from *Gustavianskt*):

Priest cures the sick with electrical machine

The use of electricity against falling sickness, particularly in nervous patients, and against tapeworm spasms in both older and younger people, has had the desired outcome many a time during my small experiments. The former very often had a high degree of recurring paroxysms during the operation, with pain over the calves, and the illness was not present for a long time if at all. Patients of the latter kind had at these times, during violent twitching and several minutes of blindness and buzzing in the ears, expelled larger or smaller worms. The use of breast milk in the former and bitter laxatives in the latter has generally been found to be of later benefit.

I have often wanted to own a larger electrical machine to be able to do on a larger scale that which I must currently make do with doing on a smaller scale, and to erect a small, separate building containing 3 or 4 beds, partly to accommodate and expand the electricity and the parish pharmacy after the Darelii method; and partly also to be able to protect old, sickly soldiers or other poor folk who may want and need my well-intentioned actions. However, scarcity of assets still denies me this satisfaction.

Steninge and Heda vicarage on 6 July 1807

HANS OLOF SUNDELIUS

Sundelius' reference to the 'Darelii method' did not concern electricity but the work *Sockenapothek och några huscurer* (Parish pharmacies and some household remedies, published in 1760 by *Johan Anders Darelius*, Chief Physician at the Seraphim Hospital in Stockholm.

More scientifically orientated was the experiment that was carried out at the end of the 1700s by *Aloisius Galvani* (1737-1798), Professor of Practical Anatomy at the University of Boulogne. In targeted studies of frogs' nerves and muscle movements, Galvani found that the electric charge brought about severe cramps in frogs' legs. He published the paper called *De viribus electricitatis in motu musculari* ('Commentary on the Effect of Electricity on Muscular Motion') on this in 1791.

Galvani's most interesting discovery, however, was not that the muscles of frogs' legs contracted if they were subjected to electricity. It was that they also contracted in the same way if you touched the nerves in the frog's spinal cord with a brass wire that was connected to the iron plate on which the frog's body lay without any external electricity having been applied. This led Galvani to draw the shrewd – yet incorrect - conclusion that the frog's body itself actually functioned as a source of electricity.

The correct explanation was instead revealed by Galvani's compatriot *Alessandro Volta* (1745-1827). In the 1790s, Volta performed a series of experiments to test Galvani's theory that there was animal electricity in the body of an animal, just as Mesmer had spoken of animal magnetism. Volta examined Galvani's experiments with a critical eye and was able, step by step, to eliminate inessential observations. In May 1792, he still shared Galvani's belief that the electricity originated in the animal's body but, following a few more months of experiments, he changed his mind. He had by then got metal wires to touch two nearby points on one and the same nerve while the animal's body was 'disconnected' from the experiment in electrical terms. Although the muscles that Galvani believed produced electricity were disconnected and insulated, Volta was still able to provoke a muscle spasm that could be explained only by the presence of electricity. So where did this come from?

Volta saw that the only physical factor that he had yet to examine was the contact between the different metals that had been used in the experiments. Perhaps electricity was generated when you allowed different metals to touch one another? However, if that were the case, how was it that this had not been discovered long ago in one of the innumerable electricity experiments?

Volta proposed that the explanation must lie in the fact that, in experimenting with the nerves of frogs, Galvani was the first to use a sufficiently sensitive electricity detector. The nerves reacted to amounts of electricity that were too small to be detected in any other way. This therefore had to be the reason why the contact electricity had remained undiscovered until now.

While continuing his experiments, Volta found that you did not need to use frogs' nerves, as opposed to any other nerves, in order to demonstrate electricity. It was also possible to use human nerves, the easiest being the taste nerves of the tongue.

Volta performed the following experiments (which the reader is advised not to repeat, bearing in mind the risk of suffocation!): he placed a piece of stanniol, i.e., tin, not the aluminium foil of today, which would have been even better, on the tip of his tongue and a silver coin touching it (a silver spoon would have been less risky) towards the back of his tongue. He then experienced an acidic taste on the tip of his tongue. On the other hand, if the stanniol and the coin changed places, he would have experienced an alkaline taste on the tip of his tongue.

This led Volta to arrange the metals and a few other electrical conductors according to the tastes that were produced on the tip of his tongue, from acidic to alkaline:

zinc → stanniol (tin) → iron → copper → gold → silver → charcoal

Volta also found that it was essential to the formation of electricity for the two substances to form a circuit through a moist conductor such as saline solution or saliva on the tongue. He therefore made a pile of alternate silver and zinc metal plates, separated by cloth soaked in saline solution. On 20 March in 1800, Volta announced in a letter to the British Royal Society that he had invented this device with its capacity to give electric shocks. The device was thereafter called the *Voltaic pile*.

It thereby became clear that Galvani's animal electricity had been an incorrect interpretation, although it had contained a grain of truth bearing in mind what we now know about electric rays and cardiac action currents. Volta's pile could be used as a durable source of electric discharges of longer duration than the shocks from the Leyden jars. The Voltaic pile was gradually modified into a series of small, connected vessels containing a saline solution and appropriate metals. In this form, the device would be known as a *galvanic battery* in acknowledgement of Galvani. The further development thereof led to today's torch batteries.

If you connect both of a battery's outer terminals by means of a metal wire, an electric current will pass through the wire, thereby heating it up. The long-lasting electric current that it was possible to obtain from the galvanic batteries was a new experience compared with the short surges from the Leyden jars. Now that it was also possible to experiment with electric currents, i.e., electric charges in motion, this opened the doors to completely new discoveries: the link between electricity and magnetism.

When Danish physicist *Hans Christian Ørsted* (1777-1851) experimented with live wires in 1820, he found that a compass needle changed direction if it came near the wires. This was because an electric current through a wire generated a magnetic field.

Ørsted, who had yet to talk about 'electric current', said that the galvanic battery through the connected wire produced an 'electric conflict' in the surroundings. The newly-discovered magnetic fields distinguished themselves from the previously-studied magnetic fields in that the lines of force did not start and end at the magnetic terminals. They instead described closed circles around the live wires.

A direct connection between electricity and magnetism had now been observed for the first time. Ørsted described the phenomenon in a brochure on 21 July 1820.

There was lively discussion around Ørsted's discovery in various scientific societies, not least the French Academy of Sciences. Physicist and mathematician *André Marie Ampère* (1775-1836) gave an explanation of the phenomenon on 18 September 1820. He wrote (quoted from Liljeström):

> The electromotive effect manifests itself through two types of influence, which I must first separate through accurate definition. I will call the first one electric voltage and the second electric current.

This was how the concept of *electric current* was coined, and later, the name *ampere* was naturally given to the unit in which it was to be expressed. Ampère thought that a current of electric charges flowed through the wire connected to the galvanic battery. The flow produced a magnetic field whose lines of force formed circles that described a rotational motion in the same direction as when turning a screwdriver to screw a screw in the direction of the current. The connection between the flux density of the magnetic field (B) at a distance r from the wire, and the current density (I) through the wire, was formulated by both Ampère and French physicist *Jean-Baptiste Biot* (1774-1862) and *Félix Savart* (1791-1841). *Biot-Savart's law*, also from 1820, gives the relation as

$$B = \text{const} \cdot I / r \qquad (3.5)$$

The true nature of the current, i.e., that which flowed, would remain unclear until the end of the 1800s when studies of gas discharges were used to begin the formulation of theories on electric unit charges in motion.

4. THE ELECTROMAGNETIC WAVES

In 1821, practically-minded Faraday decided to examine whether it was also possible to do the opposite thing. If electric currents generated magnetic fields, was it possible that magnetic fields could also generate electric currents?

One instrument at Faraday's disposal with which to detect and measure electric currents was the *galvanometer*, already proposed in September 1820 by Ampère, which consisted of a compass needle surrounded by a wire through which the electric current was released.

Faraday's first experiment consisted of connecting to a galvanic battery a wire in the form of a coil on a wooden core. On the same wooden core there was also a second wire in the form of a coil around the first. This latter wire was connected to a galvanometer. When Faraday connected the first spool to the galvanic battery, he expected, if Ørsted's and Ampère's experiences were anything to go by, that magnetic lines of force would be formed through the spool. He also expected these lines of force, which also passed through the second spool, to generate an electric current through the latter and thereby deflect on the galvanometer.

However, the galvanometer needle stubbornly pointed in the same direction as before. Faraday did not succeed in generating any current through the second spool in spite of having generated a magnetic field through it. Following repeated attempts, however, he noticed that the galvanometer needle jerked each time he connected his spool to the battery, and also when he disconnected it.

Faraday then drew the conclusion that it was not the magnetic field as such but the *change* to the magnetic field that generated an electric current. He found support for the conclusion by also getting a galvanometer deflection if he moved the live spool rapidly in relation to the other one. As soon as he changed the magnetic field in some way, a surge seemed to pass through the secondary spool.

Faraday lacked the requisite mathematical knowledge to formulate the physical laws to describe his observations, but he systematically implemented all of the experiments that were needed to provide a starting point for a theorist. It was principally thanks to William Thomson (Lord Kelvin) that the available mathematical tool had been pointed out. Thomson's fundamental discussions about these problems were published in 1872 with the help of the colleague who was six years his junior, *James Clark Maxwell* (1831-1879). It would be up to Maxwell to dress Faraday's observations in mathematical attire.

While Maxwell was helping Thomson to edit the latter's collated papers in 1872, he was working on his own epochal work, *A Treatise on Electricity and Magnetism,* which came out in 1873. In this book, Maxwell arranged his renowned equations which definitively linked the electric phenomena with the magnetic ones by including time in the calculation:

$$\text{curl}\,\mathbf{H} = \mathbf{I} + d\mathbf{D}/dt \qquad (4.1)$$

$$\text{curl}\,\mathbf{E} = -\,d\mathbf{B}/dt \qquad (4.2)$$

The equations show that a *change* is needed to the time (i.e., dD/dt or dB/dt) of the electric and magnetic flows for the equivalent magnetic or electric field to be created. The term 'curl' is a mathematical expression for calculating a vortex phenomenon ('curl' is read as 'the rotation') and here, it means that the new field that is created due to each change has a rotational direction around the direction of the flow that has changed. The bold letters **H, I, D, E** and **B** show that the quantities are considered to be vectors and thus have a direction.

Maxwell's equations show that there is no magnetic field around a wire that is charged with static electricity since that is when the electric field constant and both **I** and d**D**/dt = 0. However, if the electric charges start to move, the condition is no longer static and, according to Maxwell's first equation (4.1), the electric current **I** creates a constant magnetic field that produces circular lines of force around the conductor. As long as the current flow remains unchanged, the conduction current **I** is constant and the magnetic field is thereby also constant. As Faraday's first experiment showed, it does not generate a new electric field. If the current is broken, however, the magnetic field ceases to exist, which means that d**B**/dt momentarily has a very high negative value which, according to Maxwell's second equation (4.2), generates an electric field for a moment. It was this field, which existed during the time that it took for the magnetic field to disappear, that generated a surge in Faraday's secondary spool.

Faraday had been dead for six years when Maxwell published his textbook. He therefore never had the satisfaction of seeing his observations generalised through the simplicity of Maxwell's equations which, in spite of his limited mathematical training, he would certainly have understood.

Above all, Faraday would have better understood the inertia against change which the electric phenomena could sometimes exhibit. He had observed that the voltage over a conductive spool did not disappear as soon as you broke the electric current through the spool. About this he wrote the following:

> The first thought that arises in the mind is, that the electricity circulates with something like *momentum or inertia* in the wires, and that thus a long wire produces effects at the instant the current is stopped, which a short wire cannot produce. Such an explanation is, however, at once set aside by the fact, that the same length of wire produces the effects in very different degrees, according it is simply extended, or made into a helix, or forms the circuit of an electromagnet.[5]

It was thus the magnetic field caused by the current which generated the inertia observed by Faraday. The phenomenon that he primarily found strange was that you could short-circuit the spools on a galvanic battery with a short copper wire without any electric spark being formed. If on the other hand you were to connect the battery terminals by means of a wire that constitutes a spool around an electromagnet and then break the contact, a spark would be generated to indicate a larger voltage than the one that the battery could have generated.

This is once again a consequence of Maxwell's equations. The current from the battery can generate a powerful magnetic field through the connected spool, even if the voltage is not large. The strength of the magnetic field depends on just three factors: current density, the number of turns plus the magnetic permeability μ of the iron core of the spool if it has one. According to Ohm's law (from 1827*), the magnitude of the current density in turn becomes equal to the voltage divided by the resistance of the wire which is stated in *ohms* (Ω). With a coarse copper wire, the resistance of the wire becomes insignificant and the current density becomes high even where the battery voltage is low. According to Maxwell's first equation (4.1), the current maintains a powerful magnetic flow through the spool for as long as it passes through the wire in the spool. When the conduction is broken, the current disappears and the magnetic field must thereby also disappear. This involves a powerful magnetic field change which, according to Maxwell's second law (4.2), creates an electric field in the spool. The electric field manifests itself through a voltage between the spools terminals. The faster the magnetic field changes, the larger the electric field strength and thereby the voltage. The voltage can thereby become much

[5] Phil.Trans.R.Soc.Lond.125:41 (1835)

*Deduced by German physicist *Georg Simon Ohm* (1787-1854) following significantly greater practical difficulties than we would ever be able to imagine.

larger than the original battery voltage that gave the current that was sufficient to maintain the magnetic field which later disappeared.

The capacity of a conductive spool to generate a magnetic field is now described by its *inductance* (L) which is indicated by the unit *henry* (H).* A spool has the inductance of 1 henry if a change in current density by 1 ampere per second creates an induced voltage of 1 volt between the terminals of the spool. 1 henry is thus the same as 1 volt-second per ampere (Vs/A).

In order to carry out a practical study of the electromagnetic connection, you also needed a quantity, the *capacitance* (C), which indicates the ability of a capacitor to bind electric charges. The unit for capacitance is the *farad* (F), and 1 farad is the ability of a capacitor to bind the charge of 1 coulomb (1 C = 1 ampere-second, As) when the voltage between the plates of the capacitor is 1 volt. 1 farad is therefore the same as 1 ampere-second per volt (1 As/V).

The interesting thing is that it was now possible to 'translate' the electric phenomenon to mechanical events, which helped the understanding of non-static situations. An electric circuit with a capacitor connected to a spool could, for example, be compared with a mechanical system consisting of a compact mass on a swing axle. The following translation table applies the same mathematical expressions for the vibrations that can be generated:

electric charge	corresponds to	the deflection of the compact mass from its equilibrium position
current strength	" "	the speed of the compact mass
electric voltage	" "	mechanical force active on the compact mass
electrical energy stored in the capacitor	" "	potential energy for the compact mass
electrical energy bound in the spool	" "	the kinetic energy of the compact mass

If you want to displace a compact mass from an equilibrium position, you must add energy by compressing a spring or by lifting a pendulum, for example. The energy will then be bound as potential energy for a moment until the compact mass starts to move. In a new position when the spring is completely unstressed or the pendulum has reached its bottom position, there is no potential energy left. Instead, all energy has been transformed into kinetic energy and the compact mass has reached its fastest speed. As the compact mass swings back and forth, the energy changes between potential energy and kinetic energy while the system is losing energy through friction, which means that the oscillation is damped.

Exactly the same mathematical expression can describe oscillations in electric systems. One comparison tells us that the energy that is bound in a capacitor can generate an electric current (the corresponding motion of the compact mass) through a connected circuit with a spool. This creates vibrations in the electric circuit so that energy is bound alternately as electrical energy in the capacitor and magnetic energy in the magnetic field of the spool. The vibration is damped by energy disappearing from the system in the form of heat in the wire. Faraday was also able to show that energy is lost if the magnetic field is used to carry out work, e.g., to generate movement in an electromotor.

One important property of Maxwell's equations is that they introduce the concept of time into the formulae. Earlier formulae such as Coulomb's law had described purely static conditions. Maxwell's

*After the American physicist *Joseph Henry* (1797-1878).

equations describe the dynamic condition; that which corresponds to motions in a mechanical system. It was inevitable that the next question would then be how long it could take from the time when an electric circuit had been connected over a magnetic spool until the change in the magnetic field strength reached remote points in outer space. Maxwell's equations were formulated in such a way that the change could not arrive at its destination until a certain length of time had passed.

If we imagine a simple case of a straight wire in which a constant electric current is flowing, we know, following Ampère's observations and supported by Maxwell's first equation (4.1), that there is a stationary magnetic vortex field around the wire, i.e., magnetic lines of force that travel around the wire in closed circles. If we suddenly change the direction of the current and wait for a very short time, we can imagine a new situation where - at a given moment - the magnetic field strength closest to the wire is determined by the new current but where 'the message' about the change has not yet reached the areas at great distances from the wire. There must be an area between the new and the old magnetic field where the magnetic field strength undergoes a process of change from the old to the new values. However, according to Maxwell's second equation (4.2), the change to the magnetic field must generate an electric field in the interface.

When the current in the wire is changed, a change to the magnetic field propagates as a wave from the wire and, at the point when the wave arrives, the field change induces an electric field. If the wave passes another wire such as an aerial, an electric voltage will be induced over this for a brief period.

If the current in the wire changes direction not just once but continuously and at a constant frequency (f), i.e., consists of an alternating current, wave after wave will be sent out with new changes to the magnetic field. This also means wave after wave of short-term electric field strengths. The distance between near field waves with the same field change message constitutes a wavelength (λ). The product of frequency and wavelength is, as for all waves, equal to the propagation speed of the wave (v) (cf. equation 2.1).

Unlike mechanical waves where something changes position (e.g., the surface of water or the molecules in the air), the *electromagnetic wave* involves nothing more than progressive changes to the magnetic and electric field strengths in space. Both Faraday and Maxwell looked for an explanation for the remote effect of forces that this involved by imagining some type of elastic medium - like ether - to convey the waves. They worked largely with mechanical analogies in their endeavours to understand the phenomenon. We may need to surrender our ambition to 'understand' the field phenomenon and satisfy ourselves with the world of mathematical concepts which enables us to *describe* them using models.

Many experiments were performed to see how the potential and current electric field quantities, and their units of volt and ampere, could be adapted to the fundamental physical quantities of length, mass and time. Mankind was at liberty to choose its units for these three quantities (metre, kilogramme and second), but when these choices were made, many other units became secondary. Power, for example, can be calculated as mass multiplied by acceleration, thus leading to the dimension of mass × length / time^2, so the unit for force is actually kgm/s^2, although this unit is called the *newton* (N). The unit of energy known as the *joule* (J) is the equivalent of a number of other unit constellations: 1 joule = 1 newton·meter (1 Nm) = 1 watt·second (1 Ws) = 1 volt·ampere·second (1 VAs).

The electric field strength is indicated in volt per metre, but 1 V/m = 1 (Nm/As)/m = 1 N/As, which gives a more comprehensible link to Coulomb's law (3.1). Correspondingly, 1 volt = 1 Nm/As. The electric and mechanical quantities and units systems can be linked if the three fundamental units of metre, kilogramme and second are supplemented with a fourth electric unit, i.e., the ampere. The definition of 1 ampere is now the strength of the current that, when it flows through two incredibly long, straight and parallel conductors placed with a distance of 1 metre between them, causes a mutual influence of $2 \cdot 10^{-7}$ newton per metre of each conductor.

The accommodation of various types of quantity and unit has been a longstanding puzzle to solve which has also involved the option of choosing the magnitude of the constants ε_o and μ_o and of the constant in Coulomb's law. Different unit systems have been tried over the years before the current *Système International d'Unités* (SI) became generally accepted. Depending on where the systems have been defined using fundamental definitions, sometimes a factor of 4π and sometimes a constant of 'c' with the dimension of speed has been required to make everything coherent.

When one of the world's greatest mathematicians, *Carl Friedrich Gauss*, was considering the concept of potential in 1835, he saw a connection between Coulomb's law for charges at rest and Ampère's law for charges in motion, and he drew the conclusion that charges in motion influence one another with different forces from when they are at rest. The idea was taken up by his compatriot *Wilhelm Weber* (1804-1891), who strove to create a unit system based on the electromagnetic phenomenon. Weber attempted to supplement Coulomb's law with a dynamic term, the 'electrodynamic potential', which would take into account the motion of the charges. However, Maxwell's equations would come to do this in a more elegant way.

In order to get his dynamic term added to Coulomb's static one, Weber was forced to introduce a constant that could be written as $1/c^2$ where c had the dimension of speed. In order to be able to test Weber's expression through experiments, the value of this constant had to be known. Along with physicist *Rudolf Kohlrausch** (1809-1858), Weber carried out experiments to estimate c. They found that the numerical value of the constant c was approximately equal to the numerical value for the speed of light. This was encouraging for Maxwell. His equations showed that electromagnetic waves had to have a propagation speed (v) that satisfied the following term:

$$v^2 = 1/(\varepsilon\varepsilon_o \mu\mu_o) \qquad (4.3)$$

This means that the wave must have different propagation speeds in different media. If the speed in vacuum ($\varepsilon = \mu = 1$) is designated by c, this means that $c^2 = 1/(\varepsilon_o\mu_o)$, and therefore:

$$c/v = \sqrt{(\varepsilon\mu)} \qquad (4.4)$$

Were the equivalent expression to apply to the visible *light*, i.e., if c were the speed of light in vacuum (or an approximation in air) and v the speed of light in a denser medium, c/v would be equal to the refractive index (n) for the denser medium. Could it be that light, the true nature of which was as yet fully unknown, was nothing other than an electromagnetic wave?

This was a fantastic hypothesis, but it was possible to test it through experiments. This involved finding out whether $\sqrt{(\varepsilon\mu)}$ really was equal to n for a number of transparent substances, which for most of them would mean that $n = \sqrt{\varepsilon}$ since, as a rule, $\mu \approx 1$. In Maxwell's time, there were no particularly good estimates of ε, but the material that was available did support the hypothesis.

One fundamental idea behind Maxwell's theory was that the electric current should not just be seen as the conduction current that could be explained by some form of electric power transmission through conductors. It was also presumed to consist of a *displacement current* that could be generated by the fact that electric charges that were not free to move but appeared to be bound, for example in the molecules of an insulating material, could still be displaced by very small distances in one direction or the other of the electric field. Where electric field strengths were constant, the displacement current could not permit any movement of electricity in the insulating medium. If on the other hand the electric field changed very regularly, the sympathetic movements of the bound charges would have the same influence as that of a conduction current.

An extension of the concept was based on the idea of an electric field where 'field fluctuations' emanate from each electric charge. A current through a wire then simultaneously leads to a corresponding migration of the field fluctuations outside the conductor. The displacement current could become more important than the conduction current at high frequencies. In extreme cases, the displacement current in space becomes equivalent to the electric current in a conductor. The current in conductor creates a magnetic field, but so does the varying electric field in space, as well as in vacuum.

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* Father of *Friedrich Kohlrauch* (1840-1911) who is well known among countless old Swedish engineers and physics students for his physics textbook which was quite simply called 'Kohlrausch'.

With the absence of the ether that could give the electromagnetic waves a mechanical analogy, we have to allow the mathematical model given by Maxwell's equations to control the concepts.

Maxwell's electromagnetic light theory initially met with some scepticism, that is until 13 December 1888 when young German physicist *Heinrich Hertz* (1857-1894) was able to present a paper, *Über Strahlen electrischer Kraft*, in which he gave an account of the way in which he had produced electromagnetic radiation using a high frequency alternating electric current. There was now no doubt that electric charges in motion generated electromagnetic waves.

Hertz worked with a transmitter and a receiver with no wire connection between them, but both of them equipped with aerials and a spark gap. When the transmitter was in working mode, it was possible to observe sparks in the spark gap of the receiver. No explanation other than that of an electromagnetic wave between transmitter and receiver was possible. Hertz had found *radio waves*. The unit, *hertz* (= one vibration per second), which is now used to indicate frequencies, pays tribute to Heinrich Hertz.

In 1884, Englishman *John Henry Poynting* (1852-1914) was able to use Maxwell's equations to show that electromagnetic fields carried energy and that energy was transported in a direction that is perpendicular to both the electric and the magnetic field strength, which means that the energy is transported in the direction of the wave, the 'Poynting vector'.

If electric vibrations on a macroscopic scale could generate electromagnetic waves, was it then not possible and likely that electric charges in atoms and molecules also could oscillate so that they became sources of electromagnetic waves? If this were the case, an explanation for the radiation of heat had come to light. The higher the temperature, the more active the heat transfer of the molecules of an object - including their electric charges - could be expected to become. This should then mean that warm objects radiated electromagnetic waves at a far higher frequency, i.e., the shorter the wavelength, the higher the temperature would be.

It was now becoming clear that heat radiation and visible light were waves in the form of changes to the field strength of electric and magnetic fields in the same way as the radio waves demonstrated by Hertz. The electromagnetic waves thereby covered wavelengths from a few ten thousandths of a millimetre to getting on for one kilometre. Since the product of wavelength and frequency is equal to the speed of light, 300 000 km/s, this corresponds to frequencies from 100 000 to 10^{15} hertz. The visible light comes either from very hot objects - the sun or an incandescent lamp - where electric charges are in active motion, or as disseminated or reflected light from the object illuminated by the original light (moonlight is one example).

The fact that light from the heated substances carries different colours was used from the mid-1800s as a physical analysis method within chemistry. Pioneers were two Germans, chemist *Robert Bunsen* (1811-1899) and physicist *Gustav Kirchhoff* (1824-1887) who introduced spectroscopy in 1859 to provide help with chemical analyses. The strange thing was that the light did not always consist of a continuous spectrum of wavelengths or corresponding frequencies. If you were to deflect the light from heated samples of various pure elements, you would instead have narrow bands of different colours that were separated by darkness - bands corresponding to wavelengths and frequencies which are characteristic of each substance.

Quantities of wavelength and frequency information were collected over the years. While using the spectroscopy, Bunsen discovered the elements caesium in 1860 and rubidium in 1861. However, was there any relationship behind all this different data?

It would take something of magician to find the connection. The first was Swiss mathematician *Johann Jacob Balmer* (1825-1898) who found a simple relationship behind the frequencies for visible light from hydrogen atoms in 1885. The frequencies were proportional to the difference between the terms $1/2^2$ and $1/m^2$ where m represented whole numbers greater than 2.

A more general law was discovered by Swedish physicist *Janne Rydberg* (1854-1919) from Lund, which was described in 1890 as a formula for the calculation of the *wavenumber*, i.e., the inverted value ($1/\lambda$) of the wavelength of the different rays:

$$1/\lambda = R \times (1/n^2 - 1/m^2) \qquad (4.5)$$

where R was a constant that would be known as the *Rydberg constant*. Balmer's formula was thus a special case of Rydberg's expression, with n = 2. Rydberg's formula had already been modified by Rydberg himself into a more complicated expression when more in-depth studies showed a fine structure of the spectral lines. It is quite sufficient and illustrative for the purpose of my depiction here, however.

Since the relationship between frequency, wavelength and the speed of light is $f \cdot \lambda = c$, the wavenumber can be replaced with f/c. This means that Rydberg's expression for the wavenumber is proportional to the frequency of the radiation. As we go on, I will, as is usual in physics, be using the Greek letter ν ('nu') to designate frequency.

In 1900, the same year in which Janne Rydberg published his expression for the calculation of the wavenumber, *Max Planck* (1858-1947) published his theory on energy quanta. Following Hertz's practical proof of the existence of electromagnetic radiation and the insight that heat radiation and light were highly likely to be generated through oscillations of electric charges within the atoms, it was evident that there would be experiments to search for the relationship between the energy radiated from a warm object and the frequency of the electromagnetic radiation.

The English physicist *John William Strutt* (who became *Lord Rayleigh*, 1842-1919) studied heat radiation for that purpose. Based on classical physics, he reached the surprising conclusion that, according to the theory, the energy radiated from a blackbody* ought to increase beyond all borders if the wavelength of the radiation approached zero. This theoretical find which, luckily, does not relate to reality, is usually referred to as the 'ultraviolet catastrophe'.

German physicist *Wilhelm Wien* (1864-1928) had also studied the radiation of heat from a blackbody, but without coming up with a theory that tallied with the results of experiments. This would instead be the privilege of Max Planck. His explanation was revolutionary and seemingly contravened the laws of classical physics. It had previously been presumed that electric oscillations in the atoms could achieve higher and higher frequencies as temperatures increased, but that all frequencies were actually possible. However, such a hypothesis led to the impossible ultraviolet catastrophe. In order to get the theory to relate to reality, Planck was forced to assume that only some frequencies were possible (as also observed by Balmer and Rydberg) and that the atomic oscillators could not transmit energy continuously, only in the form of intermittent 'wave packets' with amounts of energy that are proportional to the frequency of the oscillator (ν).

Planck called the wave packets *quanta* and, according to him, they can convey only a specified amount of energy, $h \cdot \nu$. The proportionality constant h is called *Planck's constant.* It has the dimension of energy x time and has the value of $6.62 \cdot 10^{-34}$ joule.seconds. *Planck's radiation law* states the way in which the energy emission per wavelength from a black surface varies with absolute temperature (T) and wavelength (λ):

$$dE/d\lambda = \text{const} \times \lambda^{-5} \times \frac{h\,c^2}{e^{hc/k\lambda T} - 1} \qquad (4.6)$$

where h is Planck's constant, c the speed of light and k *Boltzmann's constant* ($k = 1.381 \cdot 10^{-23}$ joule per degree).

Here, the spectrum of the radiation is given with a variable wavelength (as was initially also the case for x rays) because a spectrography registered wavelengths. It is actually more natural to indicate an energy spectrum with the frequency of the radiation as variable or - even more natural - to show the way in which the energy distributes itself over different orders of magnitude of energy quanta, hν. You state the probability density for different quantum orders of magnitude, which is straight to the point.

* In physics, a 'blackbody' means a body that fully absorbs all types of light and heat radiation and that therefore can also radiate at all wavelengths if its temperature is high enough.

This is now done using x rays, gamma radiation and particle radiation, but perhaps piety leads us to write Planck's radiation law with the wavelength as variable.

If we add up the energy contributions from the various wavelength ranges, we see how the total radiated energy varies with temperature. It appears that the radiated energy is proportional to the temperature raised to four ($E = \text{const} \times T^4$); the relationship is known as *Stefan-Boltzmann's law*.*

The wavelength that corresponds to the maximum energy radiation *per wavelength range* is on the other hand proportional to the temperature; this relation is known as *Wien's displacement law* (the maximum of the spectral curves is displaced to shorter wavelengths when the temperature increases). If you instead indicate the spectral distribution over frequency range, you get a completely different impression of the distribution. The relative intensity per range of radiation with different wavelengths changes at the transition from wavelength to frequency, whereas this is not the case for the intensity of spectral lines for monochromatic radiation (i.e., a determined wavelength and frequency).

Planck refrained from giving his theory any physical implication. He said that he had introduced the quantum concept purely to get the formulae to relate to reality. However, the theory had unexpected consequences.

There was still no good hypothesis for the structure of an atom except that it ought to contain electric oscillators. However, the character of these had been unknown until 1897, which is when the electron was discovered by English physicist *J.J. Thomson* (1856-1940). The German physiologist *Hermann von Helmholtz* (1821-1894) had already said in a lecture sixteen years previously that unit particles probably existed as both electricity and atomic mass. Thomson succeeded in passing an electric current through a glass tube in which he had produced an adequate vacuum to prevent disruption by any gas molecules. He was able to show that the current ceased if he added a magnetic field. This related to the hypothesis that the current was conveyed by small, electrically-charged particles. Since these particles could be made to deviate from their normal path by using an electrostatic field and a magnetic field, Thomson was able to balance the electric and the magnetic forces on the particles of the current and thereby determine the e/m ratio between their electric charge and mass. He found that the ratio was approximately 2 000 times the size of the particles of the current as for an ionised hydrogen atom. This had to mean that the particle of the current either had a much greater electric charge than the ionised hydrogen atom or, which Thomson presumed, a much smaller mass. We now believe that the charge of the electron is $1.6 \cdot 10^{-19}$ coulomb (As) and that its mass is $9.11 \cdot 10^{-31}$ kilogrammes.

The name *electron* for the smallest electric charge carrier had been suggested a few years previously by British physicist *George Johnstone Stoney* (1826-1911), but we have already seen the origin of the word in the Greek name for amber - the material on which electric charges were first observed.

Planck had not looked for physical explanations for his energy quanta, but a demonstration of interesting connections was offered by the *photoelectric effect*. This had already been observed by Heinrich Hertz in 1887 when he studied electric currents through discharge tubes. He found that current density increased if he illuminated one of the electrodes with ultraviolet light. Thomson had utilised the same effect when he was searching for evidence that the electron existed.

It was down to *Albert Einstein* (1879-1955) in 1905 to connect Planck's energy quantum, $h\nu$, with kinetic energy, E_k ('k' for 'kinetic'), in the electrons that could be released from a surface after having been irradiated with light. The relationship could be written as

$$E_k = h\nu - W \qquad (4.7)$$

It had been observed that the energy of the electrons emitted was, unexpectedly, independent from the intensity of the light, although ever greater the shorter the shortwave light was. Einstein said that it

* After the Austrian physicists *Joseph Stefan* (1835-1893) and *Ludwig Boltzmann* (1844-1906), the people who later came to interpret the entropy concept.

was the energy in a quantum of light, hν, that was transferred to the electron - although not in full, but that which remained after a certain amount of energy (W) had been expended to release the electron.

Einstein's application of Planck's hypothesis on the photoelectric effect was the definitive departure from classical physics to quantum physics and the first step into the 'age of the atom'. Anyone who is surprised that Einstein was awarded the 1921 Nobel Prize for his studies of the photoelectric effect rather than for the theories of relativity and the famous relationship $E = mc^2$, has forgotten how difficult and important this first step actually was. Its deviation from classical physics was already difficult to accept in the academic world, and the theories of relativity were long considered to be unproven.

The most important properties of light, heat, radiation and radio waves were recognised at the start of the new century without there yet being a modern picture of the actual atoms. Bewilderingly enough, Planck's quantum theory and Einstein's description of the photoelectric effect had shown that even if these types of radiation could be described as waves in electric and magnetic fields, they could also, if the wavelength was short enough, be described as currents comprising energy packets, 'particles of light' or *photons*.

We will now conclude this Chapter by looking at a few concepts and terms that are common to all cases when something flows from a point source, which may be a tube of force, particle masses, energy, or a bee from a beehive.

The *flow* designates the total quantity, mass, number, etc. that has flowed out. *Flux density* indicates the corresponding quantity, mass, number per unit area perpendicular to the flow. If the total flow, M, emanates from a point source and is equally distributed in all directions and is not subjected to any losses along the way, the quantity M will pass through each spherical surface around that point. Since this surface ($4\pi r^2$) increases with the square of the distance, the flux density of a distance r will be $M/4\pi r^2$ ('the inverse square law'). On this basis, the electric flux density (D) around an electric charge (Q) can be written as $D = Q/4\pi r^2$, which is another way of writing Coulomb's law.

Correspondingly, we can talk about *particle flow* and *energy flow* and *particle flux density* and *energy flux density*. We may also need to state what the flow, or the flux density, per unit of time is, i.e., the *flow rate* or *flux density rate*. If it is an energy flow per unit of time, we instead talk about the *power flow* and *power flux density*. If the latter magnitude refers to the power flux density of a radiation, we call this the *intensity* of the radiation, which can then be stated in watt/m^2.

There are unfortunately difficulties when we come to want to provide information on visible light with the corresponding physical quantities. This is where the quantities of physics conflict with the limited eyesight of humans. Our eye reacts only to light within a narrow frequency range, $0.37 \cdot 10^{15}$ – $0.75 \cdot 10^{15}$ hertz, corresponding to wavelengths of between 4000 and 8000 ångström units, i.e., 0.4 – 0.8 micrometres. This means that, when determining the luminous flux and illumination, i.e., within *photometry*, quantities and units which are based on our subjective understanding of light are used.

Since light is a subjective concept that is linked to the sensitivity of our eyes, luminous flux measurements are based on the definition of the *luminosity* of the radiation source which is stated using the unit *candela* (cd). The definition of a candela applies to a monochromatic source of radiation that emits radiation of the frequency 540.0154 terahertz (1 THz = 10^{12} hertz), which is the frequency that our eyes are most sensitive to. It is said to have a luminosity of 1 candela if the radiation's power flux to the receiver is 1/683 watts per steradian.*

A light source that has a luminosity of 1 candela in all directions therefore has a total power flux of $4\pi/683$ watt. It is then said to have a *luminous flux* (Φ) of 4π *lumen*, which also defines the luminous flux unit lumen (lm). Note that the luminous flux is a power flux, i.e., energy per unit of time, and not an energy flux. In luminosity, a candela is the approximate equivalent of a stearine light or a lighting candle, hence the name.

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* A *steradian* is the unit for a solid angle and constitutes the fraction 1/4π of a unit sphere's surface. It corresponds to the aperture angle of 65.56 degrees in a straight circular cone.

The *illumination* (E) of a surface means the luminous flux density. The illumination is given in lumen/m^2, a unit which also goes by the name of *lux*. Since the luminosity of a stearine light is approximately 1 candela, the luminosity flux from the light is 4π lumen and the illumination of a surface at a distance of one metre from the light is 1 lux.

5. DARWIN AND MENDEL

Before we continue looking at the progress within physics and technology, it may be appropriate to take a look at the way in which the knowledge of how living beings came about has progressed. I will loiter primarily around the mid-1800s when two gentlemen by the name of *Johann 'Gregor' Mendel* (1822-1884) and *Charles Darwin* (1809-1882) laid down the basis for the theory of evolution which radically changed mankind's idea of the process of creation.

There are many questions that we brood over as children as soon as we have enough information to be able to think about them - and before we have been indoctrinated by the thoughts of others. How did we come to exist? And why? Why do we start off as children rather than directly as adults? Why do we grow? Why don't adults continue to grow? Why do some children resemble their parents? Why aren't they exact replicas of their parents?

The fact that, generation after generation, people, animals and plants have demonstrated family relationships and similarities between the generations has been evident since ancient times. But why this is so and, most of all, how living beings came about, was a long road to travel.

The fact that people and vertebrates required sexual intercourse in order for children to be born was not difficult to understand, even if the act itself remained a mystery for a long time. As early as the fourth century BC (at the time when Adam and Eve left Paradise according to what those who remain true to the Bible believed during the 1800s), it was understood that intercourse was required with a man for a woman to be able to have a child. According to British anthropologist *Bronislaw Malinowski* (1884-1942), there remained primitive cultures in the 1900s in which it was still not understood which role the man played in reproduction.

Aristotle obviously knew that reproduction for people and higher animals required sexual intercourse, but he believed that lower animals could be generated spontaneously through what would be known as 'abiogenesis'. Aristotle also noted the importance of the egg in cases where the eggs were so large that he could easily see them. Some animals such as birds and snakes hatched such eggs that had grown inside their bodies. Other animals, including human beings, gave birth to young without having any idea of eggs being involved. By observing hens, it was easy to see that a rooster was needed for the hens' eggs to create a chicken. Without the rooster, the eggs did not hatch.

The insects caused Aristotle much trouble. It was easy to see that copulation took place between them and that this led to offspring but Aristotle was mystified as to why he was unable to see any eggs. He wrote (according to Hogben):

> Most insects produce young very soon after copulation. All such types, except for some butterflies and moths, engender grubs. These bring forth a hard substance ... which is fluid within. From the grub grows an animal but not from a portion of it, as is the case with an egg, but the entire thing grows and becomes a developed animal ...

Butterflies and mosquitoes on the other hand belonged to the animals that Aristotle thought came about through abiogenesis. Of those he wrote (quoted from Hogben):

> Other animals do not originate from animals of the same kind, but are formed spontaneously, as some of them are formed from the dew which falls from plants ... Some originate from rotten mud and dung ... The butterflies are formed from caterpillars, and these originate from the leaves of green plants, especially of the

> radish, which some people call cabbage. Gnats come from threadworms and these originate in the mud that occurs in wells and running water which flows over farmland. The sinking mud first turns white, then black and finally red ...

Aristotle's theory of abiogenesis proved to be very robust and I myself as a child of the 1920s met people who firmly believed it. There were sceptics, however, including an Italian researcher by the name of *Francesco Redi* who, in the 1600s, performed a practical experiment to examine whether grubs really did form spontaneously in rotten meat. He placed meat in a bowl which he covered with fine-meshed cloth. When the meat had rotted, it attracted flies. Since the flies could not get to the meat, they laid their eggs on the cloth where they developed into the 'grubs' that Aristotle believed could only originate from the meat.

Another sceptic was British doctor *William Harvey* (1578-1657), the man who discovered the circulatory system in 1628. In his last paper, which was published after his death, he ascertained that *omnia ex ovo* (everything originates from eggs). The reason why he believed this was the possibilities of observation that were afforded by a new invention called the *microscope*. Simple microscopes from Holland were discussed at the end of the 1500s, but the breacthrough for the instrument came as a result of the optical studies carried out by *Johannes Kepler* (1571-1630) in his endeavours to develop telescopes for astronomic observations.

Another major step towards the revelation of the mysteries behind reproduction was taken by Dutch naturalist *Antonie van Leeuwenhoek* (1632-1723). In 1671, he started to produce a microscope with an extraordinary capacity of up to 500 times magnification and a resolution of 1 micrometre. Another one hundred and fifty years or more would pass before any improvement was made.

However, van Leeuwenhoek did more than just produce microscopes - he used them as well. He was a pioneer in the study of microorganisms, and studies of semen led him to discover the *sperm* in 1677. He was also the person who discovered red blood cells. van Leeuwenhoek's studies of sperm led him to draw the conclusion that a new individual did in fact develop from a sperm.

In 1672, another Dutchman, *Reinier de Graaf* (1641-1673) had discovered the vesicle in the ovary, the *Graafian follicle*, which ruptures when an egg is released into the fallopian tube where it can be fertilised. However, he believed that the actual vesicle was the equivalent of birds' eggs and that fertilisation therefore had to take place in the ovary. The real mammalian egg, which lay inside the vesicle, was not described until 1827 by the prominent Estonian *Ernst von Baer* (1792-1876). He was Professor of Zoology and Anatomy in Königsberg at the time, but would become Professor at the Academy of Sciences in St Petersburg in 1834.

So, at the end of 1600s there were two competing theories: one held that the hen's egg was the one that completely formed the new individual and that the male's semen simply initiated the process, while another said that the individual simply originated from the seed and that the hen functioned purely as somewhere for the new individual to grow.

The 'advocates of the egg' believed that a child could inherit its father's external characteristics because the father made such a strong impression on the mother. Similarly, it was presumed that deformities such as 'birth marks' could be due to the mother, while pregnant, having witnessed a blazing fire. It was therefore appropriate to protect a pregnant woman from influences that were far too dangerous.

British doctor *Nehemia Grew* (1641-1712) used the microscope to study the structure of plants which, in 1682, resulted in the work called *The Anatomy of Plants*. He also studied the structure of flowers and identified stamens and pistils as the reproductive organs of plants. The pistils were their female organs and the stamens their male ones. This meant that the pollen grain could be thought to have the same function as sperm, whose existence had been demonstrated by van Leeuwenhoek five years previously. This opened up new ways of understanding the nature of fertilisation, but many years would pass before Mendel would reveal the simplest of the laws of heredity. It was starting to become very clear, though, that new individuals in some way inherited characteristics from both of their parents. In animals, sperm made the father's contribution while the contribution from the mother was in the egg.

It is now time for our Swedish naturalist *Carl von Linné* (1707-1778) to enter the fray. In proposing that stamens and pistils were the male and female sex organs of the phanerogam plants, Linné had a basis for his sexual system: he could classify the plants according to the number of stamens. His contribution was primarily to establish a uniform terminology and the manner of naming genera and species.*

Many myths concerning abiogenesis had now already been removed. Aristotle's belief that gnats come from threadworms which are spontaneously created from mud had been superseded by more exact observations. From Linné's lecture on *Märkvärdigheter uti Insekterna* [Peculiarities of Insects] we can quote:

> See how the *mosquito* (Culex), the *dayfly* (Ephemera), the *night fly* (Phryganea) and the *dragonfly* (Libellula) fly over the water all day to lay their eggs which, when hatched in the water, must live below the water while they remain as grubs; but as soon as they have grown wings, they drown should they once fall into the wet.

In the mid-1600s, the cell as a structural element of plants and animals was as yet an unknown concept which it had not been possible to study without access to a microscope. It was Englishman *Robert Hooke* (1635-1703), assistant to *Robert Boyle* (1627-1691) in Oxford who gave its name. Hooke appears to have designed the air pumps used by Boyle and was not on good terms with Newton. Hooke was the first person to show that biological material - in Hooke's case cork - was made up of small units that he called *cells.*

However, German botanists *Matthias Schleiden* (1804-1881) and *Theodor Schwann* (1810-1882) were the first to study cells from plants and animals in more scientific detail. Schwann demonstrated the likeness between cells irrespective of whether they came from animals or plants, and he showed that all tissues were made up of cells. Schleiden ascertained the importance of the nucleus, wrote poems under the pseudonym of *Ernst* and published *Zeitschrift für wissenschaftliche Botanik* with *Carl von Nägeli* (1817-1891).

The difference between the views of von Nägeli and Schwann illustrates the robustness of the adoption of 'abiogenesis'. Schwann opposed the abiogenesis theory and von Nägeli subscribed to it. He would not come to believe what Darwin said but believed that all species were unchangeable and were created as they are.

A powerful opponent to the theory of 'abiogenesis' of cells (*generatio spontanea)* was the highly esteemed German doctor, Professor *Rudolf Virchow* (1821-1902). Virchow had first studied medicine in Berlin, but in 1849 he was forced to move to Würzburg due to indiscreet criticism of the conditions behind a typhus epidemic. In 1856, he returned to Berlin for a Professorship in Pathology. Virchow emphasised the importance of the cells and said that they were in principle responsible for the body's life processes. He opposed the old *humoral pathology*, which held that diseases were caused by problems in the balance between the four 'bodily fluids' of blood, phlegm, yellow bile and black bile.**

Virchow was far too well-informed to consider believing in any abiogenesis. He realised that each cell originated from another cell, and expressed this with the words *omnis cellula e cellula* (all cells come from cells).

It was now evident that characteristics could be inherited from both parents and that characteristics could also be acquired through education and training. But, could characteristics be inherited in the same way as had been believed that the impression made on the mother during pregnancy could

* The *hierarchical system* that is used to classify plants and animals uses the following order of precedence: *kingdom* > *division* (*phylum* for animals) > *class* > *order* > *family* > *genus* (plural. *genera*) > species. The layperson is surprised that a 'family' is higher up the order than a 'genus', but that is how it is.

** A remnant of the humoral pathology is when we still say today that someone is 'of sound mind' or speak of sanguine temperament (from *sanguis* = blood), phlegmatic (*phlegma* = phlegm), choleric (*cholë* = bile) and melancholic (*cholë melaina* = black bile).

characterise the new infant? This was a general understanding during the 1700s. It was shared by French biologist *Jean-Baptiste de Lamarck* (1744-1829), a Jesuit priest and nobleman who began to take an interest in zoology and became a Professor in Paris despite the French revolution. Lamarck was the first to use the expression *biology*.

Lamarck became the first major representative of a real doctrine when in 1809 he distanced himself from the idea that species were immutable and remained the same as when they were originally created or that they were suddenly changed in leaps and bounds.* He thought that when one did change, it was in the form of an adaptation to the environment so that gradual hereditary changes made the species suitable for the new surroundings. According to Lamarck, a change in the routine use of different body parts could mean that these were changed in the long term. The stretching of a giraffe's neck while searching for food among the leaves of trees is usually given as an example of a Lamarckian change in the length of the neck. Correspondingly, a reduction in the use of an organ would mean that it became more and more insignificant or rudimentary in future generations. Lamarck's theory has been shown to be largely correct, although he never succeeded in finding any reasonable explanation. However, the expression *Lamarckism* has come to be related to views of successors who primarily placed emphasis on Lamarck's (and his contemporaries') mistaken belief that acquired characteristics can also be handed down. One present-day example was Ukrainian *Trofim Lysenko* (1898-1976) who won Stalin's favour by maintaining exactly that.

There had been speculations as to the development of species long before Lamarck and Darwin. The latter would make references as far back as Aristotle in the introductory Chapter to 'The Origin of Species'. Darwin wrote in a footnote that: Aristotle said in his *Physicae Auscultationes* that 'rain does not fall in order to make the corn grow, any more than it falls to spoil the farmer's corn when threshed out of doors'. Darwin continued to quote Aristotle:

> 'So what hinders the different parts [of the body] from having this merely accidental relation in nature? As the teeth, for example, grow by necessity, the front ones sharp, adapted for dividing, and the grinders flat, and serviceable for masticating the food; since they were not made for the sake of this, but it was the result of accident. And in like manner as to the other parts in which there appears to exist an adaptation to an end. Wheresoever, therefore, all things together (that is all the parts of one whole) happened like as if they were made for the sake of something, these were preserved, having been appropriately constituted by an internal spontaneity, and whatsoever things were not thus constituted, perished, and still perish.'
>
> We here see the principle of natural selection shadowed forth, but how little Aristotle fully comprehended the principle, is shown by his remarks on the formation of the teeth.

It was Charles Darwin himself who would give a better explanation of the doctrine. Darwin came from a well-to-do family. His maternal grandfather was a potter, *Josiah Wedgwood* (1730-1795), his father was a doctor and the family lived in Shrewsbury in Shropshire (the area between England and Wales).

After having been to school in Shrewsbury, Darwin spent two years at the University of Edinburgh studying to be a doctor, but he became tired of this and went to Cambridge to study theology at Christ's College. He himself admitted that he was far too financially indulged to have a wholehearted interest in his studies.

At Cambridge, however, Darwin got to know a couple of naturalists, botanist Professor *John Henslow* (1796-1861) and geologist *Adam Sedgwick* (1785-1873), and he accompanied Sedgwick on a study trip to Wales. The interest in geology that this aroused made him amenable to the theories that would be presented later on by another geologist, Sir *Charles Lyell* (1797-1875). Between 1830 and

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* It is less well known that Charles Darwin's grandfather, *Erasmus Darwin* (1731-1802) also maintained that species can change and develop as a consequence of an inherent power. This view may have influenced his grandson to some extent.

1833, Lyell published a book in three volumes entitled *Principles of Geology* in which he estimated the age of the Earth by drawing conclusions from observations of modern processes that influence the surface of the Earth. He came to the conclusion that the Earth must have existed for an incredibly long time, not just the four thousand or so years that were usually calculated from the text in the Old Testament.

Lamarck's development theory had been greeted with scepticism, not just because it went against the theory of creation which said that God created all species exactly as they now appeared, but also because the short time* for which the Earth was believed to have existed would not have been sufficient for drastic changes in species. Lyell's calculations gave the development of species a completely new timeframe.

When Darwin returned from his trip to Wales with Sedgwick, he received an offer through Professor Henslow which completely changed his life. In 1831, British brig sloop *HMS Beagle*, captained by *Robert Fitzroy* (1805-1865), was to sail to South America for a round-the-world trip that would start doing surveys along the coast of Patagonia and Tierra del Fuego or the 'Land of Fire'. Darwin was invited to accompany them as a 'naturalist', but also to offer the captain appropriate company. It was on this trip, which lasted for five years, that Darwin made the observations which, after returning home, he turned into the epochal work *The Origin of Species,* the first edition of which would not be published until 1859, twenty-three years after he had returned home. Darwin had unparalleled conditions on his trip in that he visited so many different environments around the globe and had access to an excellent library on board which made it possible for him to expand his literary knowledge while on the trip. In the library, he also had access to the first volume of Lyell's book on geology.

Upon his return, Darwin married his cousin *Emma Wedgwood*, which did nothing to impair his already good financial situation. This allowed him to continue the rest of his life as a private scholar on the Downs farm in Kent near London, and ill-health following the challenging journey also necessitated this. This is where he worked on his comprehensive observations from the five-year world sailing trip and began to outline his theory on the origin of species. He had a first draft ready by 1844 but did not publish it then. He did on the other hand allow a few interested researchers to see it, including Charles Lyell, who encouraged him to complete the manuscript for printing. Darwin wrote to one of his friends: 'I'm almost convinced (in contrast to the initial view that I had) that species are not (this is like confessing to a murder) immutable'.

Darwin's increasing fear was understandable. A literal Bible interpretation was normal in his time and most people in his cultural world were religious. A theory which deviated from the Bible's theory of creation was provocative and would offend many, far more than we can imagine now.**

In 1858, before Darwin had got around to finishing his book, he was sent a manuscript from *Alfred Wallace* (1823-1913), a British naturalist who was on a research trip to the East Indies (where he stayed between 1854 and 1862). Wallace asked Darwin to read through the manuscript and forward it to Lyell if he liked it. Darwin found that Wallace also had a theory on the origin of species, in many parts the same as Darwin's own. He now had doubts about publishing his book. However, he was convinced by Lyell and others to quickly write a summary of his results and send them along with Wallace's manuscript to the Linnean Society in London. The two contributions were printed under a common title and with Darwin and Wallace as authors in the *Journal of the Linnean Society* 1858

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* It is known that there were civilised forms of society from up to 8 000 years BC in Mesopotamia and 6 000 years BC in China. *Homo sapiens* remains have been found from 23 000 years BC in China where remains of *Homo erectus* have been dated to almost one million years BC. Even older are finds of stone tools in Africa which may have been produced two million years ago by *Homo habilis*, and remains of *Homo erectus* from more than one million years BC. Rock paintings that are more than 20 000 years old have also been found there. The Earth is now estimated to be closer to five billion years old.

** I might be reminded of the following: In an article in *Dagens Nyheter* on 19 January 1995, it is stated that a recently undertaken Gallup survey has shown that 47 per cent of the American population believes that God created man approximately as he looks now and that this took place sometime during the past 10 000 years!

(volume 3). In the following year, Darwin completed his masterpiece *On the Origin of Species by Means of Natural Selection* (1859). One thousand two hundred and fifty copies were printed and, since the rumour of Darwin's strange views had already spread, the whole edition sold out immediately once the book became available on 24 November.

Scientific papers are usually impenetrable to the layperson due to inaccessibility of the substance and the number of specialist expressions. Darwin's book is an exception and can be read through to the end even by a layperson. It is also fascinating to follow his argumentation.

On his journey, Darwin observed nature's wastefulness with newly-born individuals within many species. Nature can even be wasteful with species that produce few offspring, but in such cases with regard to the quantity of gametes. He had also been influenced by *Thomas Malthus* (1766-1834) and the latter's main thesis in *An essay on the principles of population, as it affects the future improvement of society*, a paper that was initially published as anonymous in 1798 but which, when supplemented, was published in 1803. Malthus had pointed out that population increases appeared to take place faster than the growth in possible supplies. Darwin realised that this led, through a fight for survival, to an evolutionary pressure which he realised was general and valid for all species.

If, within one species, there were a variation of characteristics, those individuals that had the most appropriate characteristics under the circumstances should be the ones that coped best under such an evolutionary pressure. These circumstances would then become represented more and more within the species over time so that the species as a whole would become better adapted to a new environment or changed living conditions. In the fight for survival, those who had the best conditions under the prevailing circumstances should therefore be the ones that would survive. Darwin presumed that the characteristics to which this applied were hereditary. If all individuals were identical, fate would determine who survived. With the presence of different characteristics, natural selection would favour the most suitable. Individuals with fewer beneficial characteristics would have a lesser likelihood of reproducing the less favourable characteristics.

In his book, Darwin gave various examples to support his theory, but he did sometimes draw the same incorrect conclusions as Lamarck, namely that a reduction in the use (disuse) of a limb or an organ would mean that it would wither as it was handed down. He wrote the following example in his fifth Chapter:

> I believe that the nearly wingless condition of several birds, which now inhabit or have lately inhabited several oceanic islands, tenanted by no beast of prey, has been caused by disuse.

This regimentation through disuse does not now appear to be a likely explanation. A more likely selection criterion is described by *Rachel Carson* (1907-1964) in her book *The Sea Around Us* (1951) as follows:

> The loss of wing use and even of the wings themselves (the moas had none) are common results of insular life. Insects on small, wind-swept islands tend to lose the power of flight – those that retain it are in danger of being blown out to sea.

Darwin simply did not know what lay behind the variation in characteristics that he was able to observe. He even tested Lamarck's hypothesis that acquired characteristics could also be inherited and that this helped to explain the wide range of material that was available for selection. Darwin's speculation about how this could take place is called the *pangenesis theory*. According to this, all body cells would contribute small particles, *gemmulae*, to the structure of the gametes which would thereby be given information on all of the body's characteristics in detail. This would mean that they also received information on acquired characteristics so that these could be inherited.

The subsequent *neo-Darwinism*, which was represented by German zoologist *August Weismann* (1834-1914), rejects this hypothesis. Weismann maintained that there is a direct developmental line from the fertilised egg to the new individual's gametes, with no influence from other cells, the body cells (*somatic*). This has proven to be a correct hypothesis. The theory on the inheritance of acquired characteristics had many advocates, however, including British engineer and philosopher *Herbert Spencer* (1820-1903), who believed in a theory of evolution that involved constant progress. Spencer expanded on Darwin's theory of evolution so it would also apply to the development of society, a disputed 'social Darwinism' where Spencer introduced the expression survival of the fittest.

Darwin presumed that the biological development through natural selection principally went through a large number of insignificant changes rather than through sudden, powerful changes - which he called 'sports'.* It was still difficult to explain how nature could provide the new characteristics that were required to make individuals competitive in a changed external environment. Mendelian inheritance and spontaneous mutations were still unknown concepts.

The theory of evolution stated that man was one species among others and perhaps shared a common ancestor with 'soulless animals'. This was an almost blasphemous theory since man was supposed to have been created in the image of God. Darwin hesitated for a long time in further developing this consequence of his theory of evolution. It was not until 1871 that he published the paper called *The Descent of Man*.

It offended many people that the human being could be seen as, indeed *was*, a mammal that belonged to the order of *Primates* and thereby related to the monkey. The indignation still lives on in places, with protests against mentioning Darwin's theory of evolution in schools – 'the monkey process' in the USA is one example. I myself was almost lynched by my friends in junior school when, answering the teacher's question as to whether we could give examples of mammals, I replied: humans. 'We are not monkeys!!' shouted my incensed friends at the next break.

Around the time when Darwin's *Origin of Species* came off the press, another researcher began a series of methodical experiments to answer the question on the existence of *abiogenesis* once and for all. It was French biochemist *Louis Pasteur* (1822-1895) who, at this time, was a teacher at École Normale in Paris. Pasteur had shown that fermentation was not, as had thus far been believed, a purely chemical process, but was caused by microorganisms. In around 1860, he began to systematically examine whether there were any grounds for the theory of abiogenesis, *generatio spontanea*. He succeeded in showing that the microorganisms which caused fermentation or the rotting of foods certainly did not occur in the foods - they were in the air. You could therefore store foods for longer if you kept them airtight and insulated. You could also kill the microorganisms using heat. Pasteur's discovery was extremely beneficial to the wine producers who had previously been affected by big losses when yeast fungi attacked the vines. Pasteur showed that the wine did not perish if it was first heated to approximately 55 ºC and that this heating, which would be called *pasteurisation*, did not spoil the flavour of the wine.

While Pasteur refuted the theory of abiogenesis and Darwin pondered the origin of human beings, genetic engineering was taking shape in a monastery in Brünn (Czech Brno). The man shaping it was Augustine monk *Gregor Mendel.* He was born in 1822 in Heinzendorf in Mähren (which then belonged to the Austrian empire) and was christened Johann. After having gone through high school in Troppau (Czech Opava), financial troubles due to his father's illness led to difficulties with his further education. He studied natural science between 1841 and 1843, including at the University of Olmütz (Czech Olomouc), six miles north-east of Brünn, but he had to support himself through private lessons and became very overworked. These difficulties meant that Johann left his university studies in 1843 and applied to enter the Queen Monastery in Brünn where he accepted the name Gregor. He was ordained in 1848.

In becoming a monk, Mendel had not left behind the option of continuing his naturalistic education. The Monastery's abbot was interested in natural science, and crop production experiments had been carried out before Mendel joined. In 1846, Mendel had the opportunity to participate in the course on crop production and artificial cross-pollination between different plants that was given by Professor *Franz Diebl*. After having been ordained, Mendel actively taught between 1848 and 1851 and then studied for three years at the University of Vienna where he was given lectures in scientific

* Cf. 'sports' in rose production, for example.

methodology by Professor of Physics *Christian Doppler* (1803-1853), the man who had described the *Doppler Effect* in 1842. Professor of Physiology *Franz Unger* (1800-1870), also director of the botanical garden, encouraged him to study the cause of variation in different types of plant. Unger suggested that new forms could be a result of a ‘combination of elements within the cells’. Between 1854 and 1868, Mendel was a teacher at the secondary school in Brünn. In 1868 he became abbot of the Queen Monastery in Brünn.

So, it was not exactly an uneducated, unworldly monk who occupied himself with cross-pollination at the Monastery. He was inventive, his experiments were carefully thought out and executed, and his interpretations of the results were characteristically very logical.

Mendel’s research results still had no immediate impact. They were also most unassumingly presented on 8 February and 8 March 1865 in Brünn’s natural research association (which he himself had been involved in forming). A summary report was published the year after that in 1866, *Versuche über Pflanzenhybriden* in the natural research association’s announcements (it was dated 1865). The report was sent to a good few hundred libraries but was not paid much attention. Mendel himself received forty offprints, one of which he sent to Carl von Nägeli in Munich, who thereby got to see Mendel’s discovery of the consistency in the variations of the characteristics of the cross products.

Mendel had carried out his cross-pollinations on peas (*Pisum sativum*). von Nägeli had carried out cross-pollination experiments himself using hawkweed (*Hieracium*) and had found no variation in the cross-pollination products. He therefore believed that Mendel’s results were a special case that applied only to peas and sent Mendel the seeds and plants of the Hieracium species. Mendel experimented with cross-pollination experiments on Hieracium and also found no variation in the cross-pollination - products. So, who was it who had the special case - von Nägeli or Mendel? Mendel suspected that it was Nägeli, and he delivered the explanation at the natural research association at a lecture in 1869. We now know that Mendel was right. The Hieracium family is principally *apomictic*, i.e., reproduces asexually. Dandelions and hawkweed can form seeds without any fertilisation and, in cases where this does take place, there are no conditions for variations in the offspring - variation does not occur until fertilisation.

The two versions of Mendel’s three lectures in Brünn’s natural research association’s announcements are the only printed material in which Mendel describes his work. He left behind no notes or letters; the only letters that are preserved are those he wrote to von Nägeli.

Mendel knew of Darwin’s book but Darwin was probably not aware of Mendel’s work. For Mendel, as an experimental biologist, Darwin’s theory of evolution was reasonably irrelevant. On the other hand, knowledge of Mendel’s discovery could have meant a great deal to Darwin’s philosophy, even if it would not have answered all of his remaining questions.

So, what exactly was it that Mendel had discovered? There was still no knowledge of the nature of the nucleus and of the role of chromosomes as carriers of the gene pool. Mendel was reduced to observing *characteristics* of his pea plants. After having experimented for a few years, he decided to study seven easily identifiable characteristics, i.e.:

1. The shape of the mature peas: round or angular
2. The colour of the endosperm: yellow or green
3. The colour of the seed coat: white or grey-brown
4. The shape of the pod: evenly rounded or constricted
5. The colour of the pod: green or yellow
6. The position of the flowers on the stem: distributed or arranged in flock
7. The length of the stem: tall (6-7 feet) or short (3/4-1 ½ feet)

Mendel firstly ascertained that fertilisation between plants that showed the same characteristics of some of these points also had offspring that always had the same characteristics. If the characteristic for one point was different between seedling and pollen plant, the offspring could be a hybrid with a new characteristic. In some cases, however, the hybrid seemed to have either of the original characteristics that Mendel at that time designated as *dominant* while the other characteristic did not show through until doing so in a few of them in later generations. Mendel called it *recessive.*

If in one pair of characteristics (e.g., round or angular) one of the characteristics was dominant and the other was recessive, Mendel gave their designations with capital and small letters respectively, e.g., A and a. If the fertilisation transfers characteristics on a random basis, there are four possibilities for a combination of the characteristics by means of seed and pollen: AA, Aa, aA and aa, i.e., the ratio of 1:2:1 between characteristic A, hybrid (Aa, aA) and characteristic a. In the next generation, the ratio is 6:4:6 (i.e., 3:2:3); in the next one 28:8:28 (i.e., 7:2:7), and in next generations $(2^n-1):2:(2^n-1)$. For the 10th generation, Mendel pointed out that $(2^n-1) = 1\ 023$. If each plant in each generation gives 4 seeds, you then have 2 048 plants, of which an average of 1 043 have characteristic A, and 1 043 have characteristic a, but only 2 hybrids (Aa or aA).

Mendel then continued to describe an experiment in which he used letters A and a to designate the shape of the seed (A for round and a for angular) and B and b the colour of the endosperm (B for yellow and b for green). He carried out the fertilisation so that the dominant characteristics A and B originated from the seedling and the recessive a and b from the pollen plant. The seeds of the gathered plants were cultivated the following year and their offspring showed nine different shapes with a relative distribution according to the expression:

AB + Ab + aB + ab + 2 A(Bb) + 2 a(Bb) + 2 (Aa)B + 2 (Aa)b + 4 (Aa)(Bb)

Mendel wrote: 'This developmental series is indisputably a combination series in which the particular development series for characteristics A and a, B and b are linked like a chain. You get the complete series through a combination of the following expressions:

A + 2 (Aa) + a
B + 2 (Bb) + b'

Mendel continued to show that a combination of three characteristics A-a, B-b and C-c gave relative frequencies for the offspring of the hybrids corresponding to a combination of the following expressions:

A + 2 (Aa) + a
B + 2 (Bb) + b
C + 2 (Cc) + c

In other words, as he expressed it, 'the proportions for each of a pair of segregating hybrid characteristics are independent from the other differences between the two parent plants'.

If n designates the number of differences in both of the parent plants, according to Mendel, the number of constant characteristics is 2^n. Mendel continued (according to Ernst Nilsson's *Ärftlighetslärans urkunder* [Heredity Records]):

> All constant characteristics that are possible in the *Pisum* through a combination of the 7 stated characteristic traits have also in reality been obtained through repeated crossing. The number thereof is given by $2^7 = 128$. This is also the actual evidence to show that *constant characteristics that occur in different forms of a genus can, when artificially fertilised, form part of all characteristics that are possible according to the combination rules.*

Mendel was almost right, but not completely. The characteristic that is observed cannot be controlled by the same chromosome, but Mendel could not have known this.

Mendel's observations and conclusions would not be noted until the turn of the century; this was the period during which biological research had made progress. In 1875, young German zoologist *Oscar Hertwig* (1849-1922) used studies of sea urchin eggs to demonstrate that fertilisation meant that the male and the female nuclei merged. Improved microscopic technology meant that you could observe thread-shaped structures during cell division in the nucleus. German anatomist *Walther Flemming* (1843-1905) in his capacity as Professor at Kiel was able to show in 1879 that these structures, *chromosomes*, were divided lengthways immediately before a cell divided and that each of the two new cells has as many chromosomes as the mother cell. In 1887, Belgian embryologist *Édouard van Beneden* (1846-1910), Professor at the University of Liège, showed after studying the horse roundworm (which has just four chromosomes) that the fertilised egg received half of its chromosomes from the mother's egg cell and half from the father's sperm. These gametes were formed by means of a special cell division, the *reduction division*, so that they received just half of the normal number of chromosomes. Upon fertilisation, the number of chromosomes in the fertilised egg returned to normal.

van Beneden's observations confirmed Mendel's results, which means that the fertilised egg from each of the parents inherits just one of the possible characteristics in each pair of characteristics. In 1888, chromosomes were given their name by *Wilhelm Waldeyer* (1836-1921) when it became possible to stain them and thereby study them under the microscope.

Additional time would pass before Mendel was rediscovered. This would not be until after the turn of the century when three researchers made similar observations. The one who can take the most credit for having rediscovered the mendalian laws of heredity and Mendel's two works is considered to be German botanist *Carl Correns* (1864-1933), who was Professor at Tübingen in 1900, and who went on to become the first director of the Kaiser Wilhelm Institute for Biology in Berlin-Dahlem.

So, the start of the new century saw the rebirth of genetic science. Mankind knew about the chromosomes in cell nuclei and knew that they were somehow responsible for genes, and that these were randomly transferred from both the father and the mother. But knowledge of deoxyribonucleic acid (DNA) and the genetic code was yet to come.

6. PROFESSOR RÖNTGEN'S RAYS

When William Gilbert published his masterpiece on magnetism in 1600 and, in that context, carefully distinguished between the electric and the magnetic phenomena, he certainly called some substances electric, but he did not actually use the word 'electricity'. He was primarily interested in the electrical influence and had no perception of electricity as something that could conceivably exist in the electric bodies and from them flow over to other bodies.

Later in the 1600s when the first experiments with vacuums began was when new aspects of the electric phenomena were discovered. In the mid-1600s, *Otto von Guericke* (1602-1686), one of Magdeburg's four mayors, succeeded in generating pumps that could suck air from large containers. This meant that he was the first person who could perform comprehensive experiments with vacuums. Before Parliament in Regensburg in 1654, von Guericke was able to demonstrate two hemispheres made of copper which, because they had ground edges, could be joined together into one airtight container. When the air was pumped out of the container, it proved very difficult to pull the hemispheres apart; eight horses needed to be hitched in front of each hemisphere, and when the sphere was finally separated, a big bang was heard when the vacuum was replaced by air.

The Englishman Robert Boyle further developed the vacuum pump so that it was possible to study phenomena at very low air pressure. In 1675, Abbot *Jean Picard* (1620-1682) observed strange light phenomena in the vacuum above the mercury column in the barometers he was working with at the time. Neither Boyle nor von Guericke, both of whom experimented with static electricity, appear to have known of Abbot Picard's observations. On the other hand, von Guericke noticed light phenomena in the air when he moved his hand to his device to produce static electricity. His hand seemed to 'shine like sugar when it is broken' wrote von Guericke.

However, Englishman *Francis Hauksbee* (around 1660-1713) did publish results of in-depth investigations into 'barometric light'. He pumped the air out of a glass tube containing mercury. When he then shook the glass tube, it lit up. Hauksbee guessed that it was the mercury rubbing against the glass which had produced the light phenomenon and that this was electric in nature. He was subsequently able to show that the presence of mercury was not necessary, that is to say he also obtained a light phenomenon if he used his hand to rub a glass vessel containing no mercury and then quickly pumped out the air.

Hauksbee also found that, by holding his hand against a rapidly rotating glass ball, he could make this sufficiently electric to create a spark when he subsequently moved a finger towards the glass. He was thereby able to ascertain that 'something' seemed to move away from an electrically-charged body. When in 1729 Stephen Gray proceeded to give an account of his experiments with the transfer of electricity through conductive threads, this gave birth to the idea that 'electricity' might be something that could be transported between charged bodies if they were connected to threads. It was as though there was some sort of fluid that could neither be weighed nor directly seen other than through the light phenomena that it was able to generate.

Our friend Dufay, who performed his experiments at the French court, realised that there must be two types of electricity. He wrote: 'What characterises these two types of electricity is that they repel one another yet attract one another'.

Benjamin Franklin (1706-1790), who was interested in natural science and who is best known for contributing to the theory of electricity through his invention of the lightening conductor, also thought that there could be two types of bodily electrification: he described the one as a surplus and the other as a deficit of electricity. Electricity is normally evenly distributed, realised Franklin, and the charging of one body must therefore always involve a 'shortage of electricity' in another. Both Dufay's and Franklin's descriptions are pretty coherent with our modern perception of the occurrence of 'positive' and 'negative' electric charges.

On 2 May 1800, British surgeon *Anthony Carlisle* (1768-1840) and chemist *William Nicholson* (1754-1815) discovered that water could be decomposed into hydrogen and oxygen under the influence of electricity. This ignited the interest of the chemists in the electric phenomena, which led to a greater understanding of the transportation of electric charges.

In Sweden, the most prominent electrochemist was *Jöns Jacob Berzelius* (1779-1848) and in England it was Sir Humphry Davy. When Ampère's current flow concept and fundamental electric laws became known during the 1820s (e.g., Ohm's law in 1826), better conditions were created for quantitative experiments. In 1833, for example, Faraday found that an electric current between two electrodes in a saline solution was able, by means of the electrodes, to segregate different substances in quantities that were in constant relation to the amount of electricity (expressed in ampere-seconds, i.e., coulombs) passing through the connecting electrical wire.

Faraday's observations were what made it possible to consider an electric charge to be atomic for the first time. If one and the same amount of electricity always transferred equal amounts of gramme equivalents (the equivalent weight was the molecular weight divided by the chemical valence) of a substance upon electrolysis, and if the number of molecules of a gram-molecule was known, it was possible to examine whether all molecules transported electric charges of equal size or possibly multiples of a specific unit charge. The number of molecules per gram-molecule is determined by *Avogadro's number*, which is $6.02214 \cdot 10^{23}$ mol^{-1} (or per cm^3 gas using *Loschmidt's number,* $2.68676 \cdot 10^{19}$).* Using Faraday's hypothesis, it is possible to calculate the number of molecules that assist with the transportation of a given amount of electricity and thereby the amount of electricity that falls on each molecule. Faraday called such charged molecules *ions* (from Greek *ion* = going).

Such a calculation was made for the first time in 1874 by George Stoney. He presented a paper in Belfast entitled *On the physical Units of Nature*, but it was not published until 1881. In another paper ten years later, Stoney wrote:

> A charge of this amount is associated in the chemical atom with each bond. There may accordingly be several such charges in one chemical atom, and there appear to be at least two in each atom. These charges, which it will be convenient to call *electrons*, cannot be removed from the atom; but they become disguised when atoms chemically unite.[6]

By 'chemical atoms', Stoney was referring specifically to the ions, and when he spoke of 'at least two charges', he must have meant one positive and one negative charge which neutralised one another.

German physicist *Rudolf Clausius* (1822-1888) endeavoured to explain the splitting of chemical compounds into their constituent parts through electrolysis by assuming that the molecules were already split to some extent in the saline solution. This idea formed the basis for the *dissociation - theory* which was presented when a thesis was defended in Uppsala in 1884 by Swede *Svante Arrhenius* (1859-1927).

Arrhenius' doctoral thesis was not well received - his theory was far too bold. He presumed that some of the salt molecules in an aqueous solution dissociated. This did not seem entirely impossible bearing in mind that the dielectric constant of water was approximately 80, which is why any electric

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* Loschmidt's number was calculated by Austrian physicist *Joseph Loschmidt* (1821-1895) based on the law set out in 1811 by Italian physicist *Amadeo Avogadro* (1776-1856) and which says that each gram-molecule, irrespective of the substance, contains the same number of molecules.

[6] Trans.Roy.Dub.Soc., N.S. Vol IV Part XI (1891).

cohesion forces could be expected to be reduced to 1/80 of what they had been in air. Arrhenius also presumed that it was not particularly likely that the moieties would be recombined if they were very dilute. On the other hand, concentrated solutions ought to contain a good number of molecules that had just been reconstructed. Arrhenius therefore said that a number of the salt molecules *dissociated*, i.e., were split down into electrically-charged ions which, thanks to their electric charge, were drawn to the electrodes by electrolysis. Arrhenius wrote the following about his own thesis in 1907 (according to Liljeström):

> The in-depth investigations into conductive capacity brought to my attention the fact that the conductive fraction of the salt molecules must be taken into account in the first instance. While defending his doctoral thesis, however, the faculty opponent, Dr Å.G. Ekstrand*, maintained as an aggravating circumstance that I had presumed that the conductive salt molecules split into ions. Presuming that this objection would set most chemists against my thesis, I endeavoured to emphasise the dissociation as little as possible. This delayed the impact of the dissociation theory for three years.

Faraday (1838) also carried out in-depth studies of what happens at the time of an electric discharge at low pressure, i.e., the phenomena that were studied more than one hundred years ago by Picard, von Guericke, Boyle and Hauksbee. If an electric voltage source is connected to two electrodes in a glass tube at low air pressure, you see a light phenomenon, a 'glow', next to the cathode and in a more widespread area in front of the anode. Between the luminous areas is a dark area that is usually called *Faraday's dark space*.

If the air pressure is further lowered, the Faraday dark space is displaced closer to the anode so that the column of light in front of the anode is compressed and gradually completely disappears. At the same time, the cathode light broadens out even further in the direction of the anode and instead the area closest to the anode becomes dark. At very low pressure, this darkness spreads and fills the whole of the tube whose glass walls then in contrast light up with a slightly fluorescent shimmer.

Even more in-depth studies of phenomena in discharge tubes were made possible by one of Faraday's many important practical inventions, namely the electric transformer. In its simplest form, the transformer consisted of two conductive spools around an iron core and its task was to increase the magnetic induction. The primary spool had a few turns and carried a high electric current when it was connected to a galvanic battery. By alternately connecting and disconnecting the primary spool, it was possible to generate a varying magnetic field through the secondary spool. If this consisted of many turns, a very high total electric voltage would be induced between the wire ends of the secondary spool. The current was initially connected and interrupted through the primary spool using a spring contact to a rotating toothed wheel, but later using vibrating spring tongues in the same way as in simple electric alarm clocks.

Whenever we hear people talking about electric transformers nowadays, we usually think of a step-up or step-down transformation of alternating voltages. However, in Faraday's transformer, the current always passed through the primary spool in the same direction and the voltage over the secondary spool was a pulsating direct voltage. Such transformers for very high pulsating voltages were called *inductoria*, and they became very appropriate tools when someone wanted to explore the electric light phenomena in low pressure gases. One particularly famous design was *Ruhmkorff's coil*, whose secondary spool had a very large number of turns of fine wire (a total of more than 100 kilometres in length!) and produced voltages that were high enough to generate foot-long sparks.

Emperor Napoleon had once offered a prize of 60 000 francs to the person who had taken science forward by 'a step compared with that taken by Franklin and Volta'. The prize was never awarded but it spurred Napoleon III on to organise a prize of 50 000 francs in 1852 for the most important use of

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* *Åke Ekstrand* (1846-1933) was a senior master in chemistry, was a leading figure within the Swedish Chemical Society and in 1907 became senior engineer of the Control and Adjustment Board (a government agency which from 1919 to 1971 came to oversee the application of the alcohol legislation.

the Volta battery. This latter prize was awarded just once, in 1864, when it went to the designer of the Ruhmkorff coil, the German instrument manufacturer *Heinrich Rühmkorff* (1803-1877) who was active in Paris.

Inductoria and improved vacuum technology enabled the study of the electric discharge phenomena in vacuum in far greater detail. A valuable contribution was made by German instrument manufacturer *Heinrich Geissler* (1814-1879) who, in designing a new vacuum pump, made it possible to easily produce vacuum-pumped glass tubes, known as *Geissler tubes*.*

Among those who experimented with the glow light and Geissler tubes were Germans *Julius Plücker* (1801-1868) and *Johann Wilhelm Hittorf* (1824-1914), and Englishman *Sir William Crookes* (1832-1919). The latter had already found in 1857 that the column of light in the glow tube could be made to change position using a magnet. When we look at the years in which these researchers were born and died, it may be of interest to place Jules Verne and his mysterious island into the historical context. Jules Verne was alive between 1828 and 1905 and wrote *L'Ile mystérieuse* [The Mysterious Island] in 1875, setting the event in 1865.

Hittorf was able to show that objects placed inside the glass tube before the air was pumped out were able to get the strange light by the glass wall to disappear just as if the objects were casting a shadow on the wall. Crookes repeated Hittorf's experiment using a discharge tube in which the anode was shaped like a cross which could stand upright in the tube or be laid down. He was thereby able to observe the 'shadow' of the cross on the glass wall and also to note signs of how the ability of the glass wall to fluoresce became tired when no shadow had been thrown.

At the end of the 1870s, as a consequence of Hittorf's and Crookes' experiment, people began to talk of *cathode rays* which seemed to emanate from the cathode in electric discharge tubes when the air pressure was sufficiently low. Crookes realised that the cathode rays were electrically-charged particles.

In 1890, English physicist Sir *Arthur Schuster* pointed out that it ought to be possible to calculate the ratio between the charge (e) and the mass (m) of the cathode ray particles if their speed were known and it was possible to measure their change of direction in a known magnetic field. However, it was still not possible to determine their speed.

In 1892, Dutch physicist *Hendrik Lorentz* (1853-1928) put forward a theory stating that all matter contains small, electrically-charged particles for which he used Stoney's designation 'electrons'. Supported by this hypothesis, Lorentz was able to explain some optical phenomena that had not been covered by Maxwell's original electromagnetic wave theory.

In his Nobel Prize lecture in 1902, Lorentz said:

> A successor to Maxwell now has merely to translate this conception of co-vibrating particles into the language of the electromagnetic theory of light.
>
> Now what must these particles be like if they can be moved by the pulsating *electrical* forces of a beam of light? The simplest and most obvious answer was: they must be electrically charged.

Did the cathode rays consist of electrons? In 1892, Hertz showed that it was possible to force the cathode rays out of the Geissler tube if you made a hole in the glass wall and covered this with a thin 'window' of aluminium. This discovery was then further developed by his assistant, Hungarian *Philipp von Lenard* (1862-1947). Hertz' discovery appeared to contradict the idea that the cathode rays consisted of particles since it was difficult to believe that particles could penetrate through solid matter.

The *Cavendish Laboratory* had been established in Cambridge in England in 1870 in memory of chemist Henry Cavendish, the man who actually discovered Coulomb's law before Coulomb did but

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* Geissler and Plücker are also known for a completely different achievement: they were the first people to discover that the density of water is at its greatest at 4^{o}C, which, luckily, means that lakes will not start to freeze solid.

had not published the discovery. The first director of the laboratory was Maxwell and, after his death in 1879, he was succeeded by Lord Rayleigh. The latter was succeeded in 1884 by J.J. Thomson, who had still not reached the age of 28.

In 1894, Thomson succeeded in estimating the speed of cathode ray particles using a device with rotating mirrors, and he found that it was less than one thousandth of the speed of light. The cathode rays therefore could not consist of electromagnetic waves - these had to propagate at the speed of light. Not until 1897 would Thomson (as we saw in Chapter 4) succeed in demonstrating the electron and estimating its mass and electric charge. But there was one other epochal discovery before this.

In the autumn of 1895, a great deal was evidently known about cathode rays, though by no means everything that there was to know. The conjecture was that they consisted of incredibly small, electrically-charged particles that were emitted the cathode and drawn to the anode of the electric field at very high speed, although just a few thousandths of the speed of light. The share of the cathode rays which passed the anode without being captured by it generated a greenish fluorescent light when they collided with the glass wall of the vacuum tube. If the air pressure was not far too low, cathode rays also collided with residual air molecules, giving rise to a glow.

Another part of the glow was thought to be generated by positively-charged particles that emanated from the anode. The effect of these particles could be observed once they had passed jumps established for the purpose, 'channels', in the cathode. They were therefore called *channel ray* when they were discovered in 1886 by German physicist *Eugen Goldstein* (1850-1930). We now know that they consist of ionised atoms and positively-charged molecules.

For the experimenters in the mid-1890s, the most popular discharge tubes were the Crookes, Hittorf and Lenard tubes, all intended for the study of cathode rays and their fluorescence effect. Numerous physicists performed experiments with such tubes.

Late in the evening of 8 November 1895, one of them was preparing himself for yet another cathode ray experiment in Würzburg - an experiment whose results would have a powerful influence on the continued development of natural science. His name was *Wilhelm Conrad Röntgen* (1845-1923), and he was born in the upland city of Lennep north-east of Cologne on 27 March 1845.

Röntgen's career is something of a cautionary tale. He was expelled from the high school in Utrecht where he was living with relatives while studying. The reason he was expelled was his obstinate refusal to reveal which of his friends had drawn a cartoon of a teacher on the blackboard. When he was excluded from his high school studies, he was forced to take up more practical studies in Holland but happened, through his interest in the locomotive at the railway depot, to meet a Swiss engineer who helped him to apply to the technical college in Zürich.

By virtue of this assistance, Röntgen graduated in engineering in 1868, specialising in machine technology, and was subsequently able to defend his thesis at the college, which he did the year after. Being somewhat surprised at this success yet unsure as to what he would do in the future, Röntgen contacted the young experimental physicist and Professor of Acoustics *August Kundt* (1839—1894). Kundt asked:

'What do you actually want to do with your life?'

'That's exactly what I'm not sure of, Professor!' responded Röntgen.

'Would you like to try your hand at physics?' asked Kundt, whereupon the extremely honest Röntgen responded:

'I've never been involved in that as yet.'

For some unknown reason, Kundt's completely unexpected answer was:

'It's always possible to make up for lost time!'

And he employed Röntgen as his assistant. It didn't take long for Röntgen to make himself useful, and when in 1870 Kundt was offered a professorship in Würzburg, Röntgen accompanied him. When Kundt nominated Röntgen for a senior lectureship immediately thereafter, however, the faculty refused to accept him because he did not hold a school leaving certificate!

Kundt and Röntgen moved to Strasbourg a few years later where Röntgen became an unpaid 'Private Lecturer' (i.e., dependent upon the fees that he could obtain directly from his students) and eventually also a Professor.

During the 1880s, Röntgen made a number of significant achievements within physics and discovered the *Röntgen ray*, i.e., the capacity of electrically non-conductive material to induce a magnetic field when set in motion in an electric field. In October 1888, he was offered a professorship in Würzburg by the same faculty that had refused him a senior lectureship.

On the evening of 8 November 1895, we find Röntgen prepared to continue his studies of the fluorescent effects of cathode rays in his laboratory on the bottom floor of a building by the Pleicher Ring. It is difficult to fully reconstruct what really happened that night since Röntgen was a modest man of few words who rarely gave interviews and who ordered that all of his papers be burned following his death. However, in April 1896 he gave an interview to a reporter from McClure's Magazine. The interview, which is reproduced verbatim in Friedrich Dessauer's book about Röntgen, is the most reliable account we have of the way in which Röntgen rays were discovered:

> 'Now, Professor,' [began H.J.W. Dam, McClure's reporter], 'will you tell me the history of the discovery?'
>
> 'There is no history,' he said. 'I have been for a long time interested in the problem of the cathode rays from a vacuum tube as studied by Hertz and Lenard. I had followed theirs and other researches with great interest, and determined, as soon as I had the time, to make some researches of my own. This time I found at the close of last October. I had been at work for some days when I discovered something new.'
>
> 'What was the date?'
>
> 'The eighth of November.'
>
> 'And what was the discovery?'
>
> 'I was working with a Crookes tube covered by a shield of black cardboard *. A piece of barium platino-cyanide paper lay on the bench there. I had been passing a current through the tube, and I noticed a peculiar black line across the paper.'
>
> 'What of that?'
>
> 'The effect was one which could only be produced, in ordinary parlance, by the passage of light. No light could come from the tube, because the shield which covered it was impervious to any light known, even that of the electric arc.'
>
> 'And what did you think?'
>
> 'I did not think; I investigated.'

These words of Röntgen's have gone down in history: *Ich dachte nicht, sondern ich untersuchte.* 'Tentando, non cogitando' became a familiar motto following the publication of Dam's article.**

Röntgen's high voltage source was a Ruhmkorff coil which can now be seen at the *Deutsches Museum* in Munich. It emitted between 10 and 25 discharges per second and the top voltage is estimated to have been between 40 000 and 60 000 volts (40-60 kV). Röntgen's discharge tube was in all probability a simple Hittorf tube where the anode was at the side of the tube to give the cathode rays free access to the glass at the end of the tube.

Röntgen experimented intensively but privately for seven weeks after his discovery. On 28 December he had finished his research and gave his first report, *Eine neue Art von Strahlen* ('A new Type of Ray'), to Professor *Otto Lehmann,* chairman of the Würzburg physical-medical society. When he returned to his wife, he greeted her with the following words:

'That's it, now all hell may be let loose!'

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* Röntgen's intentions here have been the subject of discussion. Dessauer has guessed that Röntgen wanted to shield all fluorescent light that came from the Hittorf tube's glass walls so that he could see in the blacked-out laboratory whether the cathode rays contained any component that had sufficient penetration capacity to penetrate out through the glass wall and the dark paper. The pieces of paper covered with barium platinocyanide were there to show the penetrating radiation with the help of the fluorescence.

** According to other sources, the same interviews were granted to Sir James Mackenzie-Davidson when the latter was talking to Röntgen in 1896.

Röntgen's wife was let into the secret, but she could not help being scared by the strangeness of the new rays. The discovery seemed so peculiar to him that Röntgen himself had a nagging feeling he had made some sort of mistake. The first section of Röntgen's 'preliminary' report is as follows:

> If you allow the discharge from a large Ruhmkorff to penetrate a Hittorf vacuum tube or an adequately-evacuated Lenard or Crookes device and cover the tube with fairly tightly-wrapped thin, black cartonboard, you see in a fully blacked-out room that a paper screen coated with barium platinocyanide in close proximity to the device starts to illuminate the light at the time of each discharge, to fluoresce, irrespective of whether the coated surface or the other side of the screen is turned towards the discharge apparatus. The fluorescence can still be seen at a distance of 2 metres from the device.
>
> It is easy to convince yourself that that which causes the fluorescence emanates from the discharge apparatus rather than from any other part of the circuit.

Röntgen examined the penetration capacity of radiation with everything that was available to him: paper, books, playing cards, sheets of metal, etc. He found that radiation penetrated wood and paper with ease and also thin sheets of metal without great difficulty, but that the greater the density of the irradiated substance, the smaller the penetration capacity. He found that 1.5 mm of lead was almost impenetrable, and he therefore used lead to screen off the radiation. He showed that radiation could blacken photographic film but that it was on the other hand impossible to detect it using the body's sense. He did what we now know to be a risky experiment on himself and wrote:

> The retina of the eye is insensitive to our rays; an eye that is positioned near the discharge apparatus registers nothing, despite the fact that our experience tells us that the substances in the eye must be sufficiently permeable to rays.

Where the physical properties of the rays were concerned, Röntgen found that he could not break them in prisms or when transferring them from air to another substance, and that he could not generate any surface reflection. Nor could he polarise the rays, and he found that their penetration capacity appeared to depend mainly on the density of the material. None of this indicated a relationship with light or ultraviolet radiation.

On the other hand, the shadow formation showed that the rays were propagated in straight lines. The fluorescent light and blackening of the film showed that they transported energy. Röntgen wondered if it could possibly be a question of longitudinal electromagnetic waves, something which no-one so far (nor afterwards) had heard of.

Röntgen continued his in-depth studies of the remarkable rays. In a second report in March 1896, he told how he had examined the capacity of the rays to ionise air (which was an important discovery). He had also shown that cathode rays do not specifically need to collide with glass in order to generate the new rays. These rays seemed to arise wherever the cathode rays collided with matter.*

Röntgen's third report (of May 1897) had a new title (the previous one had quite straightforwardly been called 'continuation'): 'Further observations of the properties of x rays'. Röntgen's modesty prevented him from that talking about 'Röntgen radiation', despite the fact that this is what the new rays were already called in many countries. He had said right from the start that 'For the sake of

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* According to Maxwell, no electromagnetic waves emanate purely because electric charges are moving (e.g., from a live wire carrying direct current), but only when charges' state of motion is *changed*. At the time of Röntgen's discovery, J.J. Thomson had not yet definitively demonstrated the electron, but it was generally presumed that the cathode rays consisted of electric charges in motion. Röntgen rays emanated in this manner when the cathode rays' electrons were retarded in matter. The change in motion is faster than the acceleration of the electrons in the vacuum tube. Röntgen radiation that arises in this manner is therefore called *bremsstrahlung radiation* (German for 'braking radiation'). Braking radiation was what Röntgen studied.

simplicity, I will refer to this phenomenon as the x ray since I do not know its precise characteristic. 'X rays' is still the name that is commonly used in English literature.*

On the other hand, in German, as in Swedish, it is referred to as 'röntgen radiation'. At the Würzburg physical-medical society's meeting on 23 January 1896, a proposal had been put forward to say 'Röntgen rays' instead of x rays. The proposal was received with jubilation. The minutes of the meeting noted that the new rays would probably be very useful in medicine. Röntgen's comment on this was that radiation from available tubes had insufficient penetration capacity if you wanted to analyse body parts other than arms and legs. The production of better tubes was therefore needed.

In his third article, Röntgen gave an account of his observations on the scattering of Röntgen radiation. He was also able to show that the more metal sheets or 'filters' the radiation has passed, the more its penetration capacity increases. He showed that this was because the first filter sheets completely absorbed the 'softest', i.e., the least penetrable components of the radiation, so that which remained became 'harder' - in other words, a greater penetration capacity was achieved. Röntgen had thereby also shown that bremsstrahlung radiation was non-uniform and made up of components that have different penetration capacities.

As a basis for his three reports, Röntgen had over a short period examined practically everything essential that there was to say about Röntgen rays – an admirable experimental undertaking. Given that he studied his radiation for a somewhat limited period and, for technical measurement reasons was careful to screen off all unnecessary radiation, he was also able to avoid the harmful effects of radiation.

Röntgen was awarded the first Nobel Prize in physics in 1901. At that time, he had been a Professor in Munich and spent the rest of his life until his death in 1923 in teaching rather than research. He refused to apply for a patent for his discoveries and lived an unassuming life without actually having any financial earnings from his epochal discovery.

* A few tips may be appropriate here. Nowadays, the word 'x rays' is written in lower-case (except for in headings and at the start of sentences). A hyphen after the x must be used only when the word is used as an adjective. You therefore write 'Röntgen discovered x rays' but 'The hospital uses x-ray equipment'.

7. THE X-RAY PIONEERS

The speed at which Röntgen's discovery spread throughout the world is unparalleled. We should remind ourselves that Röntgen submitted his preliminary report to the Würzburg physical-medical society on 28 December 1895 and that he presented the discovery verbally in a lecture to the same society on 23 January 1896. On the same day, *Arthur Stanton* published the first translation of Röntgen's report in English in *Nature* magazine.

Prior to this, Röntgen himself had posted copies of his report to a number of colleagues as a New Year's greeting on 2 January 1896, including Friedrich Kohlrausch in Göttingen, Henri Poincaré in Paris and Sir Arthur Schuster in Manchester. Another recipient was his fellow student from Switzerland, Professor of Physics *Franz Serafin Exner* in Vienna, who showed his friends the innovation. Among these was physicist *Ernst Lecher* (1856-1926), whose father was the editor of *Die Freie Presse.*

This was how the first printed report of Röntgen's discovery became available to read in *Die Freie Presse* on Sunday 5 January 1896. The article had already reached other European capital cities by Monday 6 January. A telegram was sent from the *London Standard* newspaper over a great distance; one reached the USA on the Tuesday and was reproduced in a number of American newspapers on Wednesday 8 January.

Initially, scientific magazines had nothing else to go on except for these announcements of the innovation, which put the poor editors in an awkward position. If the innovation was correct, the discovery was so sensational that it would be extremely detrimental not to include it immediately. If on the other hand the innovation was unfounded, which innovations of this type usually turned out to be, it would be stupid to risk publishing it. The innovation was therefore referred to with reservations. In some cases, people were more daring and directly quizzical, however. The editor of London magazine *The Electrician* took the liberty of doubting that anyone would want to sit and have a photograph taken that would show nothing other than a skeleton and rings. (A detailed description of this interesting period has been stated in *E.R.N. Grigg's* history of the development of radiology, *The Trail of the Invisible Light*, to which those who are interested in the details can refer.)

The innovation also spread rapidly to Sweden.

Senior master in mathematics and physics at the Gothenburg Real Grammar School, *C.A. Mebius*, was one among many who repeated Röntgen's experiments out of curiosity. For his 80th birthday in 1934, Mebius wrote the following account in *Nya Dagligt Allehanda* on 13 November:

> At the start of 1896, senior master P.G. Laurin*, now Director General, and I decided to attempt to produce Röntgen rays. The experiments were performed at the Real Grammar School in Gothenburg. The only source of electricity available to us was a Holtz influence machine. Röntgen tubes were not available to buy at the time. During my time in Uppsala, I had learned to blow glass and therefore fashioned a

* *Paul Johan Gerhard Laurin* (1863-1935) was a senior master in Gothenburg and a textbook author before he became Director General and head of the Insurance Inspectorate in 1908.

> simple mercury pump and a small Röntgen tube. After a few attempts, we also succeeded in producing Röntgen rays and we also each photographed a finger. I still have these first photographs today. One peculiarity was demonstrated in this connection in that approximately one week after the photography, we both had a burn scar which showed the first physiological effects of Röntgen rays here in Sweden.

The question as to who deliberately took the first x-ray photograph after Röntgen himself was discussed in great depth by Grigg, who leads us to believe that it was a Scot by the name of *Alan Archibald Campbell Swinton.* The latter was an engineer who kept a careful record of his experiments. Following one unsuccessful attempt on 7 January 1896, he succeeded the following day in photographing a razor blade in a cardboard case. On 13 January, he photographed his own hand. In his autobiography (1930), Swinton remembers how he showed this x-ray photograph to the Prince of Wales, who exclaimed 'How disgusting!'

In 1924 in the USA, Serbian immigrant *Michael Idvorsky Pupin* (1858-1935) maintained that he produced the first x-ray photograph two weeks after Röntgen's discovery was published in Germany and, according to Pupin, on 2 January 1896. This date is suspect, however. Grigg gives a day-by-day account of Pupin's activities and shows that the latter must have been one month out. He probably did not take the x-ray photograph until 2 February, two weeks after Röntgen's verbal presentation in Würzburg on 23 January 1896. Pupin was no fly-by-nighter: he became Professor of Electrotechnology at Columbia University in New York in 1901 and is famous for his long-distance telephony experiments.

The first x-ray photograph of a human body part in Sweden appears to have been taken in Uppsala on 16 February 1896 by the then reader in physiology who would later become Professor *Hjalmar Öhrwall* (1851-1929). The photograph showed the hand and two rings of his boss, Professor *Frithiof Holmgren* (1831-1897) and is stored at *Akademiska sjukhuset*. X-ray photographs of other objects had been taken a few weeks previously, however.

In 1896, the development progressed very rapidly. At the end of February, the scientific press reported that x-ray photographs were now being taken all over the world. It had been possible to show nails on the hands and feet of patients and x-ray photographs of mummies had also been taken.

Even if Pupin was not the first to take an x-ray photograph, he was the first to intensify the photographic image using a fluorescent screen in the path of the x rays. This was natural progress, since Röntgen had already used a fluorescent screen to demonstrate x radiation.

It was now time for the professional inventors to enter the scene. *Thomas Alva Edison* (1847-1931) is said to have examined thousands of substances in his laboratory to produce the best fluorescent screen. On 25 March 1896, Edison published in *Electrical Engineer* a design where a tungstate of calcium screen was incorporated into the front of a 'peepshow' to enable the observer to keep his eyes adapted to the dark even in light surroundings and thereby find it easier to see the image produced by the x rays on the fluorescent screen. The invention was initially called the *vitascope* but soon became known as the *fluoroscope*, a name that lives on in the English name for real-time x-ray imaging, 'fluoroscopy'.

It was thus already clear a few months after Röntgen's discovery which two methods could be used for medical x-ray images: x-ray photography ('radiography') and screen projection ('fluoroscopy'). The latter method was dangerous to both researchers and patients, however. Given that the doctor had to view the fluorescent screen during the actual x-ray irradiation, it was difficult to prevent the doctor from being exposed to radiation as well. Edison's fluoroscopy also led to many radiation injuries, particularly among observers who were fascinated by being able to observe their own hands. This soon became evident to Edison when his glass blower and assistant *Clarence Madison Dally* started to lose his hair and have severe skin damage on his hands. Edison then discontinued all experiments with the fluoroscope. Others were less cautious, however….

The following was printed in *Sala Allehanda* on 7 August 1896:

> **A victim of Röntgen rays.** An interesting contribution to the strange effects of Röntgen rays recently observed on the human body can be seen from one case that occurred recently in Berlin.
>
> On 1 July, a Berlin doctor received a 17 year-old man undergoing treatment over the space of four weeks almost once a day and twice on several days, and who was being used for experiments with Röntgen rays. Each particular session usually lasted for 5 to 10 minutes. The penetration of the chest was usually continued for a longer period since the observer was particularly interested in watching the heartbeats and movements of the diaphragm. The distance from the tube - it must be added - was always unimportant, and was often in contact with the same body. However, the heat emanating from the Hittorf tube was very slight and the young man was always clothed during the experiments.
>
> The consequences of these experiments were now as follows: the skin on the side of the face turned towards the tube became extremely red verging on a brown hue. In some places it peeled. Washing with vinegar simply led to the young man saying 'my skin turned into rags'. The red colour remained, however, although it did appear to clear slightly. On the hair-covered skin of the head, the Röntgen rays had also made themselves apparent in an unpleasant manner. An almost completely bald section formed in the temple areas, the size of a two-crown piece. In this section, the scalp was surprisingly pale. The hairs that still remain are short, thin and can easily be pulled out. Traces also show clearly on the chest following the experiments. On the back is a section the size of a plate where the epidermis has become completely loose and the underlying tissue containing many small, bleeding points is exposed. A good section of the surrounding skin is of a brown-red colour. The strange thing is that neither the young man nor the people assisting at the time of the experiments noticed any sign of an illness or else the experiments would of course have been discontinued before they were.

Nowadays, we understand that a procedure from concept or discovery to practical implementation - not to mention commercial application - is time consuming. Haven't things changed since Röntgen's discovery! We should remind ourselves that the world didn't even have any knowledge of it until one week into January 1896.

Scarcely two years later, the day before New Year's Eve in 1897, lieutenant of the British Queen's 4th Hussars, *Winston Spencer Churchill* (1874-1965), sat in the cavalry barracks in Bangalore in south-west India and wrote the preface to his first book, *The Story of the Malakand Field Force*. He had written the book during and after that summer's punitive expedition to the rebellious Pathans in the Malakand Pass in what was then the north-western corner of India, close to the border with Afghanistan at the foot of the Himalayas in what is now Pakistan. The 23 year-old Churchill had participated as a correspondent for *The Pioneer* newspaper, but he participated in the conflicts when the situation became critical.

At the start of August 1897, twenty months after Röntgen's discovery, an expeditionary force had been equipped to advance towards the upper Swat valley. Churchill describes the preparations for this operation in his book:

> Before the force started, a sad event occurred. On the 12th of August, Lieut. - Colonel J. Lamb, who had been wounded on the night of the 26th of July, died. An early amputation might have saved his life; but this was postponed in the expectation that the Röntgen Rays would enable the bullet to be extracted. The Rays arrived from India after some delay. When they reached Malakand, the experiment was at once made. It was found, however, that the apparatus had been damaged in coming up, and no result was obtained. Meanwhile mortification had set in, and the gallant soldier died on the Sunday, from the effects of an amputation which he was then too weak to stand. His thigh bone had been completely shattered by the bullet. He had seen service in Afghanistan and the Zhob Valley and had been twice mentioned in despatches.

What a powerful breeze is created by the pages of history turning over those lines, which not only describe an event from summer 1897 but which were actually also written in that same year! And how remarkable that x-ray apparatuses for medical diagnostics not only already existed, but were also there and available for use at the foot of the Himalayas just nineteen to twenty months after Röntgen's discovery! Not only that, they were not mentioned by a twenty-three year-old war correspondent as a prodigy and a new invention, but as a technical aid that it was completely natural to expect to have access to and that they had every right to be annoyed with when it failed to work!

Professor *Folke Henschen* (1881-1977) has described the pioneering period in Sweden in his book of memoirs, *Min långa väg till Salamanca* [My Long Road to Salamanca] (1957). His father, the equally well-known *Salomon Henschen* (1847-1930), was Professor of Internal Medicine at Uppsala. He was an extremely enterprising man with many interests, and there were obviously echoes of Röntgen's discovery in his laboratory. Folke Henschen writes:

> Right from an early age, I was sometimes allowed to accompany my father to the Hospital. It was great, but I had to be able to keep up. He always walked very quickly and I had to hold his little finger, half running and out of breath. We rushed up the great staircase at the Hospital, which has since been removed, and entered his laboratory at the top. It smelled very strange there, a mixture of Müller-hardened brains, Canada balsam, and ether, which was used for the freezing microtome. There was life and movement in the laboratory. In the large room, the brains were drawn on the one window and microscopied on the other. Carl Hammarberg, Albin Hildebrand and others dealt with the microtomes. A Miss Mary Green and Anna Henschen, from the older branch of our family, were technical assistants. In the small laboratory was a chair with perimeters and lots of jars containing brain.* The lower laboratory was where urine examinations took place and where there was a whole shelf of chemical reagents. I remember when graduate Israel Hedenius showed me some simple reactions with hydrogen sulphide and iron salts or silver nitrate and common salt, and I remember how surprised I was.
>
> The small laboratory on the 1st floor was partially cleared out when the x-ray apparatus was to be set up there, and that was autumn 1896. Professor Knut Ångström** helped my father to assemble the apparatus and I got to help with the connections and was soon able to take care of the small, simple and completely safe apparatus with fluorescent screen, etc. and was allowed to play with it a little. Following spring and summer, a larger apparatus was displayed at the Stockholm exhibition by caretaker Andersson for Emil Holmgren.*** Nobody thought of any protective devices and this meant that Andersson's hands were pretty much destroyed: dry, heavily peeling, fragile skin on the upper sides and dry, fragile nails. However, no cancer ever developed as it did in one of radiography's pioneers in Stockholm, Dr. Tage Sjögren, from whose fingers I had to examine removed cancerous or suspected cancerous sections on various occasions.
>
> In January 1897, Dr. Thor Stenbeck used the hospital's small, simple x-ray apparatus to take photographs of a patient with a bullet in the rear section of his brain. The bullet was removed by Lennander**** and the patient recovered quickly. This is

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* Salomon Henschen's biggest scientific achievements were in the research of the brain's functions; his successes included the discovery that the eyesight is located in the occipital lobe.

** (1857-1910) Professor of Physics and son of Anders Ångström (1814-1874), whereupon the ångström unit (ÅE) to designate wavelengths of light came into being (1 ångström unit = 10^{-10} m).

*** (1866-1922) The nephew of Frithiof Holmgren who subsequently became Professor of Histology at Karolinska institutet (the Karolinska Institute) and one of Henschen's teachers there.

**** *Karl Gustav Lennander* (1857-1908), Professor of Surgery at Uppsala University, whose reputation was that of an extremely skilled surgeon.

> probably the first case where a bullet was removed using the guidance of x-ray photographs. My father, who had examined the patient, gave a lecture on the case when he was in Moscow in 1897.

Folke Henschen himself ended up with no problems from playing with father Salomon's x-ray apparatus, although it was hardly the most suitable toy. He was still an eager, vital emeritus who was often seen at *Karolinska sjukhuset's* (Karolinska Hospital's) research laboratories in the 1970s.

As indicated in Henschen's book, *Thor Stenbeck* (1864-1914) was active with both x-ray diagnostics and radiation treatment very early on. Another pioneer mentioned by Henschen was *Tage Sjögren* (1859-1939), brother of the well-known geologist *Hjalmar Sjögren* (1856-1922). Between 1899 and 1921, Sjögren ran a private x-ray clinic in Stockholm, which would be very important to the development of Swedish radiology. Gothenburg was where the third of the Swedish x-ray pioneers, *Ivar Bagge*, worked, who from 1899 and for a very long time was the only radiologist in western Sweden.

Thor Stenbeck has to be credited as being the actual pioneer, however. He was born in Skåne in 1864, studied firstly in Lund and then came to Stockholm in autumn 1886 when he entered *Karolinska institutet*. He became med. kand. (Bachelor of Medicine) in 1889 and Licentiate of Medicine in 1895.

Stenbeck was an inventive and regular experimentalist. He had already published a description of a centrifuge that he had invented and used to separate solid constituents from urine samples for microscopic examination in the Swedish Society of Medicine's magazine *Hygeia* in 1891. This was pioneering work that aroused international interest; it may have been the first time that centrifugation had been used directly for medical purposes.

As early as February 1896 (!), Stenbeck obtained x-ray equipment for his apartment in Gamla stan (the Old Town area of Stockholm), and on 18 February he gave a lecture at the Swedish Society of Medicine entitled 'About Röntgen's discovery of x rays and the historical development thereof'. He then showed x-ray images that he himself had taken on 9 February of a wooden case containing surgical instruments. On 28 February, corresponding information was given to the Uppsala Medical Society with a lecture by Hjalmar Öhrvall who had taken an x ray of a hand wearing a ring on 16 February.

In May 1896, Stenbeck received a grant from Karolinska institutet to buy equipment, and in June that same year he received a government travel grant for a study trip abroad. Over the next few years, he began to receive more and more support from many different places. One of the things that he was able to do in 1898 was study the radiation treatment of lupus (tuberculosis of the skin) at *Finseninstitutet* (the Finsen Institute) in Copenhagen and later also in Hamburg. *Finseninstitutet* was founded in 1896 by Danish doctor and subsequent Nobel Prize recipient (in 1903) *Niels R. Finsen* (1860-1904) for the treatment of tuberculosis of the skin using ultraviolet radiation from arc lamps ('daylight lamps').

As shown by Folke Henschen's recollections, Stenbeck succeeded in locating a bullet in a patient using x-ray images, which thereby enabled what may have been the first successful operation to remove a bullet from the brain. According to information, the first image was taken on 10 September 1896, something that does not tie in with Henschen's memoir stating that this took place a few months earlier.

However, the operation was performed on 2 February 1897 at *Akademiska sjukhuset* ('The Academic Hospital') in Uppsala by Professor K.G. Lennander, and Salomon Henschen gave an account of this at the 12th international medical congress in Moscow on 7-14 August 1897.

Stenbeck was also a pioneer where dental x rays were concerned, which is something that I will return to in Chapter 10.

In autumn 1897 at the Swedish Society of Medicine, Stenbeck demonstrated an x ray injury to the skin and nails of a man who had assisted with the demonstration of x-ray apparatuses to the public. It was in all likelihood that of caretaker Andersson who is mentioned by Henschen. If so, the injury was a result of the huge interest in the x-ray apparatus that was demonstrated at the Stockholm exhibition on 15 May to 1 October 1897.

In 1898, Stenbeck gave an account of the result of the treatment of 52 patients who had been given x-ray irradiation for chronic rheumatism of the joints. In some cases, the irradiation seemed to have been useful, but Stenbeck did not dare to draw any definite conclusions since he realised that the number of patients was far too small.

In 1899, Stenbeck and Sjögren each opened a private x-ray institute in Stockholm to carry out both x-ray examinations and radiation treatment.

Stenbeck moved into the ground floor of the Mäster Samuelsgatan 63 building, which was built in 1712 and had been reconstructed in 1899. Stenbeck took over a shop premises there with its entrance from Vasagatan and a waiting room inside the large shop window. The x-ray apparatus stood in the interior section of the premises.

This was where the activities and the world's first successful radiation treatment against cancer began on 4 July 1899. The x-ray apparatus was looked after by a tall, 23 year-old youngster who was given 25 *öre* for each treatment and who maintained this source of income until autumn 1900. The young man was born on 2 March 1876 and would, by the time of his death on 13 November 1950, be credited as 'the father of Swedish radiology'. His name was *Gösta Forssell*.

In 1902, Stenbeck moved from the building on Mäster Samuelsgatan to an apartment on Strandvägen 17. Using the ferry to Slussen, he commuted each day between the practice on Strandvägen and premises that he still held in Gamla stan, firstly on Triewaldsgränd 1 (now no. 3), and later (in 1903) on Västerlånggatan 66 and later still (in 1907) on Västerlånggatan 62. His life ended with a tragedy. When his young wife passed away in 1914, he took his own life in despair at just 50 years of age.

Sjögren, who had also prepared himself thoroughly for his radiological pursuits, was five years older than Stenbeck (and was thus born in 1859). In autumn 1898, he completed one of the world's first courses in x-ray physics in Berlin. This may have been very useful when he opened his x-ray practice in Stockholm in 1899 and encountered many technical difficulties, not to mention bureaucratic ones such as when he needed the electricity board's permission to connect his x-ray apparatus to the mains.

Sjögren seems to have had better contact with the Stockholm hospital than the more impulsive Stenbeck, who was more interested in his private practice. Sjögren, who worked more systematically, had the following words to say of Stenbeck: 'It is unfortunate that this extremely gifted man has not made a greater contribution to the development of new science'. This was probably not a particularly fair judgement. In 1901, Sjögren became consultant at the Sabbatsberg Hospital and cooperated closely with his hospital colleagues in developing radiography, which led to many new methods and good treatment results for more and more types of tumour. *Seved Ribbing*, who would go on to become an x-ray consultant in Linköping, remembers how, at a dinner in 1935, he was impressed by some of the older, suited and booted gentlemen, including Gösta Forssell, 'a head taller than everyone else with his lion's mane of thick, silver-white hair', and Tage Sjögren. He remembers that 'one was able to carefully press the remains of Tage Sjögren's left hand - the right one had been amputated due to x-ray injuries'. Sjögren passed away in 1939.

In 1900, Thor Stenbeck published a book entitled *Röntgenstrålarna i medicinens tjänst* [Röntgen Rays for Medical Purposes]. From this, we can obtain the following details of the period when x-ray laboratories were established in Sweden at the end of the 1800s

Table 7:1 Early x-ray laboratories in Sweden (according to Stenbeck)

University of Uppsala	From	Feburary	1896
The Thor Stenbeck Institute		"	"
University of Lund (physiol.)		Spring	"
The Schultzberg Inst. for Dental Surgery		December	"
Lidköping General Hospital		Early	1897
Dr. Elfström's Inst., Sundsvall		Spring	
Norrköping General Hospital		"	"
Växjö General Hospital		"	"
The Academic Hospital, Uppsala		"	1898
Kneipp Sanatorium, Borg		"	"
Tage Sjögren's Institute		June	1899
The Röntgen Institute, Stockholm		"	"
Ivar Bagge's Institute, Gothenburg		Summer	"

According to Dr. *Moritz Simon* who, in the 1920s (when he had recently become a consultant at the Sabbatsberg Hospital) did research into the early history of Swedish radiology, the list should also include the fact that Linköping General Hospital received its first x-ray apparatus in 1898 and that the General Hospitals in Eksjö, Karlstad, Malmö and Härnösand received their x-ray equipment in 1899. Thanks to Dr. Simon's post research, we are able to chart the development of radiology within the Swedish hospitals as follows (see the following page):

Table 7:2 The first x-ray apparatuses at Swedish hospitals

Hospital	First year	Diagnostics	Skin therapy	Deep therapy
Lidköping	1897	yes	-	-
Norrköping	"	yes	yes	yes
Växjö	1898	yes	some	-
Uppsala	"	yes	yes	yes
Linköping	"	yes	yes	yes
Eksjö	1899	yes	-	-
Karlstad	"	yes	yes	some
Malmö	"	yes	yes	yes
Härnösand	"	yes	-	-
Sabbatsberg, Sthlm	1900	yes	yes	yes
Nyköping	"	yes	yes	-
Serafimerlas. Sthlm	1901	yes	yes	yes
Sahlgrenska sj. Gbg	"	yes	yes	yes
Kristianstad	"	yes	yes	?
Falun	"	yes	yes	yes
Lund	"	yes	yes	yes
Visby	1902	yes	-	-
Vänersborg	1903	yes	some	-
Landskrona	"	yes	-	-
Trelleborg	1904	yes	?	-
Helsingborg	"	yes	yes	yes
Västerås	"	yes	yes	-
Jönköping	1905	yes	yes	some
Örebro	1906	yes	yes	yes
Simrishamn	"	yes	-	-
Östersund	"	yes	-	-
Halmstad	"	yes	-	-
Varberg	"	yes	some	-
Luleå	"	yes	some	some
Sala	"	yes	yes	yes
Gävle	"	yes	yes	-
Lovisas barnsj. Sthlm	1907	yes	-	-
Karlskrona	"	yes	-	-
Örnsköldsvik	"	yes	yes	-
Västervik	"	yes	yes	-
Sollefteå	"	yes	-	-
Sundsvall	1908	yes	?	?
Ängelholm	"	yes	-	-
Ersta, Sthlm	"	yes	-	-
Karlshamn	"	yes	yes	-
Vadstena	1909	yes	some	-
Borås	"	yes	-	-
Alingsås	"	yes	yes	-
Radiumhemmet, Sthlm	1910	yes	yes	yes

8. NATURAL RADIOACTIVITY

Let us return to January 1896. Professor Röntgen has just published his discovery of the remarkable rays, 'those that show the bones when the arms are photographed'[7]. Until that time, the researchers' relationship to science was roughly that which inspired Jules Verne's description of the settlers on the mysterious island. Newton and Maxwell had revealed the last secrets of nature - science now knew *all there was to know*. The only things left to do were some intellectual polishing, putting on the finishing touches, and systematising. Diderot and d'Alembert had led the way with their big *Encyclopédie* of the progress of reason. The time was now ripe for the final encyclopaedia of all knowledge. Man had achieved the objective of knowledge.

However, that was not the case... During the night of 8 November 1895, Wilhelm Conrad Röntgen had discovered the unexpected and found the key to doors that opened a view to endless depths and a completely new picture of the world. Röntgen's discovery initiated an avalanche of new ideas. Researchers the world over began to look for something that nobody could ever have imagined.

Röntgen's rays came from the glass wall of the cathode ray tube where it was hit by cathode rays and fluoresced. It was well known that some substances also showed *phosphoresence*. Fluorescence and phosphoresence are both special cases of *luminescence*, which is a collective term for the development of all types of light that do not depend on high temperature. Fluorescence means that the fluorescing substance in practice immediately emits light when some form of radiation energy is supplied, e.g., with x rays. With phosphorescence, the light continues to develop for a good while after the energy has ceased to be supplied. Phosphoresence was named after white phosphorus which shines in the dark (the word 'phosphorus' comes from the Greek *fos* = light and *foros* = carrier), although the light from phosphorus is not due to phosphoresence but to another type of luminescence, *chemiluminescence* as a consequence of slow oxidation.

If Röntgen rays were coming from a glass wall showing fluorescence, could it not be possible that substances which phosphoresced following the supply of energy, such as illumination with sunlight, was also emitting x rays? This was in any case a proposition that Professor *Henri Becquerel* (1852-1908) considered worth looking more closely into. On 20 January 1896, the renowned French mathematician and natural scientist *Henri Poincaré* (1854-1912) had talked about Röntgen's discovery in front of the French Academy of Sciences. Poincaré had a hypothesis about the origin of x rays. He thought that it may not be specifically necessary to have *cathode* rays: the fluorescence could be the important source and the way in which it arose might not be all that important.*

A number of researchers had already started to examine whether or not the phosphorescing substances could also be sources of x rays. They looked primarily at some sulphur compounds which showed phosphoresence after having been illuminated with sunlight. The results were contradictory. Some researchers claimed to have demonstrated that, after having been exposed to sunlight, these

[7] An untranslatable pun: The Swedish words for 'bones' and 'legs' are homonymois.

* The hypothesis was refuted by Röntgen himself in his second report in March that same year. He had shown that metals, which did not fluoresce, emitted x rays even more efficiently than glass.

substances blackened photographic plates through black paper which prevented normal light from reaching the plates. Others failed to produce such a result.

Among those who listened to Poincaré's presentation at the French Academy of Sciences was Henri Becquerel. He decided that same day to carry out detailed studies of phosphorescing substances to see whether they emitted x rays. He used the same method as the other researchers: he allowed the sun to illuminate the examined substance and then laid it on a glass photographic plate wrapped in black paper. Becquerel had a certain advantage over other researchers in that he had already previously studied the phosphorescence phenomenon with his father and hacd a collection of different phosphorescing substances whose properties he was well aware of. A 'clan' of physics researchers within the Becquerel family is shown at *Muséum national d'histoire naturelle* [The Museum of Natural History] which is in *Jardin des plantes* near the Seine in Paris. This is where Henri's grandfather *Antoine Becquerel* (1788-1878) and his father *Alexandre Edmond Becquerel* (1820-1891) used to work, both of whom were prominent researchers.

The phosphorescing substance that Henri Becquerel began to investigate was a uranium salt, uranyl sulphate. He exposed it to sunlight and left it to stand for a while on the photographic plate wrapped in black paper. When he then developed the plate, he found that it had turned black in the precise place where the uranium salt had lain. Because of this, he considered that he had confirmed Poincaré's hypothesis that x rays did come from the luminescing substance. Becquerel's first communication on this discovery was reported on 24 February 1896.

However, Becquerel was a painstaking researcher and wanted to repeat the experiment to eliminate any doubt. He wrapped a new photographic plate in black paper, but the sun had now retired behind the clouds so he decided to wait until later. He left the uranium salt (that had previously been illuminated but which was not now being illuminated for a second time) for a while on the wrapped-up plate, which he had placed in a table drawer. After a few days, on 1 March, he decided to develop the plate to see whether the expected x rays from the uranium salt could have continued to be emitted for a long period after it had first been irradiated by the sun. To his surprise, he now found that the plate had been very strongly blackened. His first explanation was that the x-ray emission had been going on for a long time after being illuminated by the sun but, as he continued to experiment, he found that the plate was blackened irrespective of whether or not he had exposed uranium salts to light. The uranium salt was quite simply self-radiating and emitted invisible light just like Röntgen's cathode ray tube. Becquerel reported this in *Académie des Sciences* on 2 March 1896.

The report was followed by several people and he was now continuing to work rapidly. In May, he was able to state that uranium metal also emitted the mysterious radiation, and people started calling it *Becquerel radiation*. The strange thing was that the uranium radiated without the supply of any energy and without the radiation appearing to weaken. What sort of radiation was this? Where did the energy come from? The phenomenon appeared to contradict the first law of thermodynamics, which says that the sum of all quantities of energy remains constant and that a *perpetuum mobile* or perpetual motion is inconceivable.

A number of years would pass before the phenomenon could be explained, following achievements by Ernest Rutherford and Albert Einstein, among others. However, the first major step towards that explanation had already been taken after a few years by a young Pole, *Marya Sklodowska* (1867-1934), who later became world-renowned under the name *Marie Curie or* 'Mme Curie'.

Marya was born in Warszaw as the youngest of five siblings. Her parents were teachers and, before marrying, her mother had been a director of a girls' school. Her mother died of tuberculosis when Marya was eleven years old and her eldest sister had died two years previously. Her father had difficulty coping financially but brought up his four surviving children in an unusually intellectual environment. Mr. Sklodowski was versed in natural scientific subjects such as physics and chemistry, but also in Greek and Latin and in living languages such as English, French and German and obviously also Polish and Russian.

Their poor financial situation forced the children to try to contribute with earnings from private - lessons in different subjects from mathematics to French. Marya's brother succeeded in studying to be a doctor, and one of her sisters became a successful singer. That left her other sister, Bronya, who dreamed of travelling to Paris and training to become a doctor. Marya encouraged her to actually do this, and they made an agreement. If Bronya went to Paris, Marya would apply for a position as governess and thus help both her father and Bronya and save some money for herself into the bargain. Later, when Bronya had become a doctor in Paris she would be able to repay Marya and enable her to follow suit.

And this was what happened six years later, when Bronya married a Polish doctor. Marya Sklodowska was with her sister in Paris in 1891 and started studying at the Sorbonne. She made rapid progress, becoming *licenciée dès sciences* in physics in 1893 and in mathematics in 1894.

At the start of 1894, Marya, who was now known as Marie, had been awarded a research grant to study the magnetic properties of different types of steel. She had begun her research in Professor Lippman's laboratory at the Sorbonne. *Gabriel Lippman* (1845-1921) had been Professor of Physics at the Sorbonne since 1884, and he was awarded the Nobel Prize in physics in 1908 for a method of photographic reproduction of colour based on the interference of light.

However, Marie found that there was not enough space in Professor Lippman's laboratory to accommodate the tools she needed. She happened to mention this to a Polish physicist, *Joseph Kovalski*, Professor of Physics at the University of Fribourg in Switzerland, whom she had met earlier and who was now in Paris on his honeymoon. Kovalski then thought of a French physicist who might have space available in his laboratory, and he therefore invited Marie to afternoon tea the next day. He then also invited the French physicist whose name was *Pierre Curie* (1859-1906).

Pierre Curie had never gone to school but had been taught at home where he showed great interest in physics and chemistry. He passed his *baccalauréat* at the age of 16 and his *licencié* when he turned 18. He had then been a teaching assistant in physics at the University of Mouton. After that, along with his brother, *Jacques Curie* (1856-1941) who was three years older than him, he had also performed ground-breaking experiments to investigate the electrical properties of crystals, which in 1880 led to the discovery of *piezoelectricity*, i.e., the electric polarisation that occurs if you compress or stretch certain crystals. In 1883, Pierre became an associate professor at the newly-established *École de physique et de chimie industrielle de la ville de Paris.* In 1894, he occupied himself with research into the magnetic properties of bodies at different temperatures. All in all, he was just the right man for Marie to meet.

And at the guest house where the Kovalskis were staying, Pierre and Marie met and were immediately attracted to one another. Marie had doubts about marriage, however; she had always intended to return to her father in Warszaw when she had finished her exams. In summer 1894, she returned to Poland but corresponded with Pierre from there. She sent him her photograph, which he showed his brother, the latter commenting:

'She has a very determined look about her; perhaps I should say *stubborn*.'

Pierre offered to come to Poland if she would marry him. Marie would not hear of such a sacrifice. In October, she returned to Paris but still had doubts. They did not get married until July 1895. In that same year, Pierre was able to defend a thesis on his experiments and was promoted to Professor at the Municipal School of Industrial Physics and Chemistry.

Initially, Marie and Pierre lived a secluded life and Marie took care of the household while studying for a teaching qualification which she passed in 1896. Pierre saw to his teaching and also continued his research into piezoelectricity. He was perhaps better known as a researcher outside France than in his homeland. Lord Kelvin was one of those who was very impressed by his work and corresponded with him.

When Marie had gained her teaching qualification, she was given permission by Pierre's boss, Professor Schützenberger, to work at the School of Physics and Chemistry, which was where she completed her experiments with the magnetic properties of steel in 1897.

Now that Marie was 'unoccupied', she began to show an interest in the discovery made by Henri Becquerel in the previous year. What sort of radiation *was* this that he had discovered? Marie Curie contacted Becquerel and wondered whether or not the new rays could be a suitable subject for a doctoral thesis. The proposal was accepted and Marie began her research. The first thing that needed to be established was whether there were any substances other than uranium which emitted rays.

Becquerel had observed that his mysterious radiation made gases electrically conductive through ionisation of the gas molecules. By showing and measuring the ionisation in air, it was possible to study the properties of the radiation as well as to investigate which of the substances emitted the newly-discovered 'Becquerel radiation'.

The clarification of what it was that actually took place when radiation made the air electrically conductive began at this particular point in time. Faraday had already called the molecule fragments that were transported to the electrodes during electrolysis *ions*. I mentioned earlier on that Stoney had described electric charges, 'electrons', in 1891 in what he called 'chemical atoms', or ions. In 1884, Svante Arrhenius had submitted his thesis on dissociation in saline solutions. In 1892, Lorentz had supported Stoney's theory that all matter contained electrons. Two years later, J.J. Thomson had shown that cathode rays consisted of electrons, and in 1897 he had determined the mass and electric charge of electrons. It is interesting to note that the ninth edition of Friedrich Kohlrausch's renowned physics textbook (May 1901) mentions neither 'electrons' nor 'ionisation', but does mention 'ions' in the context of electrolysis.

Already in March 1896, Röntgen had shown that his x rays made air electrically conductive. Becquerel rays had the same property; both were what we now call *ionising* radiation because they dislodge electrons from the atoms of air molecules, thereby disturbing their electrical neutrality and leaving them with a positive charge, i.e., in ion form.

Marie Curie used a measuring device which also utilised piezoelectricity. The measuring device contained a capacitor consisting of two horizontal, parallel metal plates. The radioactive test specimen was placed on the lower plate. The air between the plates then became conductive and the greater the amount of radiation emitted from the specimen, the greater the increase of the electric conductivity. The upper plate was connected to one of the pair of quadrants in a quadrant electrometer*, the deflection of which was dependent upon the current density. In order to make the measurement result independent of any changes in the sensitivity of the electrometer or the capacitance of the capacitor, Marie used a zero method. The change to the charge in the electrometer was compensated for by supplying a charge from a quartz crystal using piezoelectricity that resulted from the crystal being stressed and stretched. The stress that was required to compensate for the discharge from the electrometer was a measure of the intensity of the radiation.

Marie Curie began to examine all available elements, some of which were very rare. She then found that, as well as uranium, thorium also emitted these mysterious rays. She also found that the intensity of the radiation was completely independent from the temperature and chemical form of the uranium and thorium. The radiation capacity had to be seen purely as an atomic property.

One remarkable observation, however, was that materials containing uranium emitted more radiation than could have been anticipated, bearing in mind that their uranium content was known. This indicated that the uranium mineral. in addition to the uranium, also contained small quantities of some material that was even more radiant. Pierre Curie now also became sufficiently interested to leave his own research project to help his wife with her experiments. Together, they began to search for the unknown substance that Marie was certain would be found together with the uranium.

They based their work on *pitchblende* from Joachimsthal (Jáchymov) in Böhmen in what is now the Czech Republic. The mineral consists of 75% uranium oxide and also of silicic acid, iron, calcium, barium, copper, lead, bismuth, rare earths, and some thorium. If there was any other unknown substance to be found, the proportion of it had to be very small.

The Curies were facing a very labour-intensive task. They had to chemically separate the different constituents from large quantities of pitchblende. However, they were quickly encouraged by the fact

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* An electrometer is an instrument that was used very early on to measure an electric charge. It consists of thin metal plates (quadrant electrometer) or threads (dual-thread electrometer) which, during charging, are displaced due to the repellent coulomb forces. The displacements were observed under a microscope. It was possible to calibrate the instrument so that the displacement constituted a measurement of the charge. Alternatively, the same way in which the instrument was used by Marie Curie, it could be used as a zero instrument in a compensation procedure.

that it was possible to regularly trace the source of the radiation to specific fractions. In July 1898, they were able to report the following in the scientific journal *Comptes rendus*:

> We believe that the material which we separated from pitchblende contains a previously undescribed metal close to bismuth in its properties. If the existence of this new metal can be confirmed, we propose to call it *polonium* after the country from which one of us originates.

However, the Curies' strongly radioactive preparation, which they thought contained polonium, proved to produce only spectral lines from well-known, non-radioactive substances upon optical spectral analysis. Was there actually any polonium? We now know that the quantity of polonium needed to generate the strong ionising radiation that was observed was so unbelievably small that the only way to show it was through ionisation.

The couple tirelessly continued their chemical separations from even greater quantities of pitchblende, however. On Boxing Day of 1898, they made a new and epochal discovery, and they submitted a report about it on that same day to the French Academy of Sciences entitled *Sur une nouvelle substance fortement radioactive* ('About a new, strongly radioactive substance'), in so doing coining the word 'radioactive'. But, what was more, they had discovered *radium*.

They were still careful with their formulations. They wrote that their continued investigations had produced results that were in keeping with those previously reported but that they had also 'encountered a new substance which had properties that differed widely from the previous one'. The new substance was similar to barium, but they had reason to believe that they were talking about a completely new element for which they proposed the name 'radium'.

The quantities of material that they had succeeded in separating were far too small for conventional analyses, however, and the existence of radium therefore remained hypothetical. More of the expensive pitchblende was needed, as was more laboratory space. These were difficulties that looked as though they would prevent the completion of the research.

Marie Curie then thought of the possibility of using slag from the uranium production in Jáchymov rather than pitchblende. The uranium had been dissolved out of pitchblende through treatment with soda and sulphuric acid and the residual minerals discarded as useless slag – could this still contain radium?

They had a sample sent to them and, to the couple's delight, it was even more radioactive than pitchblende. This was in the days of Double Monarchy: during 1867-1918, the Austrian empire and the Kingdom of Hungary came together in a personal union and covered a large part of Europe. Böhmen belonged to Austria and the Curies approached the Austrian Academy of Sciences. With the help of the latter, Marie and Pierre were given several tonnes of slag for their research.

Since there was not enough space in the previous premises, the operations were moved out to a shed outside the college on rue Lhomond. Marie and Pierre then worked there from 1898 until 1902 to separate a sufficient quantity of radium and study the properties of the substance. Later on, Marie wrote (in the prelude to Pierre Curie's collective work, according to Eva Ramstedt's biography):

> This shed with dirt floor and glass roof, which offered inadequate protection against rain and which was as warm as a greenhouse in the summer and draughty and cold in the winter, is where we spent the best and happiest years of our life.

In the first year, they worked together to separate polonium and radium but then divided the work. Pierre studied the properties of radium and Marie did the dirty work by producing adequate quantities of salts of pure radium. At the Congress of Physics in Paris in 1900, she was able to present a preparation that undoubtedly had a higher atomic weight than barium and which showed new spectral lines upon optical spectral analysis. The couple were now receiving more help from younger researchers and the chemical work was done on a far greater scale. In 1902, Marie Curie had isolated one tenth of a gramme of almost pure radium chloride from tonnes of pitchblende slag. The existence of radium was no longer a hypothesis but a fact.

During this period, the radiation emitted by the radioactive substances was studied by various people. Two major questions demanded answers: what sort of radiation was this and where did the energy that enabled the radioactive substances to continuously radiate come from? Marie Curie had already put forward a hypothesis in 1900 in *Revue Scientifique* (according to Eva Ramstedt):

> In radioactive matter there prevails a condition of violent internal movement where matter is in the process of being torn apart. This being the case, radium ought to constantly be losing weight. - - - According to this theory, we must assume that the radioactive substances are not in their normal chemical form; their atoms are not stable since particles smaller than they themselves are ejected. Atom fragments are in motion. The radioactive material is subjected to a chemical transformation, which is the source of its radiation energy, but it is not a normal chemical transformation for such leaves the atom untouched. That which is transformed in the radioactive substance must be the atom itself since the radioactivity is bound to the atom.

So, did Becquerel radiation consist of expelled atom fragments? Becquerel himself was able to show that part of the radiation could be deflected using a magnetic field, exactly as with cathode rays, i.e., electrons. Another part was deflected to a significantly lesser extent. The latter radiation also had a lesser penetration capacity, reaching just 4-7 cm into air and being completely absorbed by even thin layers of solid material such as a sheet of paper. We now call this latter radiation α radiation (alpha radiation), while the radiation that behaved like high-energy cathode rays is called β radiation (beta radiation). However, it was not Becquerel who named these types of radiation, but a young physicist from New Zealand, *Ernest Rutherford* (1871-1937).

Rutherford's parents had both gone to New Zealand as children. Ernest was born in Brightwater near Nelson at the north end of the South Island. He was gifted and interested in natural science, studying for an academic qualification. A grant enabled him to continue his studies in England, and he came to Trinity College in Cambridge in 1895 where he had the privilege of being an assistant to J.J. Thomson at the Cavendish Laboratory. Thomson had taken over the directorship from Lord Rayleigh eleven years earlier at just 27 years of age. Under Thomson's management, the Cavendish Laboratory had developed into a world-renowned institution. Thomson was therefore not even 40 years old and Ernest Rutherford was just 24 when they met. Thomson was right in the middle of his groundbreaking studies of the properties of the electron.

It was quite natural that Thomson, following Röntgen's and Becquerel's discoveries, would give his young assistant the task of investigating Becquerel radiation. In 1899, Rutherford was able to report the following: 'There are at least two different types of radiation – one which is easily absorbed and which, for the sake of simplicity, we will call α *radiation*, and one which has a greater penetration capacity and which we will call β *radiation'*.

The uranium salts (radium had not yet been definitely proven in 1899 despite the Curies' observations at the end of 1898 and afterwards) also emitted a third type of radiation. In 1899, German physicist *Friedrich Oscar Giesel* (1852-1927) was able to show that it was not deflected in magnetic fields and therefore could not consist of electrically-charged atom fragments. Frenchman *Paul Villard* (1860-1934), who is thought to be the first person to have demonstrated this radiation, proposed the name of *γ radiation* (gamma radiation) on behalf of Rutherford. Early on it was found to have the same properties as x radiation.

In 1898, Rutherford had left the Cavendish Laboratory since, with the support of J.J. Thomson, he had applied for and been granted a Professorship in Physics at McGill University in Montreal. This university, which was established using funds from a tobacco dealer, had the very best reputation, and Rutherford would spend almost a decade ensuring that it became even more well-known and respected.

Rutherford employed a young Oxford student, *Frederick Soddy* (1877-1956), as his assistant. At around the turn of the century, it had been discovered that radium and thorium emitted radioactive gases, 'emanations'. Radium emanation (now called *radon*) was discovered in 1900 by *Friedrich Dorn* (1848-1916) and was studied by the Curie couple. Thorium emanation (*thoron*) had already been discovered by Rutherford in 1899, who gave Soddy the task of studying its chemical properties. Both radon and thoron proved to be noble gases, i.e., they could form no other chemical compounds. They were both radioactive. It was also found that they in turn produced new radioactive substances that became attached as a radioactive coating (in early publications, this was called 'induced activity' – a term now reserved for activity which is actually induced) on walls, tables and instruments in the room in which radium or thorium was stored. This was noticed primarily when working with radium. Marie Curie wrote despondently (according to her daughter Eve's biography):

> When one studies strongly radioactive substances, special precautions must be taken if one wishes to be able to continue taking delicate measurements. The various objects used in a chemical laboratory, and those which serve for experiments in physics, all become radioactive in a short time and act upon photographic plates through black paper. Dust, the air of the room, and one's clothes all become radioactive. The air in the room is a conductor. In the laboratory where we work the evil has reached an acute stage, and we can no longer have any apparatus completely isolated.[8]

It was soon found that neither the emanations nor the radioactive coatings were persistently radioactive as radium and thorium were. Their radiation intensity decreased with time and the reduction for radon and thoron was exponential, i.e., it always took an equal length of time for the radiation intensity to be reduced to half of what it was at the time of starting the measurement. It was also found that the same thing applied to polonium. On the other hand, the radiation intensity from the radioactive coatings did not decline equally regularly.

In 1902, Rutherford and Soddy put forward a 'transformation theory' which held that the atoms in the radioactive substances were presumed to be unstable so that, during each small unit of time (dt), there was a characteristic probability for the substance, $\lambda \cdot dt$, for each atom to decay. If there are N atoms in the sample of the substance, the number (dN) that dissociate during the time (dt) will then be:

$$dN = \lambda N dt \qquad (8.1)$$

This leads to an exponential reduction in the number of radioactive atoms (and thereby in the intensity of the radiation) so that the number of radioactive atoms N(t) after the time (t) from when the number was N_o is

$$N(t) = N_o e^{-\lambda t} \qquad (8.2)$$

The *half-life* $T_{½}$ is the time after which the number of radioactive atoms has been reduced to half. It is $T_{½} = 0.693/\lambda$ where 0.693 is the natural logarithm for 2 (i.e., ln 2). The fact that the number of *radioactive* atoms has fallen to half of what it was does not mean that there are now fewer atoms, simply that half of them have lost their radioactive property.

The half-life and the *decay constant* (λ) thereby became characteristic properties of the radioactive atomic species. It was possible to show that the more complicated decrease in radiation intensity from the radioactive coating was compatible with the individual exponential decay of several different radioactive substances which together formed a coating. It became apparent that a number of different radioactive substances were gradually formed after radon (whose radiation would then be reduced by a half-life of approximately eight days) had emanated from radium. Radium creates a *decay chain* of *daughter products* (see the end of this Chapter). The 'decay' of an atomic species means that the atoms of the substance are transformed into atoms of another atomic species.

Rutherford and Soddy had sounded the death knell for the old perception of atoms being indivisible foundation stones of the structure of matter. The alchemists' dream now appeared to be possible. Atoms could be transformed and they were not indivisible.

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[8] Translated from the French by Vincent Sheean (1943)

The nature of Becquerel rays continued to bewilder the researchers for a while. However, in 1900, Becquerel himself showed that the value of e/m, i.e., the ratio between the electric charge and the mass of the particles, was the same for the electrically-charged particles of β rays and for electrons. There was soon little doubt that β rays consisted of electrons. It also became clear that γ radiation was the same type as x radiation. But what about α radiation?

When α radiation was deflected in a magnetic field, this occurred in the opposite direction to the one in which the β radiation was deflected. Since by definition electrons have a negative electric charge, the α particles had to be positively charged. It was also deflected to a significantly lesser extent than β radiation, which meant that the mass or kinetic energy of the α particles was significantly greater.

This is where the noble gas helium comes into the picture. The existence of helium had been proposed when French astronomer *Jules Janssen* (1824-1907) found a new spectral line during a solar eclipse in 1868, whereupon English astronomer *Norman Lockyer* (1836-1920) suggested the name *helium* for the new element that may have given rise to the spectral line. The fact that helium did actually exist on Earth was not ascertained until 1895, however, when *William Ramsay* (1852-1916), Professor of Chemistry at University College London, along with William Crookes, observed the same spectral line in gas that was emitted from a uranium mineral. In the previous year, Ramsay, along with Lord Rayleigh, had discovered the noble gas argon in the atmosphere.

It was now safe to assume that helium was in some way linked to the radioactivity of radium. In 1903, Ramsay and Soddy published a model for the radium decay scheme. It was initially presumed that helium was involved in this, perhaps as an end product. Senior master P.G. Laurin's book from 1904, *Om radium och öfriga aktiva ämnen* [About radium and other active substances] gives the following description:

> When, for example, a radium atom explodes, an α particle is ejected and the residue of the atom is an atom in the emanation. When this atom residue explodes once more, an α particle is ejected again, and the residue of the atom is now an ex-emanation atom. When this explodes, both α particles and β particles are ejected and γ rays are generated, thereby forming the residue of an atom which, according to *Ramsay & Soddy's* discovery, is a helium atom. Several explosions probably occur between those during which radium's ex-emanation and helium are formed, but not much is yet known of these or the substances that are thus generated, and they are therefore not stated here.*

So, in 1904 the location of helium and the identity of α radiation were not yet clear, even though Rutherford harboured strong suspicions that α radiation was made up of helium. In 1907, he found that the e/m ratio of α-radiation particles was significantly smaller than the corresponding ratio of the positively-charged (ionised) hydrogen atom. Therefore, either the mass was significantly greater or the charge was also correspondingly smaller, which was pretty unlikely. Rutherford anticipated that the α particles were ionised atoms with an atomic weight between 2 and 4.

While this research continued, Marie Curie had managed to defend her thesis in 1903. That same year, the Nobel Prize in Physics was shared by Becquerel and the Curie couple. The latter were far too busy and tired to come to Stockholm to receive the prize, which was instead handed over to the French Ambassador by King Oscar II. Their Nobel lecture was not given by Pierre to the Swedish Academy of Sciences until 6 June 1905. Pierre wrote to a good friend about the visit to Sweden in the summer (according to daughter Eve's biography on Marie Curie):

* 'An atom of emanation' referred to a radon atom, and 'ex-emanation' referred to the atomic species that was formed when the radon decayed.

My wife and I have just made a very agreeable journey to Sweden. We were free of all care, and it was a rest for us. Anyhow there was hardly anybody in Stockholm in June and the official side of things was a great deal simplified by this fact.

Sweden is composed of lakes and arms of the sea, with a little land round about; pines, moraines, houses of red wood; it is a rather uniform landscape, but very pretty and restful. There was no night at all during the time of our journey, and an autumn *[sic]* sun shone nearly always.

Pierre Curie concluded his Nobel lecture by commenting on the hazardous properties of radium:

One may also imagine that in criminal hands radium might become very dangerous, and here we may ask ourselves if humanity has anything to gain by learning the secrets of nature, if it is ripe enough to profit by them, or if this knowledge is not harmful. The example of Nobel's discoveries is characteristic: powerful explosives have permitted men to perform admirable work. They are also a terrible means of destruction in the hands of the great criminals who lead the peoples towards war. I am among those who think, with Nobel, that humanity will obtain more good than evil from the new discoveries.

When in 1900 Pierre Curie came to hear that the German researchers Giesel and *Walkhoff* had found that the radioactive substances had physiological effects, he was quick to do experiments for himself. He intentionally exposed his arm to the radiation from radium and later reported his observations to the Academy of Sciences (according to Eve Curie):

After the action of the rays, the skin became red over a surface of six square centimetres; the appearance was that of a burn, but the skin was not painful, or barely so. At the end of several days the redness, without growing larger, began to increase in intensity; on the twentieth day it formed scabs, and then a wound which was dressed with bandages; on the forty-second day the epidermis began to form again on the edges, working toward the centre, and fifty-two days after the action of the rays there was still a surface of one square centimetre in the condition of a wound, which assumed a greyish appearance indicating deeper mortification.

I may add that Mme Curie, in carrying a few centigrammes of very active matter in a little sealed tube, received analogous burns, even though the little tube was enclosed in a thin metallic box. One action lasting less than half an hour, in particular, produced a red spot at the end of fifteen days, which left a blister similar to that of a superficial burn and took fifteen more days to cure.

These facts show that the duration of the evolution of the changes varies with the intensity of the active rays and with the duration of the action which originally excites them.

Besides these lively effects, we have had various effects on our hands during researches made with very active products. The hands have a general tendency toward desquamation; the extremities of the fingers which have held tubes or capsules containing very active products become hard and sometimes very painful; with one of us, the inflammation of the extremities of the fingers lasted about a fortnight and ended by the scaling of the skin, but their painful sensitiveness had not yet completely disappeared at the end of two months.

The biological effects were for better or for worse. Maybe the radiation could also cure diseases. Pierre Curie studied the effects of radiation on animals in cooperation with, e.g., Professor *Charles Bouchard* (1837-1915). The radiation was shown to be able to kill tumour cells and could therefore be used for radiation treatment. A technique was developed to capture the radon from the radium preparation and utilise an ampoule containing radon as the radiation source. The radiation treatment technique was called *Curietherapy* for a long time.* It was practised early on by a number of French doctors. Radium became a coveted substance that had a mysterious healing power. In the first few years, one gramme of radium commanded a price of 150 000 dollars. A radium production industry was born.

Between 1902 and 1903, Pierre Curie corresponded with American industrialists in Buffalo. They had written and requested information on the technique to purify radium. Pierre and Marie had a choice to make. Either they could describe all processes in the minutest detail (Marie's thesis was not yet finished), or they could also take out a patent on the technique and thereby obtain production rights throughout the world. The choice was an easy one for them. Taking out a patent would conflict with the spirit of science. Twenty years later, Mme Curie wrote the following about this (according to Eve Curie's bibliography):

> In agreement with me Pierre Curie decided to take no material profit from our discovery: in consequence we took out no patent and we have published the results of our research without reserve, as well as the processes of preparation of radium. Moreover, we gave interested persons all the information they requested. This was a great benefit to the radium industry, which was enabled to develop in full liberty, first in France and then abroad, furnishing to scientists, and doctors the products they needed. As a matter of fact, this industry is still using; today, almost without modification, the processes which we pointed out.
>
> The 'Buffalo Society of Natural Science' has offered me, as a souvenir, a publication on the development of the radium industry in the United States, accompanied by photographic reproductions of the letters in which Pierre Curie replied most fully to the questions asked by the American engineers.

The stir created at the start of the century following the discovery of radium had sapped the couple's strength. Marie Curie also could not believe that her husband had not been given a Professorship with appropriate resources, in spite of the fact that he was widely acclaimed throughout the world. As well as the Nobel Prize, the couple had also been awarded the prestigious Davy medal in 1902 by the Royal Society of London in England (said medal being in memory of Sir Humphry Davy, the prominent British chemist). However, Pierre's opinion was that he would set much greater store by a proper laboratory rather than acclaim and medals.

Pierre Curie was a genial researcher. Marie was intelligent and also resolute, stubborn and energetic. The two together made a very efficient pair of researchers. Unfortunately, Pierre's research run was interrupted early before he could meet all major expectations. Pierre was an unusually modest man with no ambitions at all for himself. Their daughter Eve, in her book about her mother, has the following to say of her father:

> He was devoid of all spirit of competition, and in the 'race for discoveries' he was able to endure being beaten by his colleagues without annoyance. 'What difference does it make if I didn't publish such and such a work,' he had the habit of saying, 'since somebody else has published it?'

In 1905, Pierre Curie was fairly whacked and was complaining about rheumatism. 'We still lead the same life, people very busy doing nothing of interest', he wrote (according to Eve) to a friend in the summer. 'A whole year has passed since I was able to do any work, and I have not one moment to myself. Evidently, I have not yet found the way of defending us against all this frittering away of our

* Where the treatment of skin complaints was concerned, this became a complement to what was then the completely new *Finsen technique*, introduced by the Dane Niels Finsen. Finsen used ultraviolet radiation primarily to treat tuberculosis of the skin (*lupus vulgaris*). The Finsen Institute (The Finsen Medical Light Institute) was founded in 1896 and Finsen was awarded a Nobel Prize (in medicine) in 1903, the same year as Becquerel and the Curie couple.

time, and yet it is very necessary that I should do so. It is a question of life or death from the intellectual point of view. My pains appear to come from some kind of neurasthenia rather than from true rheumatism, and I am getting better since I have been eating more suitably'.

On 19 April 1906, Pierre Curie had eaten lunch with his Professor colleagues at Hôtel des Sociétés Savantes on rue Danton. He was then going to visit his publishers, Gauthier-Villars, to discuss a correction. However, the publishers' had closed due to a strike. Pierre walked from there down the very busy rue Dauphine, sometimes walking on the pavement and sometimes on the actual road where he finally ended up behind a large vehicle which cleared the way for him. Close to the bank of the Seine at Pont Neuf he decided to cross the road. The large vehicle blocked his view and when he stepped out to cross to the other side of the road, he was surprised by two snorting horses that were pulling a heavy coach at top speed in that direction from the bridge. Pierre tried to hang onto one of the horses which stepped in such a way that he fell between the horses without being injured. The front wheel also passed him but the rear wheel crushed his skull.

Marie was to survive her husband for 28 years and make even more significant achievements. She became Professor at the Sorbonne in 1908 and was again awarded the Nobel Prize in 1911, this time for chemistry, for her chemical research into the radioactive elements. Rutherford was awarded the Chemistry Prize in 1908 for his equivalent achievements, and Soddy would receive it in 1921.

These researches led to the knowledge that there are radioactive substances in the natural surroundings whose half-life is long enough for them to have 'survived' since the Earth came into being. These substances are primarily* *uranium-238* (with a half-life of 4.56 billion years), *thorium-232* (half-life of 13.9 billion years) and *uranium-235* (0.71 billion years). When the Earth was created maybe 4.6 billion years ago, there was thus twice as much uranium-238 as there is now and approximately ninety times as much uranium-235. Of the natural uranium, the majority now is uranium-238 and approximately 0.7% uranium-235. When the Earth came into being, maybe 30% of the natural uranium was uranium-235 - the uranium in nature was then not far off nuclear weapon quality!

Each and every one of the three elements, uranium-238, thorium-232 and uranium-235, creates a *radioactive decay chain.* The most important features of these decay chains are shown on the next page (some of the intermediate, short-lived substances and less important alternative decay routes have been omitted). Half-lives that are more than 1 000 years are in bold type and those less than one hour are in italics.

The old names 'radium A', 'thorium A', etc. were used for a long time before it was fully understood that it was a matter of *isotopes* of known elements. The 'isotope' (from the Greek *iso* = equal and *topos* = place) of an element refers to atoms that occupy the same place in the periodic system, i.e., have the same chemical properties but have different atomic masses. The atomic mass is indicated using the *mass number* which is the number that follows the element names above.

* We now also know that all potassium on Earth contains *potassium-40* (half-life 1.27 billion years).

The three most important natural decay chains:

uranium-238
⇓
thorium-234
⇓
uranium-234
⇓
thorium-230
⇓
radium-226
⇓
radon-222 (radon)
⇓
polonium-218 (radium A)
⇓
lead-214 (radium B)
⇓
bismuth-214 (radium C)
⇓
polonium-214 (radium C′)
⇓
thorium-210 (radium C′)
⇓
lead-210 (radium D)
⇓
bismuth-210 (radium E)
⇓
polonium-210 (radium F)
⇓
stable lead-206

thorium-232
⇓
radium-228
⇓
actinium-228
⇓
thorium-228 (mesothorium)
⇓
radium-224
⇓
radon-220 (thoron)
⇓
polonium-216 (thorium A)
⇓
lead-212 (thorium B)
⇓
bismuth-212 (thorium C)
⇓
polonium-212 (thorium C′)
⇓
thorium-208 (thorium C′)
⇓
stable lead-208

uranium-235
⇓
protactinium-231
⇓
actinium-227
⇓
radium-223
⇓
radon-219 (actinon)
⇓
polonium-215
⇓
lead-211
⇓
bismuth-211
⇓
stable lead-207

9. THE SURPRISING ATOM

At the start of the 1900s, people began to understand that atoms - the indivisible building blocks of all matter - were not small, hard, indivisible grains. It was proven that they could emit electrons and thereby be ionised and acquire a positive electric charge. But where were the electrons hidden? The fact that there were electric charges within the seemingly neutral atom was possible since high-frequency vibrations as a consequence of the application of heat generated the radiation of light and heat which could be presumed to be electromagnetic waves.

During his experiments with cathode rays, Lenard had bombarded various elements with electrons, and in 1903 he had drawn the conclusion that matter was principally made up of vacuum. Just like in outer space, in spite of all the stars, he explained. By studying the distribution of electrons in thin metal foil, Lenard was able to estimate the approximate size of the atoms. To the extent that they were spherical, the radius would be a few ångströms.*

In 1904, J.J. Thomson speculated about an atom model according to which the electrons would be spread out like raisins in a cake (the 'plum pudding model') and subsequently about a more geometrically regular electron distribution within the atom.

Several astronomic comparisons were made, however. In 1903, a Japanese theoretical physicist, *Hantaro Nagaoka* (1865-1950), had proposed an atom model in which the electrons would hover like the rings of Saturn around a core containing the principal mass of the atom, which would then be presumed to have a positive charge. Nagaoka's model was soon forgotten, however, since it was not possible to imagine stability with electrons that had to repel one another by having the same electric charge.

In 1906, Rutherford made an interesting observation in his laboratory at McGill where he studied the deflection of α rays in magnetic fields. In order to obtain a well-defined ray, he allowed it to pass through a narrow column which he varied with regard to material, etc. He made the well-defined ray collide with a photographic film in order to be able to register the deflection once the ray had passed through a magnetic field. He then found that part of the ray had been deflected, or *spread*, more than anticipated. The α ray must have been affected by very strong electric forces in the column.

Strong forces but also large amounts of energy were clearly hidden in the atoms. Early calculations were made of the amount of energy per unit time (i.e., output) emitted by a radium preparation. It was shown that 1 g radium has an output of around 0.1 watt. Where did this energy come from? Were equivalent amounts of energy hidden in other substance atoms which could be released by whoever could conjure up the magic formula?

Sir William Crookes, the man behind the renowned discharge tube, had predicted the future already as early as 1879. At a lecture at the British Association for the Advancement of Science, in a comparison between the dualistic properties of the mass and radiation of cathode rays, he pointed out just how close the concepts of mass and energy ('force') were to one another:

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* The *ångström* unit (Å, sometimes ÅE), which was previously often used as a measurement unit for very short lengths, was 10^{-10} metres. The nanometre (10^{-9} metres) is now preferred.

> We have seen that, in some of its properties, radiant matter is as material as this table, while in other properties it almost assumes the character of radiant energy. We have actually touched the border-land where matter and force seem to merge into one another, the shadowy realm between known and unknown, which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this border-land, and even beyond; here, it seems to me, lie ultimate realities, subtile, far-reaching, wonderful.[9]

In 1903, following a calculation of the energy released, Soddy ascertained that the radioactivity of radium 'sets free more energy for a given weight than any other chemical change known. The energy of radioactive change must therefore be at least twenty-thousand times, and may be a million times, as great as the energy of any molecular change.'

Rutherford is quoted as having said that a wave of atomic changes could make the whole of the old world go up in smoke, and that 'some idiot in a laboratory could blow up the whole of the universe without understanding what he is doing'. And Soddy pointed out in a lecture to the Corps of Royal Engineers in 1904 (quoted from Richard Rhodes' *The Making of the Atomic Bomb*):

> It is probable that all heavy matter possesses – latent and bound up with the structure of the atom - a similar quantity of energy to that possessed by radium. If it could be tapped and controlled what an agent it would be in shaping the world's destiny! The man who put his hand on the lever by which a parsimonious nature regulates so jealously the output of this store of energy would possess a weapon by which he could destroy the earth if he chose.

Soddy expressed himself in a similar way in his later book, *The Interpretation of Radium* (1909). This was after having read the book written in 1914 by author *H.G. Wells* (1866-1946), a man who shared Jules Verne's capacity to predict, *The World Set Free* (1918). In this, Wells describes a future world war (set in 1956) in which the atomic weapon would be used. Atom bombs were a threat in the minds of many people long before they became a reality.

It is a remarkable coincidence that *Otto Hahn* (1879-1968), who would come to demonstrate the possibility of nuclear fission in 1939, visited Rutherford at McGill to work in his laboratory for a year. Hahn, who was a very competent chemist, had recently discovered 'radiothorium', i.e., thorium-228, an isotope of thorium in the thorium-232 decay chain.

In 1907, Rutherford returned from Montreal to England where he had been given a professorship in physics in Manchester. He was given a German physicist, *Hans Geiger* (1882-1945), as an assistant. Rutherford now began making plans for work in his new laboratory, and the identification of the particles in α radiation was given the highest priority. In 1908, Rutherford succeeded in definitively showing that α radiation consisted of ionised helium atoms.

One of the most important tasks after that was to continue studies into the scattering of α particles. This research led to remarkable discoveries, but to understand the consequences thereof, we must look at other research that was taking place simultaneously.

It is now time to introduce Einstein's theory of relativity and quantum physics to the story. In so doing, we are entering a field in which 'common sense' is no longer the guide, so physicists therefore quite simply have to postulate connections that conflict with our experience such as that described in Galilei's and Newton's concepts of the world. Until the turn of the century up to the 1900s, our experience was that of movements at moderate speeds. Not until the study of cathode rays did we gain any knowledge of particles that moved at speeds close to the speed of light. The old concept of the world does not apply to them. Another problem arose when the study of the consequences of Maxwell's equations for the connection between electric and magnetic fields began.

[9] W. Crookes: On Radiant Matter. Popular Science Monthly 16:157 (1880).

Let us begin by looking at what we may call 'Newton's concept of the world'. All prior experience had led us to believe that the laws of nature must always apply, irrespective of who observed them and where and when the observation occurred. I will illustrate this with an example.

Suppose we find ourselves on a train that is moving at a speed of v metres per second. In one of the carriage corridors we let a ball roll in the same direction as the the train is moving and measure the speed of the ball in relation to the moving system, i.e., the train. We find that the speed of the ball is now u metres per second.

Common sense tells us that an observer standing on the ground outside the train would find that that the ball, if it were visible, was moving faster than the train in relation to the ground, at a speed of ($v + u$) metres per second. Had we rolled the ball at the same speed (in relation to the train) in the opposite direction, the observer outside the train would have found that the speed of the ball in relation to the ground was ($v - u$) metres per second. Had we given the ball such a speed that $u = -v$, the observer outside the train would have thought that the ball was standing still.

We could have marked reference points inside and outside the train and introduced a timescale by starting a stopwatch such that the time $t = 0$ just as the end of the last train carriage passed a mark on the ground. We would then be able to measure the distance x (metres) along the track on the ground to a marked point further along inside the train. The distance x would of course increase with time since we presume that the train is moving forward at speed v. Just as we start the stopwatch and the time $t = 0$, we find that $x = x_0$ metres. At a later time, t seconds after we started the stopwatch, $x = x_0 + v \cdot t$ metres.

Our marks on the train have not moved in relation to the end of the train, however, so the distance there is still x_0 metres. Not just for the distance to our marked point, but for each distance within the train, x' metres counted from its end, the following applies:

$$x' = x - v \cdot t \quad \text{metres} \qquad (9.1)$$

if x is the distance that we measure outside the train, along the rails, from the point where we started the stopwatch. We have also more or less unconsciously presumed that a stopwatch on the train would show the same time as a stopwatch on the ground, i.e., $t' = t$. Assuming that the stopwatch would show different times would completely contradict common sense.

Our formula (9.1) states what we can call a 'Galilean transformation'. We can transfer coordinates for position and time in our 'fixed' system to another moving system. Common sense also tells us that the laws of nature must be the same, irrespective of whether we are viewing an event in the first or the second of the systems. This was also found to be the case as long as the speed v at which the one system moved in relation to the other was constant and small relative to the speed of light.

However, if all laws of nature applied equally to one system as they did to the other system, it would be impossible for us to determine which one was moving and which one was fixed. Most of us have doubtless sat on a train at a railway station when it has stood next to another train that has suddenly started to 'move' but have been unable to determine whether it was our train or the other one which was moving until we looked out through the windows on the other side and found a 'fixed' point, maybe in the form of a station building. This impossibility to decide what is at 'complete rest' and what is moving can be called 'Newton's theory of relativity'. This theory does not contradict common sense, but maybe it does contradict old preconceptions.

My example concerned two systems, the moving train and the stationary ground. That which is 'mobile' or 'stationary' is arbitrary. For us, it is of course easiest to consider the ground to be stationary, since the whole globe is full of rock, roads, buildings and plants which do not move in relation to one another and therefore together form a practical reference system. If on the other hand we look at the Earth from a broader perspective, it is more rational to say that the Earth moves around the sun rather than the other way round. The sun stakes its claim as our fixed reference point purely and simply through its size, although there was once a blasphemous thought that the Earth was not the fixed point of existence. If on the other hand we compare our solar system with others in the universe, it becomes more difficult to maintain that anything is immovable and is thereby a natural reference point.

The monumental search for a fixed point in existence lasted a long time, however, and the *ether concept* offered such a reference for several hundred years. From the time when Huygens had imagined light to be a wave in the 1600s there had been speculations about a medium, the *ether*, which was thought to fill outer space and through which the waves could propagate. Maxwell had taken it for granted that the ether existed.

However, if the ether did exist, while moving around the sun the Earth had to be moving through the ether and therefore be exposed to an 'ether wind' in the same way that an object which moves through the air is exposed to a breeze from the air. Here was a possibility for experiments. One such had been carried out in 1887 by the two American physicists *Albert Michelson* (1852-1931) and *Edward Morley* (1838-1927). Michelson had already attempted to prove the existence of the ether wind in 1881, but it was the experiment that he performed in 1887 along with Morley which put him in the history books. If the ether wind existed, light ought to move at the speed of light, c, perpendicular to the direction of movement of the Earth at speed c-v in its direction of movement and at speed c+v in the opposite direction if the Earth were expected to move at speed v through the ether. The speed of the Earth around the sun is approximately 30 kilometres per second, which is $1/10000^{th}$ of the speed of light. The difference between c–v and c+v is therefore approximately 0.02% and ought to be possible to prove.

Michelson and Morley used a device in which a beam of light from a monochrome source of radiation was divided into two beams which travelled along different paths, perpendicular to one another, and which were reflected backwards and forwards a number of times using mirrors, and were then brought together so that they were superimposed on one another. If the waves of light were synchronised they would then intensify one another, but if their run times were different, they would have created an interference that could have been shown by an *interferometer*, an optical instrument that can be used to demonstrate interference patterns. Given that the whole device floated on mercury, it was easy to rotate so that the one beam of light could run either in the direction of the Earth or in the opposite direction. With this experimental arrangement, it would have been possible to demonstrate an ether wind with a speed of 30 metres per second, i.e., one thousandth of the Earth's speed, but no time difference was found at all between the directions. There was no ether wind.

Had there been an ether wind, the path of the light in the direction of movement of the Earth would have abbreviated in the ratio of $\sqrt{1-(v/c)^2}:1$, an abbreviation that could not be found. One conceivable explanation was that the ether was dragged by the Earth and rotated around the sun. Another explanation was put forward in 1892 by Irish physicist *Francis FitzGerald* (1851-1901), stating that the ether wind exerted a pressure that led to a length contraction which accurately compensated for the anticipated shortening of the path. However, there were more convincing explanations based on studies of the consequences of Maxwell's equations.

The fact that the laws of nature are the same in both of the systems without us being able to say that either of the systems is at rest can be expressed using a mathematical term stating that the laws are *invariant* in the classical Galilean transformation. For example, the mass of a body (m) at the time of invariance is always the same in each system, and likewise the expression for its kinetic energy = ½ $m \cdot u^2$ and for its kinetic energy $m \cdot u$ if u is the speed of the body within the system.

When seeking explanations for the result of the Michelson and Morley experiment, this notion of the world was changed. It was certainly possible to show that the electric and magnetic laws took the same form irrespective of the reference system, but if you executed a Galilean transformation of space (e.g., $x' = x - v \cdot t$, $y' = y$ and $z' = z$) and time ($t' = t$), Maxwell's equations would be adapted to include terms containing the relative speed between the two systems. This would mean that the original equations were not invariant under the Galilean transformation. However, the Newtonian relativity would then not apply and it would be possible to see differences between different reference systems and, using measurements, determine which one you were in.

In order to preserve the relativity principle, you could imagine either modifying the laws for electromagnetism or abandoning the Galilean transformation. The first one proved to be practically unviable. The latter was possible on the other hand, and was shown by Dutch physicist Hendrik Lorentz in 1904, who superseded the Galilean transformation with the *Lorentz transformation*:

$$x' = \frac{x - v \cdot t}{\sqrt{1 - (v/c)^2}} \qquad (9.2)$$

$$t' = \frac{t - v \cdot x/c^2}{\sqrt{1 - (v/c)^2}} \qquad (9.3)$$

The theory had now without doubt exited the field of common sense, but as with the Lorentz transformation, Maxwell's equations were invariant and the Newtonian relativity principle (all speeds are relative) was preserved. The Lorentz transformation also explained the Michelson and Morley results. A body in relative motion is contracted in the direction of movement as described by the expression (9.2) for the distance x'. This contraction, proposed by FitzGerald, is called the *Lorentz contraction* and is a direct consequence of the endeavour to make Maxwell's equations invariant at the time of the transformation.

Equations (9.2) and (9.3) lead to a number of surprising conclusions. Even *time* is relative. Since $t' = t$ no longer holds, there is no universal time; time and space are also interwoven (distance x is part of the expression [9.3] for time). Time is a fourth dimension in addition to the three dimensions of space, something which was developed by German mathematician *Hermann Minkowski* (1864-1909).

It would fall to Albert Einstein to fully appreciate the consequences of FitzGerald's and Lorentz' proposals and incorporate the Lorentz transformation into the *special theory of relativity*. Einstein's theory of relativity has been the object of comprehensive publicity and was presented as something so complicated that prominent professors left the lecture theatre in which the theory had been presented in tears. That is a fallacy. If a professor could cry, it was doubtless that he believed he could not comprehend but had not actually tried. The principle of relativity was not a discovery by Einstein but already existed through the difficulty of looking at something such as existence at absolute rest. Nor was the relativity of time shown by Einstein – it was a consequence of Lorentz' theory. What Einstein did was to draw more far-reaching conclusions from the Lorentz transformation than those drawn by Lorentz. His argument, which I will now endeavour to depict, was revolutionary, but not actually that difficult to follow.

Albert Einstein was born in Ulm on 14 March 1879 and grew up in Munich. Stories often describe him as slightly backward at school, but they do not tell the truth. Albert had high and sometimes the highest marks in both mathematics and Latin in primary school classes and at high school. He read Kant and Darwin and mathematics under his own steam alongside his studies. He became disillusioned with religions early on when his knowledge of natural science told him that the stories in the Bible could not be strictly true. In his youth, it irritated him that, in his eyes, the State consciously misled young people with lies. His rebellious thoughts in his youth led to a permanent mistrust of authorities and a sceptical attitude to declarations and dogma.

Following the failure of his father's businesses, the family moved to Milan while 15 year-old Albert stayed in Munich to graduate from high school. This he did not do, however, but was expelled due to his rebellious nature - he loathed German discipline. He therefore went to Zurich via Milan to complete his high school studies, and he convinced his father to ask the German authorities to allow his son to surrender his German citizenship, which he did in January 1896. In 1901, he became a Swiss citizen. He constantly endeavoured to preserve something of his childhood and contradict the norms and requirements of adulthood. To a friend he later wrote:

> I sometimes ask myself how it came about that I was the one to develop the theory of relativity. The reason, I think, is that a normal adult never stops to think about problems of space and time. These are things which he has thought of as a child. But my intellectual development was retarded, as a result of which I began to wonder about space and time only when I had already grown up.

From 1902 to 1909, Einstein worked at the patent office in Zurich. That period, between the ages of 23 and 30, is when he wrote some of his most important documents. He issued three documents on

Brownian motion. This is the irregular motion of small particles, e.g., as smoke particles in air or as silted particles in a liquid. The motion can be studied under a microscope and was described for the first time in 1827 by English botanist *Robert Brown* (1773-1858).

Einstein explained Brownian motion as the result of incessant collisions of molecules in the air or the liquid in which the molecules are in thermal motion. The consequence is that the dust particles are knocked hither and thither, which explains their jerky motion and irregular pathways. For the first time there was demonstrable evidence that molecules did actually exist as free particles.

I have already mentioned Einstein's description of the photoelectric effect in an earlier Chapter. He describes what appears to be strange in that it is the incident frequency of the radiation, ν, and not its intensity which determines the energy of the electrons released by the radiation from the irradiated medium. Einstein showed that the kinetic energy E_k of electrons can be calculated as follows:

$$E_k = h\nu - W \qquad (9.4), \text{ see also } (4.7)$$

where h is Planck's constant and W is the amount of energy expended to release the electron.

However, the derivation of the *special theory of relativity* was what mainly constituted Einstein's major achievement during his years in Zurich. Later, Einstein would go on to formulate the *general* theory of relativity. Many were misled by the names and thought that the word 'special' meant that the first theory was more complicated; in fact, the word means that the theory is limited to the special case of uniform, rectilinear motion at constant speed between the observation systems. The general theory of relativity was an attempt to generalise the theory to apply to all types of motion, including accelerating systems.

Einstein pondered, as Lorentz had previously, over the lack of invariance of Maxwell's equations with the Galilean transformation. However, he was working on the basis of another hypothesis. One consequence of Maxwell's equations is that changes in the field strengths in a vacuum spread like a wave of spheres at the speed of light (c) irrespective of which system the observer is in. Furthermore, this is a hypothesis that contradicts common sense. From a light source in motion, the light, observed from a 'fixed' point, would move at speed c in all directions and not at speed $v+c$ in one direction and $v-c$ in the other direction, despite the fact that an observer who moves with the light source would also find that the light moved at speed c in all directions.

Einstein investigated the consequences of the hypothesis that the speed of light in vacuum is constant.* The hypothesis does lead to the Lorentz transformation, which Einstein thereby derived in a more elegant manner than did Lorentz.

The Lorentz transformation makes Maxwell's equations invariant, i.e., the equations take the same form in all observation systems with constant relative speed. There was now a new difficulty, however. With the Lorentz transformation, the *mechanical* laws of motion were no longer invariant. This seemed unreasonable. Mechanical motion and electromagnetic phenomena are so closely linked that it is illogical to conceive that the mechanical laws would not be invariant when the electromagnetic laws were.

You can rightly ask yourself when you can rely on logic and common sense, given that the Lorentz transformation already contradicts all common sense. Common sense and logic are two completely different things, however. 'Common sense' appeals to our senses and experiences. It is therefore not always the best guide where unknown areas are concerned. Logic on the other hand requires consistency which must always be satisfied. The Lorentz transformation contradicts common sense but is a logical consequence of individual hypotheses; in Einstein's case, just *one* hypothesis: the constancy of the speed of light.

Einstein's conclusion was therefore that the mechanical laws had to be modified so that they were invariant for the Lorentz transformation. He was able to show that this was the case if you did not

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* The speed of light (c) in vacuum has been defined since 1983 as *exactly* 299 792 458 metres per second. A new definition of the unit of length of *one metre* was introduced, i.e., as the distance that light in vacuum travels in the time of 1/299 792 458 seconds.

postulate only a length contraction but you also presumed that a body's mass increases with its speed so that

$$m = \frac{m_o}{\sqrt{1 - (v/c)^2}} \qquad (9.5)$$

where m_o is the body's *inertial mass*, i.e., the mass that can be measured when the body is not moving in relation to the observer. This expression is not that difficult to derive. You can work on the basis of an elastic collision between two particles of equal weight and apply the condition that the kinetic energy, m·v, must be preserved, irrespective of which of two coordinate systems, moving at speed *v* in relation to one another, the collisions are viewed in. The connection between the speeds of the bodies in both of the systems can be obtained using the Lorentz transformation.

It was possible to test expression (9.5) through experiments, but this required high speeds. Such speeds had been achieved for cathode rays, and experiments had already been performed to show that the mass of an electron increases with its speed. Such an increase becomes clearly noticeable when the speed of the electron exceeds half the speed of light (at half the speed of light, according to the above formula, m/m_o is 1.15, i.e., the mass has increased by 15%).

The German physicist *Walter Kaufmann* (1871-1947) devoted himself to studies of electrons with high speeds, either cathode rays or β rays from naturally radioactive preparations. In the latter case, he filtered the β radiation to produce electrons that had relatively well-defined energies. In 1901, he found that the greater the energy of the electron and thereby its speed, the smaller the *e*/*m* ratio, and he drew the conclusion that this was due to an increase in the mass rather than a reduction in the charge. Here, Einstein found data that confirmed his theory. Kaufmann as an experimenter thus played a large part in Einstein's successes, even though Einstein actually criticised Kaufmann, who was not a theorist, as having given an incorrect explanation for the observed increase in mass.

In Einstein's derivation of the expression for the dependency of mass on velocity, he had a basis from which to progress to the most spectacular postulation of the special theory of relativity. The expression for $1/\sqrt{1 - (v/c)^2}$ can be conventionally developed in the form of a series so that

$$mc^2 = m_o c^2 + \frac{1}{2} m_o v^2 + \frac{1 \cdot 3}{2 \cdot 4} m_o v^2 (\frac{v}{c})^2 + \ldots \qquad (9.6)$$

The term ½ $m_o v^2$ is the classical expression for the kinetic energy of mass. If we leave out the terms that follow this in the developmental series, we can interpret the equation as stating that the total energy of a particle of mass, mc^2, is the total of the kinetic energy plus an energy amount, $m_o c^2$, which can be interpreted as the inertia of the mass, E_o. An inert particle of mass can thus be presumed to have an inertia that is

$$E_o = m_o c^2 \qquad (9.7)$$

This was a uniquely important conclusion. If the mass is expressed in kilogrammes and the speed of light in metres per second, the energy equivalence of one kilogramme of mass is approximately $9 \cdot 10^{16}$ $kg \cdot m^2 \cdot s^{-2}$. Since by definition 1 $kg \cdot m \cdot s^{-2}$ is 1 newton, this gives an energy of $9 \cdot 10^{16}$ newton-metres (or joules or watt seconds) or 25 terawatt hours (TWh). By comparison, it is worth mentioning that a large nuclear power plant with an electrical power output of 1 000 MW produces approximately 6 TWh of electrical energy per year.

One gramme of radium was shown to develop a radiation power of 0.1 watt. During the total lifetime of radium (the effective lifetime is the half-life divided by the natural logarithm of 2, which is 1 620 years/0.693 = 2 338 years = $7.37 \cdot 10^{10}$ seconds), a total energy of $7.37 \cdot 10^9$ watt seconds (joules, newton-metres) is thus developed, or approximately 2 000 kilowatt hours per gramme - an impressive amount of energy. However, according to Einstein, only $7.37 \cdot 10^9 / 9 \cdot 10^{13}$ = approximately 80 microgrammes were expended in order to create this energy from mass. It takes radium thousands of years to incur such a small total weight loss. No wonder the energy initially looked as though it was coming from nothing!

So, this was the creation of energy. With that, the first law of thermodynamics was violated - the one stating that the sum of the energy in various forms is constant and that energy can be neither created nor destroyed. However, it could be saved if the mass of a body were also regarded as a form of energy.

The remarkable consequences of the Lorentz transformation and Einstein's special theory of relativity meant that they were received with great scepticism. Everything could be seen in terms of hypotheses and theories but with no connection to reality. Who could believe that mass and energy were different aspects of the same events, that mass increased with speed, that mobile objects were contracted and that there was no such thing as absolute time? All of this contradicted common sense and created a credibility gap between theorists and experimenters. Einstein was certainly shown respect and deference bordering on that of a personality cult, but he was also derided and ridiculed.

Another factor was that Einstein was Jewish and had surrendered his German citizenship. Among the German experimenters who were particularly bitter towards him was Philipp Lenard. The latter was also generally grieved that he had managed to discover neither x rays nor the electron before Röntgen and J.J. Thomson, despite having worked for so long and intensively with cathode rays and, so he thought, paved the way for the progress of the others. Lenard's dissatisfaction did not appear to be alleviated when he was awarded the Nobel Prize in 1905, the year before Thomson in fact, for his work with cathode rays. However, the major objections came after Einstein was awarded the 1921 Nobel Prize in physics and after the theory of relativity became more generally known (since it was possible to verify some of its consequences through experiments).

Einstein's theory of relativity has also been grossly mistreated by popular science authors, something which was invited by Einstein himself to some extent through metaphysical argumentation to illustrate the mathematical relations. This meant that he, as Professor Alfred Liljeström wrote in his *Teknikens Naturvetenskapliga Grunder* (1935), led astray 'philosophers with no mathematical schooling'. Liljeström continued: 'It is, however, impossible to popularise a mathematical theory by simplifying it – the formulae of mathematics provide the simplest and most concentrated presentation form; all paraphrases in words *have to be* incomplete and misleading'.

We will now return to Ernest Rutherford who had recently come to Manchester. He was awarded the Nobel Prize in chemistry in 1908 and was very surprised by this. It was doubtless justified by his 'investigations concerning the disintegration of the element and the chemistry of radioactive substances', but Rutherford was a physicist and considered the choice of subject to be a big joke.

With the discovery that α particles are helium atoms, Rutherford had an additional motive to continue his studies of α particle scattering. As well as Hans Geiger, a young student by the name of *Ernest Marsden* (1889-1970) was also assisting with the project. They were now trying to find a better way of registering the α particles than simply letting them blacken a photographic film. Rutherford then used a method that had previously been used by Sir William Crookes. The latter had developed a *spinthariscope*, a device consisting of a zinc sulphide screen and a magnifying lens through which you could look at the screen. When an α particle collided with the screen, luminescence caused a small beam of light (scintillation) that could be observed through the magnifier if your eye was protected from the general surrounding light.

An alternative way of registering α particles was to register electric current impulses caused by the ionisation that these generated in tubes filled with gas. Devices built on this principle were also designed by Hans Geiger under Rutherford's guidance. This later led to the *Geiger-Müller counter*, developed by Geiger and *Walter Müller*, comprising a sealed metal tube ('*G-M tube*') filled with gas at low pressure. In the middle of the tube is a metal wire with a positive electric potential of approximately 1 000 volts relative to the surrounding tube. If the ionising particles enter the tube, which for α particles requires the tube to have a particularly thin-walled 'window' somewhere through which they can penetrate, the gas is ionised whereupon the released electrons are accelerated by the electric field. Their kinetic energy can then be sufficient to enable them in turn to ionise additional gas molecules, etc. so that a pulse of electric current increases like an avalanche and can be registered through surges in a loudspeaker or in an electronic or electromechanical counter.

Individual α particles could be detected and counted using both of the above measurement methods. Once Rutherford had satisfied himself that the zinc sulphide method gave a result that was just as reliable as that using the electronic measurement method, he decided to use a zinc sulphide screen for his studies of the scattering of α radiation. Using this method, he could be more precise in establishing *where* the α particle collision had taken place.

The measurements were time-consuming and tiring. The eyes of the observer had to be adapted to the dark for half an hour before the scintillations could be observed. You could not stare at the screen for more than a few minutes at a time since your eyes became tired and the results unreliable.

Geiger and Marsden repeated Rutherford's previous experiment where the α radiation passed through a narrow column and some of the α particles were scattered slightly to the side. The observations were not directly contrary to a 'plum pudding model' of the atom with its mass evenly distributed over its volume. A few inexplicable scatterings were found, however, which caused Rutherford to spend more time thinking. In the end, he proposed a 'meaningless' experiment. He asked his colleagues to send a fine ray of α particles at a 45° angle to a sheet of thin gold foil and set up the zinc sulphide screen so that there would be a collision were the α particles to ricochet like balls against the gold foil. Everyone was convinced that there was no way this could happen – the experiment was based on the fact that all α particles would easily be able to penetrate through the thin foil. Yet his colleagues still wanted to be painstakingly careful to show even the obvious things.

The experiment was carried out by Marsden in 1909. The result was a sensation. Marsden was able to demonstrate some of the α particles in a direction which showed that they had actually ricocheted like balls against the gold foil. Even though Rutherford had intuitively known that this could occur, he was outwardly surprised. He wrote later on of his reaction (according to Richard Rhodes):

> It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you fired a 15-inch shell at a piece of tissue paper and it came back and hit you. On consideration I realised that this scattering backwards must be the result of a single collision, and when I made calculations I saw that it was impossible to get anything of that order of magnitude unless you took a system in which the greatest part of the mass was concentrated in a minute nucleus.

Geiger and Marsden published a description of the experiment a few weeks later, but Rutherford took plenty of time to think through the consequences. He built models. He hung a powerful electromagnet on a ten-metre wire in front of a fixed magnet of the same polarity on a table and studied the deviation of the pendulum when it was repelled by the fixed magnet at different speeds and angles of incidence. He drew the conclusion that a 'rebound' such as that observed by Geiger and Marsden required the positive charge of the atom to be concentrated in a volume whose radius was just one ten thousandth of that of the atom. The concentrated charge repelled the α particle with a force that, according to Coulomb's law, was proportional to the product of the charge of the atomic core and the α particle. This was the discovery of the atomic nucleus.

These speculations had taken time, however. Rutherford did not make his discovery public until 7 March 1911. His pronouncement took place before a mixed audience at a meeting of the Manchester Literary and Philosophical Society. Rutherford's lecture came after a local fruit importer had told the story of finding a rare snake in a delivery of Jamaican bananas and had shown the snake.

Rutherford's discovery was so sensational that he was *en route* to being awarded another Nobel Prize between 1922 and 1923, this time in physics. However, the Academy of Sciences realised that he had sufficient esteem and was in a position such that the Nobel Prize would scarcely have added to his illustrious renown or given him better research options.

In having demonstrated the atomic nucleus, Rutherford thought the atom consisted of a positively-charged nucleus surrounded by negative electricity, probably in the form of electrons. It was difficult to imagine that these could circle around the nucleus like Saturn's rings as Nagaoka had previously suggested. Generally speaking, no adequate physical theory was known for the way in which an atomic nucleus and electrons could share space in a stable entity. If the electrons revolved around the atomic nucleus like planets around a sun, according to Maxwell's equations they ought to emit energy through electromagnetic radiation. However, if they lost energy, they would be drawn in towards the atomic nucleus and not be able to maintain their orbits. Rutherford was the leading experimenter, but it was now time for the theorists to step in. It is therefore time to bring *Niels Bohr* (1885-1962) into the story.

Niels Bohr was born in Copenhagen in 1885. His father, *Christian Bohr* (1855-1911), was Professor of Physiology at the university where Niels studied. In 1911, Niels wrote a doctoral thesis on the electron theory of metals in which he showed that it was not possible to explain the magnetic properties by using classical physics alone. Professor *Christian Christiansen* (1843-1917), Bohr's teacher, said when the thesis was defended that there was scarcely anyone in Denmark who was adequately qualified to properly assess Bohr's work.

After his thesis had been defended, Bohr received a grant from the Carlsberg Foundation and decided to travel to Cambridge to continue his studies with J.J. Thomson. Bohr was enthusiastic about his first meeting with Thomson and about being able to study at the famous Cavendish Laboratory and participate in student life, as a football player for one thing. He wrote to his younger brother *Harald Bohr* (1887-1951), who would go on to become Professor of Mathematics in Copenhagen, about the meeting with Thomson (according to Richard Rhodes):

> If you only knew what it meant to me to talk to such a man. He was extremely nice to me, and we talked about so much; and I do believe that he thought there was some sense in what I said. He is now going to read [my dissertation] and he invited me to have dinner with him Sunday at Trinity College; then he will talk with me about it. You can imagine that I am happy…

However, it is not clear whether Thomson ever read the thesis. Bohr had been indiscreet and had talked not only about his own ideas but had also pointed out some errors in Thomson's electron theory. Whether that was the reason or whether it was due to personal chemistry we will never know, but Bohr came to feel disregarded. Things took a different turn when Rutherford travelled down from Manchester in December 1911 to participate in the traditional Cavendish dinner. Rutherford's more open and brusque personality made an impression on Bohr, and immediately afterwards he went to Manchester to meet Rutherford. Bohr asked if he could continue his studies in Manchester, and Rutherford suggested that Bohr come to him at the end of March 1912.

Bohr liked it in Manchester and the personal chemistry worked. Rutherford was a shy yet jovial and friendly man who could wander around the laboratory singing or smoking his pipe and being loud, energetic and sometimes impatient. Bohr was athletically inclined and was unbeatable at table tennis but also in mental activity. Rutherford, an experimental physicist, was mistrustful of theorists but counted Bohr as an exception: 'He plays football!'

Bohr was initiated by Geiger and Marsden in the methods for studying radioactive substances and learned radiochemistry from *George de Hevesy* (1885-1966), a subsequent winner of the Nobel Prize in chemistry in 1943. After the Second World War, de Hevesy was a welcome visitor at the Rolf Sievert institution in Stockholm where I often met him - a friendly man who always made time to talk to people, and a genuine professor who left his umbrellas behind everywhere. Bohr and de Hevesy became very good friends.

From 1913, Bohr began to show an interest in the structure of the atom. He understood very quickly through his time with de Hevesy that its chemical properties were determined by the electrons around the nucleus while the radioactive properties were linked to the atomic nucleus. Since the electrons determined the chemical properties and the number of electrons in turn depended on the number of positive electric charges in the atomic nucleus, this meant that there was a connection between atomic species and chemical properties.

Such a connection had already been demonstrated in the 1860s by Russian chemist *Dmitri Mendeleev* (1834-1907), who showed the *periodic system* of the elements. He found that the different elements could be arranged in a series according to their atomic weights, but that in such a series there was a periodicity of the occurrence of elements with similar chemical properties (e.g., fluorine, chlorine, bromine, iodine). He then broke up the long series into small sections that he positioned beneath one another so that elements with similar chemical properties came to form columns. In this way, he put together a table with a box for each element. Some of the boxes were empty but Mendeleev thought that was because corresponding elements had not yet been discovered. He was correct in assuming this, and his system has been of great help in the research looking for new elements.

Bohr had the idea that rather than simply positioning the elements in the periodic system according to their atomic weights, you could number the boxes in the system from box no. 1 (hydrogen) to box no. 92 (uranium). Maybe these atomic numbers were also equal to the number of positive charges in the atomic nucleus? That could explain the periodic recurrence of the similar chemical properties.

His friend de Hevesy pointed out that the number of radioactive substances that had now been discovered (radon, radium A, radium B, radium C, etc.) was much greater than the number of boxes available. However, Soddy had drawn the conclusion that the radioactive substances, with the exception of radium and polonium, were not new elements but variants of existing elements. They were not different in terms of chemical properties, only through different atomic weights, i.e., they had different masses but the same electric charge in the atomic nucleus. They thus occupied the same place in the periodic system and could therefore be called *isotopes* of an element.

Powerful support for Bohr's ideas would come from a young Manchester researcher, *Henry Moseley* (1887-1915). Moseley studied x-ray spectra in 1913. According to Maxwell's equations, the radiation discovered by Röntgen himself originated when an electron beam braked when it collided with matter as in the anode of an x-ray tube. This is the radiation that is called *Bremsstrahlung*, and it contains a continuous spectrum of different frequencies up to a maximum frequency that is determined by the energy of the electrons, i.e., by the voltage between the anode and cathode of the x-ray tube. In addition to this *Bremsstrahlung*, however, there is also x radiation which, as with γ radiation, exhibits well-defined frequencies. These are called *characteristic x rays*, since the frequencies are characteristic of the element in the anode of the x-ray tube. The frequency of the characteristic radiation depends on the atom number, which is usually symbolised by Z.

At the time of Moseley's research, people had started to develop methods for x-ray spectroscopy. This took place largely under the leadership of *Max von Laue* (1879-1960), who in 1912 had had the idea of using crystals to form a grid that would make spectroscopy possible. von Laue was Professor of Physics in Zurich but moved to Berlin in 1919 where he became head of the Max Planck Institute for Physical Chemistry in 1951. He was awarded the Nobel Prize in Physics in 1914.

Moseley, who would no doubt also have won a Nobel Prize had he not been snatched away at such a young age during the war, found a simple connection between the frequency of the characteristic radiation (ν) and the atomic number Z. For the radiation with the highest frequency, he found that

$$\nu = R \times (3/4) \times (Z - 1) \qquad (9.8)$$

where R is *Rydberg's constant* (see equation 4.5). Moseley was able to show that the different substances in the periodic system could be arranged according to the frequencies of the characteristic x rays such that the number Z clearly showed their order. It was Moseley who introduced the name *atomic number*. The positive electric charge of the atomic nucleus must then be presumed to be $Z \cdot e$ if e is the (negative) charge of the electron.

What Bohr did then was to collate the spectroscopic information on the electromagnetic radiation from atoms (both visible light and ultraviolet light and x rays) with Planck's theory on energy quanta, $h \cdot \nu$.

Moseley's x-ray results formally tallied with Janne Rydberg's wavenumber expression (see equation 4.5), i.e.:

$$1/\lambda = R \times (1/n^2 - 1/m^2) \qquad (9.9)$$

This is because we can multiply both terms of the equation by Planck's constant h, and substitute ν/c for $1/\lambda$ where ν is the frequency of the radiation and c is the speed of light. The product, $h\nu$, can be seen as a quantum of energy, ΔE, so that

$$\Delta E = (R \cdot c \cdot h) \times (1/n^2 - 1/m^2) \qquad (9.10)$$

Bohr then made a number of hypotheses. Firstly, his theory was that the electrons circulated like planets around the sun – the atomic nucleus. The laws of classical physics being what they were, the electron had to have a centripetal acceleration, v^2/r, if the speed of the electron were v and the orbital radius r. The centripetal force follows Coulomb's law, i.e., it is proportional to both the electric charge of the electron, e, and the charge of the nucleus, $Z \cdot e$, and inversely proportional to the square of the distance (the radius). The proportionality constant is $1/4\pi\varepsilon_o$. Since the force can also be calculated as the mass multiplied by the acceleration, we can set up an equation between these two different ways of expressing force. If the electron mass is called m, we get

$$m\,v^2/r = Z\,e^2 / 4\pi\varepsilon_o r^2 \qquad (9.11)$$

The orbital radius of the electron, r, around the atomic nucleus can be calculated from eqn. 9.11. Using the classical hypotheses of physics, all orbital radii were possible and the electron would rapidly lose energy and reduce its orbital radius by transmitting electromagnetic radiation. Bohr therefore left the classical physics alone and used Planck's quantum theory. He postulated that the only orbital radii that are permitted are those for which the *angular momentum* (kinetic energy) of the electron, $m \cdot v \cdot r$, is an integer multiple of $h/2\pi$. The integer (n) is called the electron's *principal quantum number.* Bohr's theory can be written as

$$m \cdot v \cdot r = n \cdot h/2\pi \qquad n = 1, 2, 3... \qquad (9.12)$$

This was a guess on the part of Bohr. It is possible to compare it to a wave in a tight string, e.g., one with a length of $2\pi r$. There are only a specific number of strings that are of the right length to start vibrating at a given frequency. There was certainly no wave theory for particles such as electrons as yet, but in 1924, French Duke *Louis de Broglie* (1892-1987) showed that particles of matter could be attributed to wave properties, which makes it easier to understand Bohr's hypothesis.

Bohr's hypothesis can be used to calculate the permitted electron radius by combining the expressions (9.11) and (9.12) for a given value of the principal quantum number n. It is also possible to calculate the total energy of the electron and the sum of its kinetic energy ½ mv^2 and its potential energy. The latter can be calculated using Coulomb's law. You then find that the total energy in the pathway of an electron corresponding to the principal quantum number n is

$$E_n = -\frac{me^4}{8\varepsilon_o^2 h^2} \cdot \frac{Z^2}{n^2} \qquad (9.13)$$

Bohr also presumed that the electrons, in spite of their motion around the atomic nucleus, did not lose any energy through electromagnetic radiation. Bohr's reasoning was that such energy was emitted only if the electron jumped from one pathway to another. The emitted radiation would then receive a quantum of energy ΔE equal to the difference in energy between the pathways of the two electrons, i.e.

$$\Delta E = \frac{me^4 Z^2}{8\varepsilon^2 h^2} \cdot (1/n_2^2 - 1/n_1^2) \qquad (9.14)$$

If we compare this expression with (9.10), we find that the expression is identical if Rydberg's constant can be expressed as

$$R = m \cdot e^4 / 8\varepsilon^2 h^3 c \qquad (9.15)$$

Since the value of Rydberg's constant also could be calculated on the basis of spectroscopic measurements, it was possible to confirm Bohr's hypothesis. So, now we had *Bohr's atomic model*, based on a hypothesis that conflicted with classical physics and that was apparently quite arbitrary but which was shown to give a pretty good description of reality. The structure of the atom had now been revealed, but any secrets that were hidden up the sleeve of the atomic nucleus had yet to be revealed - the nucleus in which so much energy lay dormant.

The model obviously still needed many finishing touches. The different states indicated by the principal quantum numbers n = 1, 2, 3, 4 ... are usually designated by the letters K, L, M, N ... However, it was found that the state of motion of the electron was not simply determined by the principal quantum number (n) but also by a second quantum number (l), a magnetic quantum number (m) and a spin quantum number (s). The *Pauli Principle*, named after *Wolfgang Pauli* (1900-1958), does not allow more than one electron to be in each state simultaneously. The number of permitted electrons at the K level (n=1) is then 2, at the L level (n=2) 2 if l = 1 plus 6 if l = 2, at M level (n=3), 2 if l = 1, 6 if l = 2 and 10 if l = 2, as a consequence of the possibilities offered by the other quantum numbers.

When the levels, determined primarily by the principal quantum number but also by the second quantum number, are 'saturated' with electrons, the atom is chemically inert, which is the case with helium (Z=2 and thus 2 electrons) and neon (Z=10) for example. In the neon case, levels K and L are saturated with 2 and 2 + 6 electrons.

Those who continued to reveal the pathways of electrons using spectroscopy included our Swedish Nobel Prize winner *Manne Siegbahn* (1886-1978), who was working with Janne Rydberg at the University of Lund in 1908-1923, Professor of Physics in Uppsala from 1923-1937 and head of the Academy of Science's Research Institute in Frescati outside Stockholm from 1937-1964. Siegbahn had already won the Nobel Prize in physics in 1924 for his x-ray spectroscopy discoveries.

10. RADIOLOGY IN ITS INFANCY

When it came to radiation, radioactivity and the structure of the atom, the major discoveries were made in a few places such as Paris, Cambridge, Zürich, Montreal and Manchester in the decade following Röntgen's observations in Würzburg…. However, while this was going on, the use of the new sources of radiation, primarily for medical purposes, was developing rapidly. Medical radiology had been born and was set to continue developing.

This early development included many pioneering achievements in several countries. My portrayal depicts everything from the Swedish point of view. As you will appreciate, I have a better idea of what happened in Sweden than in Argentina, Australia, Azerbaijan, Belgium, Brazil, Canada, Cuba, Denmark, Egypt, England, Estonia, Finland, France, Georgia, Greece, India, Ireland, Iceland, Italy, Japan, Java, China, Latvia, Malaya, Mexico, The Netherlands, Norway, Palestine, Poland, Puerto Rico, Portugal, Romania, Russia, Switzerland, Scotland, Spain, South Africa, The Czech Republic, Tunisia, Germany, Hungary, Uruguay, the USA, Austria - the countries or regions apart from Sweden which were said to have been represented at the 2nd international radiology congress in Stockholm in 1928.

Radiology had obviously been built up quite significantly in each and every one of these areas over the previous thirty years, a build-up that I have unfortunately been unable to study for practical reasons. The account that I am able to give is therefore just a small fraction of what has happened.

A number of pioneering achievements following Röntgen's discovery have already been mentioned, particularly Stenbeck's and Sjögren's activities in Stockholm. The first x-ray photographs were called *skiagrams* (from the Greek *skiagraphia*, a picture consisting of light and shadows), and the first skiagram in the world was a picture of Röntgen's wife's hand actually taken by himself. After this, the first x-ray photograph of a body part was probably taken by Scots engineer A.A. Campbell-Swinton on 13 January 1896. It was shown at the Camera Club in London on 16 January and is now stored at the Science Museum in London. The first skiagram that was published in a magazine also showed a hand, photographed on 17 January and printed in *New York Medical Record.*

However, these skiagrams were curious. The one with a more serious intent was the medical x-ray photo that was shown by *Toussaint Barthélemy* (1852-1906) and *Paul Oudin* (1851-1923) when Henri Poincaré reported Röntgen's discovery to the French Academy of Sciences on 20 January 1896.

Another announcement worth mentioning is that Thor Stenbeck was already using intraoral films in 1896 to take x rays of the teeth, i.e., small x-ray films that are placed in the patient's mouth behind the teeth that are to be x rayed. Way into the 1940s, the dentist held the film in the correct position himself, thereby irradiating the same fingers time after time, which gradually led to radiation damage. Nowadays, preferably no-one should have to hold the film in place, but if someone has to, it should be the patient rather than the dentist. There will then be fewer instances of fingers being irradiated which are insufficient to lead to any risk of damage.

The next trivia announcement concerns *Dr. John Macintyre* in Glasgow who attempted to take mobile x-ray pictures in 1897. He took one picture at a time of a frog and changed the position of the frog from picture to picture. The full series of pictures was then copied onto a strip of film which gave the impression of movement when played.

But not everything consisted of trivia. Many superb x-ray pictures taken by the medical profession in Stockholm in 1897 and 1898 have been reproduced in Thor Stenbeck's popular and surprising early book, *Röntgenstrålarna i medicinens tjänst* [Röntgen Rays for Medical Purposes], from 1900.

The Curie couple had already observed the biological effects of radiation, while Röntgen was so afraid of having his instruments disturbed that he provided screening against the radiation and avoided damage. The first person to systematically start examining the effect of x rays on the skin was Austrian radiologist *Leopold Freund* (1868-1943) in Vienna. He can be said to be the one who formed the basis for medical radiation treatment.

On 12 December 1896, an article by a man called *Wolfram Fuchs* was published in the American magazine *Western Electrician.* He realised that skin damage from x-ray irradiation was but a temporary evil - he had already had four - and believed that 'the damage must be seen to be insignificant compared to the good that follows from this wonderful discovery'. You could in any case reduce the risk of skin damage by following some simple recommendations given by Fuchs:

(1) Make the exposure as short as possible
(2) Do not place the x-ray tube closer than 12 inches (30 cm) to the body
(3) Rub the skin thoroughly with Vaseline and leave a layer on the section that is to be exposed.

And this was in 1896! The first two rules are still now among the most important that can be called radiation protection recommendations. Fuchs could have added that the beam ought to be shielded to prevent the irradiation of an unnecessarily large surface area and that the x-ray tube ought to be equipped with a metal filter so that the very softest radiation, which was of no value to the x-ray photography but was harmful to the skin, would be filtered out. He would then have mentioned all the essentials.

But Fuchs continued:

> The x-ray 'burn' is no more dangerous than normal burns. [...] when the x rays encounter the skull for a longer period, the hair falls out but it grows back without any unpleasant after-effects.

Fuchs was an optimist. It would have been interesting to know what happened to him as time went by.

In 1897, the British Röntgen Society was formed at no. 11 Chandos Street in London with Wilhelm Conrad Röntgen as an honorary member. The president of the society was *Silvanus P. Thompson* (1851-1916), Professor of Applied Physics and Director of the Technical College in London. Thompson had been close to discovering radioactivity before Becquerel. He had placed uranium salt on a protected photographic plate and found that the centre of the section of the plate over which the salt had lain had turned black. He informed the President of the Royal Society of this fact, whose response on 29 February 1896 was that Thompson ought to publish his observation without delay. However, Becquerel had already published *his* discovery on 24 February. We therefore now use the 'becquerel' (Bq) rather than the 'thompson' (or perhaps more likely the 'rutherford').

We find that the members of the British Röntgen Society's board include the 'frog man' Macintyre and the engineer Swinton.

1897 is when two of the major radiologists enter the scene, Frenchman *Antoine Béclère* (1856-1939) and German *Heinrich Albers-Schönberg* (1865-1921), who came to dominate European radiology for a couple of decades. Old photographs show Béclère with powerful a black moustache and a full beard, reminiscent of Röntgen. Late photographs - Béclère reached 83 years of age - show a small, graceful man with a white goatee.

Albers-Schönberg did not enjoy the privilege of living so long: he died of cancer at the age of 56, probably as a consequence of radiation damage - maybe he had more of an inclination towards technical experimentation. In photographs, he reminds you of Sven Hedin with a small, well-groomed moustache. *Svensk Uppslagsbok* contains an appreciative article about him, the final words of which read: 'He was the great key figure in radiology during its first stage of development who gathered a circle of doctors, physicists and technical assistants around him ('the School of Hamburg'). His intelligence, his talent and his amiable personality brought him great esteem everywhere'. There is not one mention of Béclère in the same edition, which shows the might of the German influence way into the 1940s.

In 1897, Béclère, who is usually known as 'the father of French radiology', started as head of the medical clinic at the Tenon Hospital in Paris, where he immediately procured an x-ray apparatus for penetration ('radioscopy'), i.e., a device where the doctor looked through a fluorescent screen in front of the patient rather than allowing the x-ray picture to manifest itself on a photographic plate ('radiography'). The year after, he took the apparatus with him to the Saint Antoine hospital where, in 1900, he set up a special x-ray laboratory with a radiologist as director.

While he was still head of the medical clinic, Béclère published an article in October 1899 called 'Radioscopy and radiography at hospitals', in which he put forward the opinion that the use of x rays for medical purposes primarily covered penetration rather than x-ray photography, the latter giving the impression of being dominant when you read all magazines containing image reproductions. The penetration should be dealt with by medics rather than radiologists, said Béclère. The medic is the one who can ask questions, investigate, palpate, tap and listen and who knows the illness, he explained. Penetration is also more economical than taking x-ray photographs. His conclusion was that 'each hospital needs to be equipped with an examination room for x-ray penetration'.

Herein lies the original explanation for the unexpected details of the French x-ray diagnostics which I would receive 58 years later when I was to write a report on radiation doses for patients on behalf of the UN. The scope of penetration in France in particular was a complete surprise. Penetration was hazardous to a doctor for a start, particularly if the latter was lacking the knowledge that a radiologist ought to have. It was easy for the primary x-ray beam to end up in body parts, and the secondary radiation was also more intensive when close to the patient.

In 1897, Albers-Schönberg, who started out as a gynaecologist, opened a private x-ray institute in Hamburg together with his colleague *G. Deycke* (1845-1940), a 'phthisiologist' (doctor specialising in pulmonary tuberculosis). In the same year, Albers-Schönberg and Deycke started the renowned magazine *Fortschritte auf dem Gebiete der Röntgenstrahlen*. 'Fortschritte' thereby became the oldest of the radiological magazines that were of any importance.*

The year after, 'The Röntgen Society of Berlin' was formed in Berlin. This society undertook to arrange a major German radiology congress in Berlin in 1905. Following the congress, a decision was made on 2 May to form an all-German x-ray society, the *Deutsche Röntgen Gesellschaft*, which would end up playing a significant role.

At a meeting of the British Röntgen Society on 1 March 1898, one of its members, *Ernest Payne,* proposed that the society form a special committee to study the problem of skin damage from x rays. The committee was formed and consisted of the Röntgen Society's chairman and secretary and three additional members, with Payne becoming the secretary of the committee. In April, the committee sent out a questionnaire to the users of x-ray apparatuses. The following information was requested:

A. Medical details

1. The types of hazardous effect.
2. Description of the objective of the irradiation.
3. Exposed body parts.
4. The condition of the patient:
   (a) Well-nourished or emaciated.
   (b) Nervous or phlegmatic.
   (c) The general condition of the patient

* The other important German magazines included *Strahlentherapie* in June 1912, with Professor *Hans Meyer* (1877-1964) as chief editor (until 1965 when he was succeeded by radiologist *J. Becker* in Heidelberg), and *Zentralblatt für die Gesamte Radiologie* in Berlin 1926.

(d) Local condition of the irradiated body part
5. Has the patient complained about heat, tingling etc. during or after the exposure?
6. Duration of effects, temporary or permanent. Comments.

B. Electrical details

7. Apparatus used, electrostatic generator or induction spool, spark length, voltage number and ampere number.
8. The shape of the tube, the distance between the ends.
9. The distance of the tube from the patient's body.
10. Number of exposures, interval between the exposures if there were several, duration of each exposure.
11. The position of the tube in relation to the body or body part, i.e., the position of the anode and cathode.
12. Which covering, if appropriate, was used:
(a) Material.
(b) Rough or smooth.
(c) Colour if the material was coloured.
13. Comments.

I have not seen any account of the responses to these questions, but there must be one in the Society's archive since an Editorial in the *British Journal of Radiology* in November 1953 described the responses as confusing, adding that 'whether due to apathy or lack of knowledge among the x-ray workers, damage and death continued to occur'.

In parallel with the development of x-ray diagnostics, attempts were made to use Röntgen rays and the radiation from radium for therapeutic purposes. The first - albeit not successful - experiments to - treat cancer with x rays appear to have been carried out by an Americans producer of discharge tubes, *Emil Herman Grubbé*, as early as 29 January 1896 when he actually had erythema from x-ray irradiation. The following day, Grubbé also attempted to treat tuberculosis of the skin using x rays.

Better results were achieved in Sweden in 1899. This was the year when Thor Stenbeck carried out the world's first successful radiation treatment of a malignant tumour, a microscopically-verified skin cancer on the nose of a 49 year-old woman. The treatment was started on 4 June 1899 and included a total of 99 lots of treatment. Gösta Forssell, who was Stenbeck's assistant at the time, was able to show off the symptom-free patient 25 years later at a meeting of the Swedish Society for Medical Radiology.

However, Stenbeck was not the first to publish results of successful radiation treatment. Tage Sjögren certainly did carry out his first radiation treatment of skin cancer later than that, but was quicker where publication was concerned. He had treated an older man with a white beard who had skin cancer on his left cheek, right beneath the eye. Sjögren complained that it was difficult to determine how powerful the radiation ought to be – one had to follow one's 'gut instinct'.

The poor penetration capacity of x rays from the apparatuses at the time meant that acute radiation damage to the skin limited its use for superficial afflictions. The use of radium as a source of radiation for radiation treatment appeared to be more promising as owing to the greater penetration capacity of gamma radiation and because small containers of enclosed radium could be placed in body cavities.

In Sweden, there was also a third pioneer within radiography, *Dr. Ivar Bagge.* He worked in Gothenburg and obtained his first x-ray apparatus in summer 1899. The fact that Bagge was unable to obtain any technical assistance in Gothenburg illustrates the difficulties that the first radiologists had to master - he was the only person on the whole of the west coast who had an x-ray tube. His new tube started off working well but soon began to go wrong. Bagge was then forced to travel to Berlin with his x-ray tube to obtain advice from the supplier.

It is now appropriate to say something about older x-ray tubes. I have obtained most of the following from an essay by Sven Benner in *SydSvensk medicinhistoriska sällskapets Årsskrift* [the Southern Swedish Medical History Society's Yearbook] in 1969.

Benner starts by referring to the comprehensive studies of electric discharges in diluted gases before Röntgen's discovery and mentions the *Geissler tube* in which it was possible to study beautiful, colour-changing light phenomena that varied depending on the type of gas, gas pressure and electrical conditions. The word 'tube', as seen later in 'x-ray tube', must not be taken too literally since the shape could take many different forms.

When Crookes discovered cathode rays, it was not yet known that they consisted of electrons but it *was* found that they could be observed only at low gas pressure (a few thousandths of a millimetre on the mercurial column). The cathode rays were found to emanate from the negative electrode in the discharge tubes and to make the glass wall at the opposite end of the tube to fluoresce. Hertz and Lenard had shown that it was possible to obtain cathode rays outside the tube if it were given an extra thin wall, e.g., a hole covered with aluminium foil where the cathode rays collided. In 1895, Röntgen's discovery showed that the glass wall or metal in the pathway of the cathode rays emitted x rays (Bremsstrahlung, i.e., 'braking radiation') when the electrons of the cathode rays were braked in the material.

During further experiments allowing Röntgen rays to form in a piece of metal inside the discharge tube (in the pathway of the cathode rays to the anode at the other end of the tube), the piece of metal was first called an anticathode and was electrically connected to the positive electrode, the anode. The original anode was gradually abandoned, and the 'anticathode' instead came to be called the anode.

Before 1913, all x-ray tubes were 'cold cathode tubes', i.e., the cathode was not heated. It consisted of an aluminium plate which was shot with positive gas ions when the gas pressure was lowered and an electric voltage was connected between the anode and the cathode. The ion bombardment shot out electrons that were able to move towards the anode with ease and where, with the braking in the anode metal, they would generate x rays. Aluminium was chosen as the material in the cathode because it had been found to be the metal that could cope best with the bombardment of positive ions.

However, where the cold cathode tubes were concerned, there was a major problem in maintaining a sufficiently low gas pressure in the x-ray tubes. If the pressure increased by the anode becoming overheated by the electron bombardment, for example, the electron current increased strongly which in turn meant that the voltage was lowered. It was then said that the tube had become 'soft' and emitted 'soft' x rays, i.e., that had a low level of penetration and that it could not generate any good x-ray pictures. It was more usual for the pressure to fall by gas being absorbed, however; the electron current was then reduced while the voltage increased. It was then said that the tube became 'hard'. 'Hard' tubes meant that the x rays had a significantly greater penetration capacity, but its intensity was lowered so longer exposure times were therefore required.

Tubes that were much too soft had to be rejected. Hard tubes could be saved by adding gas with the help of 'regeneration devices', some more ingenious than others.

The most prominent early manufacturers of equipment that could be used within radiology had experience of electromedical apparatus before Röntgen's discovery. In Germany, Siemens & Halske sold x-ray equipment as early as 1896, as did Reiniger, Gebbert & Schall in Erlangen where *Max Gebbert* (1856-1907) was the driving force. Both companies later merged to become what would come to be known as Siemens-Reiniger-Werke A.G., although *Erwin Reiniger* (1854-1909) had already left the firm by 1895.

In Hamburg, *Carl Heinrich Müller* (1845-1912) had had a glass blowing factory since 1865 which sold Crookes' and Geissler's discharge tubes from 1874 and went on to sell x-ray tubes in cooperation with the instrument firm Richard Seifert & Co., which manufactured the high-voltage spools. Müller's firm was bought out by the Dutch Philips group a while later.

In New York in 1897, father and son *Ernst* and *Robert Machlett* started a glass blowing firm which was given the name of E. Machlett & Son in 1899. In summer 1897, the first x-ray tube to be marketed in the USA was manufactured under the guidance of Robert Machlett (1872-1926). When Robert Machlett died in 1926, the production of x-ray tubes was discontinued and the firm produced laboratory glassware instead. However, Robert's son *Raymond Machlett* (1900-1955), who had patent disputes within his own firm Rainbow Light Company, was convinced to resume production of x-ray tubes in 1931, which took place within a new firm, Machlett Laboratories. The business was successful and in 1934, Machlett was able to move from New York to Springdale in Connecticut. Machlett's x-ray tubes became very popular up until the period before the Second World War when Machlett manufactured radio tubes and radar tubes on behalf of Westinghouse, thereby embarking on a new operation.

Great progress was made when it became possible to regulate the emission of the electron from the cathode by means of heating the cathode. As early as the 1880s, Edison had shown that an electric current could flow in the vacuum of a light bulb if you added an extra electrode. In 1878, Edison had started a light bulb factory that was merged with a few other companies in 1892 to form the industry giant General Electric Co., which showed an early interest in x-ray equipment.

In 1903, British physicist *Owen Richardson* (1879-1959) studied the relationship between the emission of electrons from hot bodies and their temperature. The final step was taken in General Electric's research laboratories in Schenectady in 1913 when the subsequent Nobel Prize winner *Irwin Langmuir* (1881-1957) examined the emission of electrons from warm cathodes in detail, investigated electric discharges in gases and laid the foundations for plasma physics. It enabled *William Coolidge* (1873-1975), who later became head of General Electric's laboratories, to construct the first x-ray tube with a hot cathode in 1913, the *Coolidge tube.* The importance of this cannot be underestimated - they had become independent of the gas pressure in the x-ray tube for the first time!

The Coolidge tubes worked with a much lower gas pressure than the hardest cold cathode tubes, and variations in the low gas pressure were not important. The cathode in the Coolidge tubes consisted of a wolfram wire that released electrons when heated. One of Coolidge's greatest achievements was that he succeeded in finding ways of processing the brittle wolfram metal.

Using the hot cathode meant that the electron current through the x-ray tube at sufficiently high tube voltages was determined solely by the temperature of the hot cathode, which could be adjusted using the electric current through the filament. The tube voltage, and thereby the 'hardness' of the radiation, became independent of the current through the tube. If there was access to a stable source of high voltage, it was possible to adjust current and voltage independently of one another and also to keep them constant, which an unprecedented advance!

There were a great deal of studies into the material and design of the anode. The electron current, I (ampere), which had been accelerated by the tube voltage, V (volt), supplies the anode with a kinetic energy per unit time (i.e., power) of V·I watts. Some of this power is transformed into Bremsstrahlung x rays (braking radiation) when the brakes are applied to the electrons in the anode. The fraction of the power that is taken over by the x rays can be shown to be approximately 10^{-9} Z·V.* The higher the atomic number (Z) of the anode, the greater the x-ray output. Even at 100 kV and Z = 100 (no such high atomic numbers exist in practice), the efficiency is just 1%. Almost all of the kinetic energy that the tube current transfers to the anode in the x-ray tube will be converted into heat. The anode must therefore be made of a metal with a high atomic number and a high melting point. The early choice was therefore platinum (Z = 78, melting point 1 174 ºC). In order to obtain sharp x-ray pictures, the part of the anode (focus) that emits x rays needs to be small. This means that a large amount of power is being supplied to a small volume, which may require the anode metal to have good conductivity so that the heat supplied can be rapidly diverted. However, when diverting heat, there could be problems in that the outlet through the x-ray tube sometimes became far too warm. One common solution took the shape of a small, thin wolfram plate (Z = 74, melting point 3 370 ºC), which tolerated very high temperatures, recessed in a powerful copper bar which, thanks to its good conductivity, distributed the heat over a greater volume. Additional measures can be cooling using a cooling fluid (often oil which is not electrically conductive) or a powerful air current through cavities in the anode.

If the anode became so hot that the heat was lost principally through radiation, the glass wall of the x-ray tube could become much too warm. One way of avoiding this was to increase the distance of the glass from the anode, which could make the x-ray 'tube' almost ball-shaped.

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* This means that the x-ray energy and output generated are proportional to V^2, i.e., to the square of the tube voltage.

Another way of minimising the risk of overheating was to reduce the need for x-ray output by increasing the sensitivity of the recording media using something such as double-walled x-ray film (introduced by Kodak in 1918) or intensifying screens to improve the sensitivity. Before the double-walled x-ray film became available, many radiologists placed together two x-ray plates with the emulsions against one another and were thereby able to generate the same blackening using a smaller quantity of radiation (the 'Köhler method').

The development of the intensifying screens was an arduous task. To begin with, pieces of cardboard coated with a fluorescent substance (usually calcium wolframate) were placed on both sides of the x-ray film to irradiate the film not just with x rays but also with the fluorescent light from the screens. However, the grain size of the intensifying screens was much too coarse and the afterglow period far too long. Albers-Schönberg therefore wrote in 1905 that intensifying screens were no longer of anything other than purely historical interest.

However, in 1912, calcium wolframate screens with better properties were produced in the USA by *Herbert Threlkeld-Edwards* (1870-1922). He called his screen the 'T-E screen' and announced that it was 'made by a radiographer for radiographers'. Following Threlkeld-Edwards' death, his company was taken over by *Carl Patterson*, who had already manufactured an even better screen in 1914 using cadmium wolframate and who produced a very fine-grain screen in 1916. 'Patterson Cleanable Intensifying Screens' were advertised as grainless and free from afterglow. Patterson sold the business to Du Pont in 1943.

The oldest and most primitive x-ray apparatuses obtained high-voltage electricity from electrostatic induction generators. The more commonly used induction apparatuses consisted of a high-voltage transformer with an open iron core. The primary winding consisted of a few turns of coarse copper wire that was fed with direct current, initially from an accumulator battery. The current was interrupted and closed by a hammer interrupter of the same type as those in simple doorbells. Outside the primary winding lay a secondary winding consisting of a large number of thin wire turns. When the interrupter closes the primary current in such a device, the current slowly increases to a maximum owing to self-induction. This induces a moderately high, prolonged voltage surge in the secondary winding. When on the other hand the primary current is interrupted, a high, short-term voltage surge of the opposite polarity is generated over the secondary winding.

An example of this type of induction apparatus is the previously-mentioned 'Ruhmkorff coil'. The secondary winding was connected to the x-ray tube so that the anode received a positive voltage at the time of the high interruption pulse but a negative voltage at the time of the lower closure pulse. The latter pulse was often much too low for the cold cathode tubes to be able to emit a dangerous discharge in the wrong direction but, in order to avoid such a 'misfire' in any case, a special *valve tube* was often connected in series with the x-ray tube. The valve tube was a gas discharge tube that was the easiest way to allow the current to pass in the desired direction.

As the technique developed, the simple hammer interrupters were replaced by faster interruption devices to obtain more voltage pulses per second. Many of these devices, such as the mercury interrupters and the electrolytic *Wehnelt interrupters*, were complicated and led to various difficulties.

Obtaining the voltage from a normal alternating current transformer without an interrupter was of course an attractive proposition – the transformer could then be fed directly from the electric alternating current mains. However, this was not possible with the old cold cathode tubes. On the other hand, it could work well using hot cathode tubes on condition that the anode did not become warm enough to emit electrons that would destroy the x-ray tube. Dental x-ray apparatuses are one example where it was possible to directly connect the x-ray tube to a normal high voltage transformer with a symmetrical AC voltage curve with the x-ray tube functioning as its own valve tube.

A further step towards modern x-ray equipment was taken when the use of hot cathode valves with low voltages and cold anodes began to effectively prevent surges in the wrong direction – and this now means that we have left behind the infancy of radiology.

The low average voltage over the x-ray tube meant that x rays were made up of photons, some of which had an unnecessarily low level of energy. This 'longwave' or 'soft' radiation had poor penetration capacity and needed to be filtered out. However, soft x rays also originated in the irradiated body when the primary radiation was disseminated by the body's tissues. The disseminated radiation did not travel in the same direction as the primary radiation and therefore brought with it no information that could be used for an x-ray picture – all it did was to generally and annoyingly blacken the x-ray film.

Early endeavours to reduce this annoying, spread-out radiation were based on the delimitation of the primary beam to prevent an unnecessarily large part of the body generating secondary radiation. Such delimitation was first used by Albers-Schönberg. One radical solution to the problem was shown in 1909 by *Gustav Bucky* (1880-1963) in Germany. Bucky experimented with diaphragms with three dimensional grids, approximately like the honeycombs in a beehive. Only radiation going in the right direction could flow unhindered through the grid. Bucky took out a patent on his invention in 1913.

However, the disadvantage of Bucky's secondary diaphragms was that the grid image was reproduced on the x-ray film, which disturbed matters. For that reason, the diaphragms were never popular. The American *Hollis Potter* (1880-1964) therefore developed a diaphragm between 1915 and 1917 where the grid moved so that it was not reproduced on the x-ray film. This possibility had been mentioned in Bucky's patent application, but Bucky had never used it. Similar endeavours were simultaneously made by *E.W. Caldwell* (1870-1918), although he died early on having had no practical success.

The first commercially-available model of the mobile diaphragm, which was sometimes called the 'Bucky-Potter diaphragm' and sometimes the 'Potter-Bucky diaphragm', came in 1920 and, with Kodak confirming the effectiveness of the diaphragm in 1922, it was put to general use. Bucky is the one who ought to be credited with the invention, but it was Potter who put it to practical use. Bucky had made other inventions such as a device for the automatic regulation of film camera diaphragms using a photocell. According to Mould (see reference), he was awarded a patent for it in the USA along with Albert Einstein (!). Bucky also enjoyed an achievement in another area: in his book, *Grenzstrahl-Therapie* (1928), he gave an in-depth description of a technology for the treatment of skin afflictions using very soft x rays that could penetrate out of the x-ray tube only if the latter were supplied with an easily-penetrable window. He called the soft x rays 'Grenz radiation' or 'Grenz rays' (Grenzstrahlen, i.e., 'borderline rays') since, in the spectrum of the electromagnetic radiation, it lay in the borderland between x rays and ultraviolet light. The name 'Bucky radiation' has sometimes been used. Grenz radiation had wavelengths of around 2 ångström units, and the x-ray tubes that generated it had tube voltages that were usually less than 10 kilovolts.*

After these technical digressions, let us go back to the time around the turn of the century. The American Roentgen Society was formed in the USA in the 1900s which, a few years later, would be called the American Roentgen Ray Society and whose members were mainly from the Eastern states. It was supplemented in 1915 with a Western Roentgen Society which became so popular that a new organisation was formed in 1920: the Radiological Society of North America, now the largest radiology society in the world. A more illustrious society was formed in 1923 through an initiative by Los Angeles radiotherapist *Albert Soiland*, i.e., the American College of Radiology. Being a member of this was a mark of distinction and the members could use the letters 'F.A.C.R.' to show that they were 'fellows', i.e., members, of the society.

Of greater interest from the radiation protection point of view is the formation of the National Physical Laboratory (NPL) in 1900 in England, the British National Research and Testing Institute, situated in Teddington, south-west of London. The responsibility for the scientific management of the NPL initially fell to the Royal Society, the English Academy of Sciences from the 1600s. The NPL was active within the radiation protection area along with the American National Bureau of Standards (the NBS from 1901) and the German Physikalisch-Technische Reichsanstalt (PTR). Since 1988, the NBS has been replaced by the National Institute of Standards and Technology (NIST). The German institution, which was founded as early as 1887 in Berlin on the initiative of *Werner von Siemens*

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* The relationship between the tube voltage (V) and wavelength (λ) for the part of the x radiation that is the richest in energy (i.e., the one with the shortest waveleägth) is $\lambda = 12.3/V$ if the wavelength is stated in ångström units and the voltage in kilovolts.

(1816-1892), is now called the Physikalisch-Technische Bundesanstalt (PTB) and its main activities are in Braunschweig. Its first director was physicist *Hermann von Helmholtz* and the PTR was located in Berlin-Charlottenburg before the Second World War.

Said institutions were not just involved with radiation during the first decade of the century. The NPL's activity within the field originates from 1908 when a *Mr. E. Kitto* approached the laboratory with a request for help with estimating the quantity of radium in a sample of pitchblende, although this request was turned down. The real initial involvement came in 1912 when Professor Rutherford was negotiating with the Ministry for Foreign Affairs regarding the proposed British radium standard that was to be applied to the NPL.

In Stockholm, before the turn of the century, there was a company called 'John Anderssons Electro-Tekn. Byrå' whose premises were on Klarabergsgatan 58. It had been named in 1898, but had originally been founded in 1890 by engineer *John Andersson* (1862-1939). His company set up lightning conductors and installed electric bell systems and telephones. In 1901, Andersson formed a new company, 'Åskledarkontrollanstalten'. It might be worth mentioning that in 1918, he and his wife donated 600 000 Swedish kronor to the University of Uppsala for a foundation that was subsequently used to create a professorship in electrical engineering with particular reference to atmospheric discharges or, in other words, research into lightning.

In 1899, John Andersson came by a new colleague, the recently-qualified engineer *Brother Edvard Järnh* (1879-1956). After 1901, Andersson devoted himself more and more to his lightning conductor work and transferred his company to engineer Järnh in 1902, who expanded the business to also include electrical power and lighting. The step towards x-ray apparatuses was not far away.

In Stockholm, the largest municipal hospital, Sabbatsberg, got its first x-ray equipment in 1900. It was managed by Tage Sjögren, although he did not need to come to the hospital more than two or three times a week. According to Moritz Simon's account of this early activity, the hospital management had little understanding of the difficulties with and risks of x-ray work. The hospital director wondered whether it really was necessary to have a doctor: couldn't the hospital chemist take the necessary x-ray plates?

In 1906, Gösta Forssell became head of the x-ray department at *Serafimerlasarettet* [the Seraphim General Hospital] in Stockholm, where Simon was an assistant between 1907 and 1908. Simon remembers:

> Each morning, the 4-6 x-ray tubes were checked and regenerated if necessary. They were then placed in order according to their degree of hardness. The softest of the tubes were used for fingers and other small body parts and the slightly harder ones for thicker body parts; the hardest were used for radiation treatment. When a tube was new, no regeneration was allowed until it had gradually passed from the finger stage, through the elbow and knee stages to the hip and spinal column stage. When the tube had been regenerated, it hardened faster than before and had to be made softer each day to the degree that was known to function best. Each tube was an individual item in a way that is scarcely conceivable today, and a large part of the technique consisted in selecting the best tube for each examination.
>
> [...]
>
> Each exposure had to be executed on a much more individual basis than today [this was written in 1926]. When a patient had been firmly positioned and everything was ready for exposure, a weak current was turned on and the strength of the primary current was gradually increased until the tube displayed an even light; the timer was then started and the exposure time selected in relation to the level of hardness of the tube and the strength of the light. The latter was carefully observed and, when it changed, the current density or period of exposure also had to be changed.

Bordier and Sabouraud-Noiré pastilles were used in radiation treatment. They were the chemical dosimeters in the form of compressed barium platinocyanide tablets. The dose of radiation was assessed using the change of colour in the tablets. Such chemical dosimeters were developed in the first decade of the 1900s, primarily by Frenchmen *Raymond Sabouraud* (1864-1938) and *Léonard*

Bordier (1863-1943), but also by Austrians *Robert Kienböck* (1871-1954) and *Guido Holzknecht* (1872-1931). However, chemical dosimetry was soon superseded by more sensitive and more precise measurement methods based on the capacity of the radiation to ionise air and thereby make it electrically conductive.

The radiation protection at *Serafimerlasarettet* was surprisingly good. Simon writes:

> The focus - skin distance was measured precisely and the field was covered by a 1 mm thick sheet of aluminium for all cases of deep therapy. When he instructed his assistants and students, Forssell always emphasised the principle of increasing the distance to achieve better homogeneity, which was necessary for malignant tumours and which carried less of a risk to the skin.
>
> The individual doses were made relatively large, and after each sitting, no new dose was administered to the same field until several weeks or longer had passed.
>
> Great pains were taken to generate protection against damage through involuntary or radiation that was far too powerful: most of the x-ray tube was enclosed in a case of lead glass or lead rubber. Sheets of lead were placed around the exposed field: each exposure was manipulated behind a lead screen. The method of testing the tube by penetrating the operator's own hand was strictly forbidden.
>
> No damage was known to have been caused by the electric current at the time; sparks from the tubes or the leads were often applied to our fingers to demonstrate that there was no such risk.*

The use of an aluminium plate to cover the patient in the field of irradiation necessitates a comment. It was a common measure, but Holzknecht had already criticised it in 1902 since he realised that the protective effect was insignificant. According to the knowledgeable Swedish radiation protection inspector Lars Lorentzon, there was probably a misunderstanding of the physical connection behind the method: the belief was that the electric discharges in the x-ray tube were the dangerous articles and that the aluminium plate would stop the electric field. The apparatuses and the technology used within radiology at the start of the century were described in Theseuer-Wiesner's *Kompendium d. Röntgenographie* (Leipzig, 1905), Kienböck's *Radiotherapie* (Stuttgart, 1907) and Wetterer's *Handbuch der Röntgenherapie* (Leipzig, 1908).

In parallel with this early use of x rays for the treatment of mainly superficial tumours, methods for radiation treatment with radium were developed. In Chapter 8, I mentioned Pierre Curie's interest in the biological effects of the radiation from radium. As early as 1901, the Curie couple loaned a radium preparation to *Henri Danlos* (1844-1912) at the Hôpital St. Louis in Paris, so Danlos ought to be the first person who used radium for radiation treatment.

As the demand for radium increased, so did its manufacture in various parts of the world. In 1920, Union Minière du Haut Katanga and its subsidiary Radium Belge began producing radium on a large scale. The ore was shipped from the Belgian Congo to Belgium where the radium was extracted. The business caused the price of radium to fall to 70 000 dollars per gramme. In the USA, uranium ore was mined in Colorado (Paradox Valley) and in Canada, uranium ore was found by the Great Bear Lake.

The simplest form of radium treatment was superficial contact treatment ('brachtherapy' from the Greek *brachys* = short) where a radium salt encapsulated in plates or needles was placed on the skin either directly or in an applicator to safeguard the geometry. Measurements or calculations made it possible to estimate the radiation doses at different points of the irradiated body. The first successful radium treatment of this type appears to have been carried out in St. Petersburg by *Goldberg* and *Efim S. London* (1869-1939) in 1903. However, it was not until after the appearance of calculation methods such as those described by Sievert in 1921 and the standardisation of the positioning of the radium -

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* In some countries, however, there were subsequent deaths owing to contact with the high voltage leads when the high voltage unit was capable of emitting greater amounts of energy than the first simple spools.

preparation such as in the *Manchester method* (developed by *Ralston Paterson* [1897-1981] and *Herbert Parker* [1910-1984) during the 1930s) that the dosage was brought well under control. The good results of previous treatments were fully dependent on the experience and intuition of the radiologist.

A special form of brachytherapy was the *interstitial* radium treatment where the radium was fed into body cavities. As an aside, it is worth mentioning that the inventor of the telephone, *Alexander Graham Bell* (1847-1922) proposed such treatments in a letter to *Science* magazine in 1903. The letter was part of a communication with a doctor by the name of *Sowers* and read:

> Dear Dr. Sowers,
>
> I understand from you that the Roentgen X-Rays, and the rays emitted by radium, have been found to have a marked curative effect upon external cancers, but that the effects upon deep seated cancers have not thus far proved satisfactory.
>
> It has occurred to me that one reason for the unsatisfactory nature of these latter experiments arises from the fact that the rays have been applied externally, thus having to pass through healthy tissues of various depths in order to reach the cancerous matter.
>
> The Crookes tube from which the Roentgen rays are emitted is of course too bulky to be admitted into the middle of a mass of cancer, but there is no reason why a tiny fragment of radium sealed up in a fine glass tube should not be inserted into the very heart of the cancer, thus acting direetly upon the diseased material. Would it not be worthwhile making experiments along this line?
>
> Yours sincerely,
>
> (Signed) Alexander Graham Bell

However, Bell was not the first to come up with the idea – it was probably a doctor from Munich, *H. Strebel*, who was already using this technology in the same year. The American pioneers of radium treatment who can be mentioned include *Margaret Cleaves* (1848-1917) in New York, who treated uterine cancer (*cervical cancer*) with radium preparations through intracavitary insertion in 1903. *Robert Abbe* (1851-1928) at St. Luke's Hospital in New York was the first to inject a radium preparation into the actual tumour in 1905.

Howard Kelly (1858-1943), gynaecologist at the Johns Hopkins Clinic in Baltimore, also treated cervical cancer early on (in 1904). In 1914, Kelly took the initiative of forming the National Radium Institute with the support of the US Bureau of Mines which, by means of radium from mines in Paradox Valley, purified in Denver, supplied both Kelly and the Memorial Hospital in New York with sources of radiation. 8-9 grammes of radium were produced in all, and Kelly had 5 ½ grammes at most.

Different techniques were developed for the treatment of uterine cancer (cervical cancer is cancer of the cervix, the neck of the uterus, which is significantly more common than *corpus cancer*, i.e., cancer in the body of the uterus, *corpus uteri*). The difference between the techniques could be the form of the applicator, the quantity of radium and the distribution thereof, the number of treatments and the duration thereof and their distribution over time. Major centres for this type of treatment included the Fondation Curie in Paris, Radiumhemmet in Stockholm (where *James Heyman* developed the 'Stockholm method') and Kelly's clinic in Baltimore.

In 1916, Kelly received assistance from Rutherford in setting up a radon laboratory for the production of glass ampoules containing radon. After a few hours, these contained approximately the same amount of the gamma-emitting daughter products as a radium preparation of the same strength as that used during the production of the radon preparation. The equipment was in use right up until 1959.

However, the primary user of radon preparations was New York radiologist *Henry Janeway* (1873-1921), who became known in 1920 for the method of buried emanation. He implanted glass ampoules containing radon close to a tumour where possible and left the ampoule 'buried' there. It caused no harm when the radon had practically all decayed after a few weeks (radon-222 has a half-life of 3.8 days).

Janeway's method did have its disadvantages, however. The irradiation that penetrated through the glass generated radiation doses that were much too high when close up to the ampoules, with worrying death of tissue as a consequence. Janeway's radiophysicist, *Gioacchino Failla* (1890-1960) then came up with the idea of what would be known as filtered seeds: the glass ampoules were coated with a thin layer of gold which stemmed the beta radiation. Failla was a very pleasant, clever man with whom I would end up working in close cooperation in the 1950s when he played a major role within the International Radiation Protection Commission (ICRP).

The work with radium and radon gave the personnel high doses of radiation, particularly when applicators were packed with radium needles or plates. The idea of placing an empty applicator on or in the patient and not administering radium until afterwards (the 'afterloading technique') had already been applied in 1903 by Strebel and in 1906 by New York surgeon Robert Abbe. The afterloading technique allowed you to take your time with positioning the applicators correctly. The remote afterloading of radium was technically difficult, however, due to the form and fragility of the radium preparation. A good half-century therefore passed before the afterloading technique made a proper impact thanks to the possibility of adapting artificial radioactive sources of radiation to the technical requirements.

With the increase in access to radium, people began to supplement the brachyradium technique with the 'teleradium' technique i.e., remote treatment. The first devices for this purpose were called 'radium bombs' or 'radium cannons' since you 'shot' at the tumour. Owing to a certain – but still very moderate – distance, you could treat somewhat more deeply-embedded tumours, primarily in the areas that usually come under the designation of 'ear, nose and throat'. The first teleradium treatment apparatus was put to use in 1919 at the Middlesex Hospital in London and used 2.5 grammes of radium. The access to radium was limited, and the price was far more than 100 000 Swedish kronor per gramme. The earliest radium cannons therefore often consisted of a lead cylinder mounted on a support in which you could position the radium preparation that was available at the time. Such an apparatus was designed for Radiumhemmet in 1923 by *Erik Lysholm* (1891-1947), a radiologist of extraordinarily technical excellence who subsequently created a few world-renowned products: the *Lysholm grid* and the *Lysholm skull unit* for precision adjustment of the x-ray tube when examining the skull.

Later radium cannons used a cartridge loaded with special tubes containing radium, often 50 milligrammes in each tube. Given the ambition for greater protection, the device was designed so that the cartridge could be transported away from a treatment mode to a protection mode when the 'cannon' was not being used. In the radium cannons at Radiumhemmet, the protection mode container and treatment head consisted of a unit mounted on a support. They were gradually improved through new designs by Sievert and later Benner in 1926, 1933, 1937 and 1947. At the start of the 1950s, Radiumhemmet had two radium cannons for 3 and 5 grammes of radium respectively.

Greater quantities of radium made it all the more difficult to offer adequate shielding from the radium in protection mode. Some designs therefore transported the radium cartridge pneumatically through a long hose (approximately 3 metres) from the treatment apparatus to a well-shielded storage cabinet built into a wall. In the UK, such radium cannons were made by Bryant Symons. The disadvantage of this 'safer' design was that the radium cartridge occasionally got stuck *en route*, which involved extra risks for the personnel.

Radiation treatment using x rays facilitated a greater treatment distance and there were thereby fewer problems with the radiation intensity being dependent upon distance. A greater distance meant that it did not decrease as quickly towards the depth in the body according to the inverse square law. Problems instead arose with a smaller x-ray penetration capacity since the voltage of the x-ray tubes was not much more than 200-250 kilovolts (kV). Endeavours were therefore made to produce treatment apparatuses with higher tube voltages. A treatment apparatus with 750 kV of voltage was installed at the Memorial Hospital in New York in 1931.

In 1929, American Professor of Physics *Robert Van de Graaff* (1901-1967) at the Massachusetts Institute of Technology designed the high voltage generator that was named after him. The Van de Graaff generator is a belt generator in which an endless belt made of an insulating material is placed over two cylindrical rollers. One of the rolls is earthed while the other, which sits in a hollow metal globe, is isolated from the earth. A comb made of needle points which has a potential of dozens of kilovolts supplies the belt with electric charges. When the belt moves over the rollers, the charges are moved to the upper roller so that the latter receives a far greater electric charge and thereby a high voltage to earth.

In 1933, a 5 million volt (5 MV) Van de Graaff generator was built in South Dartmouth, Massachusetts. The first Van de Graaff generator for clinical use was installed at the Massachusetts Memorial Hospital in Boston in 1937 and generated a voltage of 1 MV. The high voltage was used to accelerate electrons in an x-ray tube. Actual accelerators (such as betatrons and linear accelerators) for clinical use were not yet available in the 1930s, but *Ernest Lawrence* (1901-1958) had already constructed the *cyclotron* for physical research in 1931. The first Swedish cyclotron, still for physical research, was put to use in 1937 for the Academy of Science's research institute for physics in Frescati in Stockholm (now the Manne Siegbahn Institute). It had been constructed under the leadership of physicist *Sten von Friesen* (1907-1996).

Even more advanced technical equipment emitted higher-intensity x rays and offered the option of irradiating from an even greater distance, which reduced the disruptive effect of the inverse square law. It also became possible to irradiate deeper tumours. However, the radiation doses still remained in the skin where the primary beam collided with the body, an interface for the radiation dose that could be generated in the depth of the tumour with one single treatment field. This could be circumvented by irradiating the body from different directions and thereby distributing the irradiation of the skin. This required the calculation of the radiation doses in different parts of the body between skin and tumour, and *dose planning.*

One dose planning tool was a graphic production of the way in which the radiation dose from the given treatment field varied in depth and width in the body. This could be illustrated by connecting points with the same radiation dose with curves called *isodose curves.* In 1929, the colourful English radiophysicist and subsequent ICRP member *W.V. Mayneord* (1902-1988) described the use of isodose curves in his book, *The Physics of X-Ray Theory,* in the following words:

> The result of measurements at 1-cm intervals, for example, over the whole of the x-ray beam can be appropriately represented by a series of curves connecting the points that have the same dose per second, i.e., we have drawn 'isodose lines' just like in the weather charts in Times where the points that have the same air pressure or temperature can be connected and thereby be given isobars or isotherms.

Early isodose curves were reproduced by *Friedrich Theseuer* (1881-1963) and *Hans Holfelder*, among others, both of whom were in Frankfurt am Main. From around 1920, when planning doses for the cases where the beam was allowed to come from several different directions in order to spare the skin, the dose contribution from each beam was calculated at a number of points of interest in and around the tumour to be destroyed. When isodose curves were used which, in such cases, overlapped so that two bundles of isodose curves together formed a grid, it was possible to easily obtain the curves for the sum of the doses by drawing the diagonals in the grid.

Isodose curves were obviously not particularly well-defined until 1928 when it had been agreed that the 'röntgen' unit would be used for radiation exposure. Rolf Sievert's first achievements for Radiumhemmet in Stockholm in the early 1920s concerned calculations of the radiation intensity around radium preparations of different geometries. Sievert recommended expressing the intensity of the radiation in relation to the intensity of the gamma radiation from a specified quantity of radium at a given distance, and he had a scientific dispute with the German (who later became American) physicist *Otto Glasser* concerning isodose curves. Glasser had used this designation in an article in 1922 while Sievert preferred the designation of 'isointensity' since it was the intensity of the radiation rather than the radiation dose which both had calculated.

American hospital physicist *Edith Quimby* used the *erythema dose* (see Chap.12) in 1932 as the unit for her isodose curves, but in 1944 changed over to expressing them in röntgens.

11. SOMBRE CLOUDS BEFORE THE FIRST WORLD WAR

Progress within physics at the time of Becquerel's discovery of radioactivity was largely dominated by the research at the Cavendish Laboratory in Cambridge. In this chapter, I will give you a rather more rhapsodic account of other events in natural science from the turn of the century up until the First World War - events that would come to be important to radiation physics and, in later years, radiation protection.

I have already fleetingly mentioned Philipp von Lenard's achievements at the end of the 1800s, the way in which he, as one of Hertz' collaborators, experimented with cathode rays and thereby contributed to the knowledge that existed when Röntgen made his major discovery. Unfortunately, Lenard later would end up playing a less than agreeable role in Nazi Germany (see Chap. 17). But almost one and a half years after Röntgen's discovery, he actually wrote Röntgen a letter - one that he must have deeply regretted later but for which we ought to be grateful since it probably does show the truth. The letter is dated 21 May 1897:

> Since your major discovery aroused such sudden attention in the most distant of circles, my modest work also came into the limelight, which was particularly fortunate for me, and I am doubly glad to have had your kind participation.

However, this would not be the case later on when Lenard did everything to belittle Röntgen's achievement and claimed that he was actually the person who ought to be seen as the discoverer of x rays. In an interview after the Second World War, Lenard said:

> I am the mother *[sic]* of Röntgen rays. Like a midwife who is not responsible for the actual birth, Röntgen was not responsible for discovering x rays. All Röntgen needed to do was press a button since all of the preparatory work had been done by me.

Lenard's notorious *Deutsche Physik* from 1936 mentions neither Röntgen nor Einstein. It is fortunate for Röntgen's reputation that Lenard wrote his letter in 1897 and that it happened to be preserved. Perhaps Röntgen had premonitions since he had almost all of his other letters and documents destroyed.

At the start of the new century, when research in physics was targeted at revealing the secrets of the atom since radioactivity had been discovered, a number of other important discoveries were made almost automatically. In 1896, Röntgen himself had discovered the capacity of his rays to ionise air. For a long time after that, air ionisation became one of the most prominent tools for measuring not just the properties of x rays but also studying the radiation from radioactive substances.

Charles Wilson (1869-1959) noted as early as 1900 that ionisation could be measured in a sealed volume of air even though it was not possible to show the presence of any radioactive substance. Wilson was a physicist, meteorologist and lecturer in physics at Cambridge and later invented the *cloud chamber*, often called the 'Wilson chamber', which made ionisations along the pathways of ionising particles visible. A cloud chamber contains a sealed volume of air that is filled with water vapour or some other gas that is close to the point of condensation. If you suddenly increase the volume in the chamber, this lowers the temperature and the vapour is supersaturated and condenses into drops at the slightest disruption. Such a disruption is the ionisations formed by ionising particles through the vapour. The vapour then condenses into drops along the particle pathways so they can be observed and photographed. The phenomenon is the same as when aeroplanes form lines of condensed water vapour behind themselves under corresponding conditions. In 1927, Wilson was awarded the Nobel Prize for his cloud chamber idea, and the cloud chamber later enabled Chadwick to demonstrate the neutron.

However, in 1900, Wilson discovered ionisation without an obvious source of radiation. The same observations were made by German physicists *Julius Elster* (1854-1920) and *Hans Geitel* (1855-1923). Other researchers, including Rutherford, found that this ionisation was reduced if you surrounded the ionised volume of air with absorbent material that was free from radioactive substances. The conclusion therefore had to be that the ionisation was caused by radiation from an external source.

The presence of radioactive substances in the ground and in building materials was discovered early on. In 1903, *H.L. Cook* showed γ radiation from the ground and from bricks. In 1909, *C.J. Wright* found ionisation that was approximately twice as powerful indoors as it was outdoors. The fact that potassium emitted gamma radiation (which we now know from potassium-40) was discovered in 1906 by *Campbell* and *R.W. Wood* (1868-1955) .

A number of researchers later found that the ionisation observed was reduced if you placed the studied volume of air above the surface of a body of water or above a glacier. This indicated that it was primarily radioactive substances in the ground, such as nuclides in the decay chain of uranium, which were emitting the ionising radiation. One of these researchers by the name of *T. Wulf* wanted to show the way in which the intensity of the radiation decreased with the distance from the ground, and he therefore took an ionisation vessel with him up the Eiffel Tower. However, he found a much smaller decrease than he had anticipated. Measurements for the same purpose were made during a balloon ride by another researcher, *A.Gockel* (1860-1927), who also failed to encounter the anticipated reduction. On the contrary, what Gockel found was a reduction that began slowly, followed by an increase in the ionisation at greater heights. There were now two possible explanations. There was either a source of radiation very high up in space, or much less γ radiation was absorbed in air was than was previously believed.

Austrian physicist *Victor Hess* (1883-1964), who was a lecturer in Vienna in the 1910s, quickly eliminated the latter option. He took measurements of the absorption of γ radiation in air and found that it was approximately the same as in water if the same amount of matter had been in the beam. So, that left the option of a source of radiation that was situated high up. Wilson had already made the following statement in 1901:

> Experiments were now carried out to test whether the continuous production of ions in dust-free air could be explained as being due to radiation from sources outside our atmosphere, possibly radiation like Röntgen rays or like cathode rays, but of enormously greater penetrating power.

In order to examine that possibility, Hess undertook ten balloon ascents between 1911 and 1913 and took with him better measurement instruments than those the previous researchers had had access to. Hess was able to confirm Gockel's results, and he showed with accurate measurements that there could be no doubt that an external penetrative radiation was hitting the Earth and that it had the capacity to penetrate the atmosphere down to the surface of the Earth and contribute to the air ionisation there.

German physicist *Werner Kohlhörster* in Halle later made ascents to higher altitudes than Hess and found that the ionisation continued to increase with the height. Continued measurements at even higher altitudes confirmed the presence of a very 'hard', i.e., penetrating, radiation from space. This radiation would be known as 'Hess radiation' or 'ultra-radiation'. We now call it *cosmic radiation.**

In 1936, Hess got to share the Nobel Prize in Physics for his achievement in discovering cosmic radiation. Prior to Hitler's annexation of Austria, Hess fled to the USA in 1938 where he became Professor at Fordham University in New York. Hess was without doubt the person who clarified the phenomenon through methodical investigations and put the presence and character of cosmic radiation beyond any doubt. But, as many times before, we see that the discovery developed gradually with Wilson's initial observation in 1900 and prediction in 1901, Wulf's and Gockel's observations and conjectures around 1910 plus Kohlhörster's confirmatory measurements.

Hess' demonstration of cosmic radiation in 1912 attracted the interest of a great number of physicists, while the interest in γ radiation from the ground lessened and did not grow until the desire was expressed to find uranium for nuclear reactors. The memory of information on 'radiation from the ground' did live on, however, and folklore entertained a legend of a mythical 'terrestrial radiation' ('Erdstrahlung') that had no connection with scientific observations and concepts. It is interesting to note that at the start of 1996, this legend was resurrected in Swedish weekly magazines and the daily press under the semblance of science; people wrote in earnest about a 'grid' of 'Curry lines' and other invented concepts in the spirit of superstition.

In 1904, Pierre Curie and *E. Laborde* demonstrated the presence of high levels of radon in water from 'spas'. This initiated comprehensive studies of the alleged health effects of radon right up until the 1940s when doubt set in about them. Even today, there are countries that proudly advertise the 'life-giving properties' of spas, and not so long ago, information on the radon content was found on the labels of many common bottled waters. An excellent overview of the knowledge up until 1927 was published by *Stefan Meyer* and *Egon Schweidler* (*Radioaktivität* [Radioactivity], B.G. Teubner, Leipzig, 1927).

Comprehensive measurements of radon in drinking water were made in Sweden at an early stage by Swedish mineral chemist *Naima Sahlbom* (born in 1871), who set up a private laboratory in Stockholm in 1914 to analyse minerals, rock samples and water. Along with Professor of Mineralogy *Hjalmar Sjögren* (1856-1922), who later became curator of the Swedish Museum of Natural History, gave Sahlbom details of radon in water in 1907. In 1915, she published results from radon measurements taken in 160 deep wells where she found radon levels of between 5 and 50 Mache units, which in today's units means between 80 and 800 becquerel (Bq) per litre.** Similar values are still measured in such wells.

Marie Curie had already complained about the radioactive coating that was formed when the daughter products of radon stuck to walls and instruments. The long-lived coating that remains when

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* *Primary* cosmic radiation consists mainly of atomic nuclei with very high kinetic energy (10^7-10^{20} electron-volts) and of energy-rich electrons and γ radiation. Fortunately, this primary radiation does not reach the surface of the Earth but is absorbed into the atmosphere. The primary particles generate nuclear reactions at great heights (15-25 km) whereupon mesons are formed, including rapidly decaying π^o-mesons, creating γ radiation that is absorbed through the formation of pairs. Electrons and positrons that have arisen emit energy by generating braking radiation. Charged π-mesons can decay into a myon and a neutrino. *Secondary* cosmic radiation that reaches the surface of the Earth consists largely of myons (relatives of electrons and positrons but with approximately 200 times the amount of mass) which decay into electrons and neutrinos within a few microseconds.

** The Mache unit is a unit which has been given different definitions. It was originally suggested in 1904, and the magnitude to which it referred was the concentration of radon in water - a magnitude for which a normal unit today is becquerels per litre (Bq/l). Since the radon was demonstated through ionisation, *Robert Knox* (1867-1928) introduced a definition in 1918 which stated the Mache unit as the radon concentration that could maintain a current of 1/1 000 electrostatic units per second in one litre of water. The Mache unit was then assumed to be the equivalent of 450 picocuries per litre (pCi/l). Another regularly-occurring comparative figure is 364 pCi/l. Professor *Hermann Holthusen* (see Chapter 14), who wrote about the concentration of radon in blood in 1913, used the comparative figure 500 pCi/l. Since 1 pCi/l is equal to 0.037 Bq/l, details in literature of 1 Mache unit can be expected to correspond to between 13 and 19 Bq/l or usually approximately 16 Bq/l.

the short-lived daughter products have decayed after a few hours is lead-210 (‘radium D’), which has a half-life of 22 years. When the short-lived bismuth-210 decays, this lead forms (‘radium E’), which emits hard β radiation. The bismuth in turn decays to polonium-210, which has a half-life of 140 days and decays to stable lead. After a few years, both the polonium and the bismuth are in radioactive equilibrium with the coating of lead-210, i.e., have the same activity (number of decompositions per second), while the lead coating slowly increases until it reaches an equilibrium with the radon in the air.

In 1906, *Heinrich Mache* (1876-1934, the man who gave his name to the Mache unit) and *T. Rimmer* found that the ionisation on the surface of the ground increased after rain. This was because the rain washed out the radioactive daughter products formed by radon in the air and deposited them on the ground. In that same year, *A.S. Eve* was the first to calculate the relationship between the level of naturally-occurring radioactive substances in the layers of soil and those caused by the resulting ionisation in the air above.

In 1905, Lenard won the Nobel Prize in Physics for his work with cathode rays. In his Nobel lecture, he expressed his conviction of the existence of ether and had a dig at the Cavendish physicists: ‘We found that the propagation of the rays is particularly good in an extreme vacuum; […] the rays attain lengths of several meters […]. Thus cathode rays are *phenomena in the ether.* In particular, […] it could be stated that cathode rays were not radiating *matter,* nor emitted gas molecules, as they had come to be regarded, especially in England’. He was now also starting to question Röntgen’s achievement.

In the early 1900s, one of the world’s most prominent chemists, Professor *Emil Fischer* (1852-1919), a pioneer within organic chemistry, was working in Berlin. In 1907, Otto Hahn came to Fischer in Berlin after having spent a year with Rutherford in Montreal. Rutherford had moved to Manchester that same year.

Hahn, who had already discovered thorium-228 (‘radiothorium’) before leaving for Montreal and who continued to increase his radiochemistry skills in Canada, was regarded with a certain level of wonder by Fischer’s colleagues. Fischer, who did not know much about radioactivity, had realised Hahn’s capability but the other chemists had their doubts - was Hahn really a proper chemist? Hahn was sent to a woodwork area in the university basement where he positioned his gold-leaf electrometer to demonstrate and measure the radioactive substances that he was able to isolate.

As a consequence of the lack of interest on the part of the other chemists plus their lack of knowledge of radioactivity, Hahn got into the habit of attending the physics seminars. There he met a young female Austrian physicist who had just arrived from Vienna and had not yet settled in. Her name was *Lise Meitner* (1878-1968). She had, as only the second woman ever to do so, defended her thesis at the University of Vienna and had come to Berlin to attend Max Planck’s lectures in theoretical physics. She had already written a few articles on radiation from radioactive substances and was therefore of interest to Hahn. He was 28 and she was 29 when they met, but they were the opposite of one another in many respects. Lise Meitner was a small, dark Jewish lady of aristocratic appearance combined with an aloof shyness. Hahn was an extrovert, sports-loving, friendly person with a chivalrous, good-natured attitude to women. Meitner had plenty of time after attending Planck’s lectures and was interested in an opportunity for research. Hahn needed a colleague and they soon agreed to cooperate.

Professor Fischer was just not able to accept the idea of a female researcher on his premises. Meitner could remain on condition that she promised to keep to the woodwork area in the basement and never make her way up into the premises upstairs where the male students were. Her relationship with Hahn was very formal - they never ate lunch together and were largely seen only in the woodwork area or at seminars. Hahn has described the situation as follows:

> There was no question of any closer relationship outside the laboratory. Lise Meitner had had a strict, ladylike upbringing and was very reserved, even shy. And yet we were really close friends.

More broad-minded attitudes had not pervaded the university until a few years later, and Lise Meitner was then permitted to come up from the dark surroundings of the cellar and organise Fischer's students when she wanted to. And there was a major change in 1912: the foundation of the Kaiser-Wilhelm Institute for Chemistry.*

The first two Kaiser-Wilhelm Institutes (for physics and chemistry) were established in Dahlem on the south-western fringes of the city of Berlin. The actual name of the Institute of Chemistry was the Kaiser-Wilhelm Institute for Physical Chemistry and Electrochemistry, and its first director (from 1911, i.e., before the Institute was formally inaugurated by the Emperor) was *Fritz Haber* (1868-1934). He was known for the 'Haber-Bosch method' of producing ammonia using nitrogen in the air, for which he was awarded the Nobel Prize for Chemistry in 1918, a controversial prize bearing in mind Haber's achievements for chemical warfare during the First World War.

Hahn and Meitner moved to the Kaiser-Wilhelm Institute for Chemistry in 1912, and one of Hahn's first assignments was to arrange a suitable demonstration for the Emperor at the inauguration. He placed a mesothorium preparation (i.e., radium-228) on a velvet cloth to show the Emperor the fluorescence generated by the radiation. As regards radiation intensity, the preparation corresponded to 0.3 of a gramme of 'normal' radium (i.e., radium-226), so it was a very strong preparation. Fifty years later, Hahn said: 'If I were to do the same thing today, I would end up in prison.'

Times were now sombre as war approached. Many people nowadays understand the meaninglessness of war, and that the phenomenon known as homicide or murder when executed by individuals is just as much homicide and murder in matters between states. We must understand that the situation was different in 1914. Fathers, school teachers, theatres, literature and newspapers cooperated to emphasise the importance of the Fatherland and the glory in defending it (even if this did take place through attacks). Patriotism and a sense of duty led many to carry out acts that we, within the reference frameworks available today, see as incomprehensible or criminal. However, wild west movies do still show us the how the hero leaves his despairing sweetheart since 'a man's gotta do what a man's gotta do...'.

The war came and heroes died meaningless deaths. Rutherford's young assistants were dragged away for military assignments and Niels Bohr, who was out rambling in Germany with his brother when the war broke out, returned home quickly and continued with his wife to Manchester to assist Rutherford. The young Henry Moseley was in Australia visiting his mother when patriotism took over and urged him to return home and register as a volunteer. He was sent to Gallipoli the following year. On the way there, in Alexandria on 27 June 1915, he wrote his last will and testament and decided to leave all that he owned, £2 200, to the Royal Society 'to be used to support experimental research in pathology, physics, physiology, chemistry or other branches of science, but not for pure mathematics, astronomy or any branches of science aiming only to describe, catalogue or systematise'.

The British troops had already started an invasion of Turkey by landing on Gallipoli peninsula in April, and Moseley ended up in a group that landed later and which, like others, dug themselves down in trenches. The Turks attacked at dawn on 10 August and, according to a description by the poet *John Masefield* (1878-1967) who survived the attack as a Red Cross man, the fight was a 'body against body, with knives and stones and teeth, a fight worthy of wild animals'. Henry Moseley died in that fight.

The chemists at the Kaiser-Wilhelm Institute for Chemistry also devoted themselves to the war in their own way - using chemical means. Fritz Haber, now no longer head of the Institute but called up for military service as a captain (unusual for a civilian), was ordered to set up a unit for gas warfare. Otto Hahn, also called up, was a Reserve Lieutenant and was ordered to visit Haber. Hahn was

* In 1911, Wilhelm II founded the 'Kaiser-Wilhelm-Gesellschaft' for the purpose of running a number of research institutes (there were seven of them in 1914). The first two 'Kaiser-Wilhelm Institutes' (for physics and chemistry) were opened in 1912. After the Second World War, the activities ceased but were replaced in 1948 by the 'Max-Planck-Gesellschaft' which was established in Göttingen with Otto Hahn as its first president. The Max-Planck Society correspondingly runs the 'Max-Planck Institutes' for research, whereas the Institutes for Physics and Physical Chemistry are in Göttingen.

indignant about what he heard but Haber attempted to justify the assignment. Hahn stated (according to Richard Rhodes):

> He explained to me that the western fronts, which were all bogged down, could be got moving again only by means of new weapons. One of the weapons contemplated was poison gas. […] When I objected that this was a mode of warfare violating the Hague convention he said that the French had already started it – though not to much effect – by using rifle-ammunition filled with gas. Besides, it was a way of saving countless lives, if it meant that the war could be brought to an end sooner.

A number of industrial chemists participated in the work at what would later become I.G. Farben's factories in Leverkusen. I.G. Farben was formed in 1925 as a cartel consisting of a number of chemical companies that had been convinced by the German chemist and industrialist *Karl Duisberg* (1861-1935) to cooperate. As head of the chemicals and pharmaceuticals company Bayer AG, founded in Leverkusen in 1863 (the company whose success was achieved by marketing aspirin), Duisberg had already succeeded in establishing cooperation between BASF (Badische Anilin- und Sodafabrik, which developed the Haber-Bosch method for the production of ammonia), Agfa and Bayer in 1904.

Large quantities of poisonous substances were produced in Leverkusen and sent to the Kaiser-Wilhelm Institute for chemistry in Berlin-Dahlem for further investigation. Special storage premises had to be built plus a tuition premises where Otto Hahn gave instructions in gas safety techniques.

On 22 April 1915, it came as a surprise when war gases were used at Ypres in Belgium. It was chlorine gas that killed thousands of soldiers and injured tens of thousands. The 'successful' result led to an arms race for war gases. People started using tear gas, phosgene, mustard gas and even arsenic powder. The Frenchmen charged grenades with hydrogen cyanide, although to little effect. In October 1918, just before the war ended, again at Ypres, a German Corporal by the name of Adolf Hitler was injured in a British gas attack. Had this not occurred, the history of the world may well have had a completely different outcome.

Otto Hahn had grave misgivings, Fritz Haber probably fewer.* Haber's wife, *Clara Immewahr Haber* (1870-1915), also a chemist with a doctorate from the University of Breslau, had very grave misgivings and demanded that her husband leave the work with war gases. His response was that a scientist belongs to the world in times of peace but to his country in times of war. This became too much for Clara. She took her own life the following night.

When the Germans started using mustard gas in July 1917, a comprehensive war gas research programme was started in the USA with research contracts with a number of universities, including Cornell, Johns Hopkins, Harvard, MIT, Princeton and Yale. A war gas factory was built in Maryland for 35.5 million dollars for the production of chlorine gas, phosgene, mustard gas and other poisonous substances. Ten thousand people worked there and there was a huge capacity by the end of the war. Had the war not stopped in November 1918, it would have turned into an enormous gas disaster.

However, not all efforts were aimed at worse weapons. Eve Curie writes: 'Mme Curie had foreseen everything - that the war would be long and murderous, that the wounded would have to be operated upon more and more in the places where they were found, and that the surgeons and radiologists would have to be at hand in the front ambulances; that it was urgently necessary to organise the intensive manufacture of Röntgen apparatus - and, finally, that the radiological cars would be called upon to render invaluable service'. Eve continues:

> A telegram or a telephone call would notify Mme Curie that an ambulance laden with wounded demanded a radiological post in a hurry. Marie would immediately

* In 1918, Haber won the Nobel Prize for Chemistry (given out in 1919) for his achievements in ammonia production). It raised some protests due to his involvement with poisonous gas during the war, but his institution became an internationally recognised centre for chemical research once more in the 1920s. He left the Kaiser-Wilhelm Institute in 1933 since he refused to dismiss Jewish colleagues, and moved to the Cavendish Laboratory. At the time of his death, a commemoration was held for him at the Kaiser Wilhelm Institute, despite reluctance on the part of the Nazis.

verify the equipment of her car and attach her apparatus and dynamo. While the military chauffeur took on petrol, she would go home and get her dark cloak, her little travelling hat, soft and round, which had lost both form and colour, and her baggage: a yellow leather bag, cracked and peeling. She climbed in beside the driver, on the seat exposed to the wind, and soon the stout car was rolling at full speed - namely, the 'twenty-miles-an-hour average', which was its best - toward Amiens, Ypres, Verdun. After various stops and palavers with untrustful sentries, the hospital appeared. To work! Mme Curie rapidly, chose one room as a radiological hall and had her cases brought in there. She unpacked the instruments and assembled them from their separate pieces. The cable which connected the apparatus with the dynamo in the motor car was rolled out: the chauffeur, at a given signal started the dynamo, and Marie tested the intensity of the current. Before beginning the examination of the wounded, she prepared the radioscopic screen and ranged her protecting gloves and glasses near at hand, along with special marking pencils and the leaden indicator which found the projectiles. She darkened the room by stopping up the window with the black curtains she had brought, or even with ordinary hospital blankets. At one side, in an improvised photographic dark-room, were placed the baths of chemicals where the plates would be developed. Half an hour after Marie's arrival, everything was ready.

The melancholy procession began. The surgeon shut himself and Mme Curie into the dark-room, where the apparatus in action was surrounded by a mysterious halo. One after the other the stretchers laden with suffering bodies were brought in. The wounded man would be extended on the radiological table. Marie regulated the apparatus focused on the torn flesh so as to obtain a clear view. The bones and organs showed their precise outlines, and in the midst of them appeared a thick dark fragment: the shot or piece of shell. […]

Apart from the twenty motor cars she equipped, Marie installed *two hundred* radiological rooms. The total number of wounded men examined by these 220 posts, fixed or mobile posts created and started going by Mme Curie personally rose to above a million.

The use of x rays increased heavily during the First World War. All parties were forced to use primitive equipment, which still managed to save the lives of many in that it was possible to locate bullets and grenade shrapnel and made it easier to carry out difficult operations. Radiation protection was often non-existent and, after the end of the war, radiation damage started to attract attention and was brought into the public eye. This laid the foundations for international radiation protection work.

12. JOHN BERG, RADIUMHEMMET, FORSSELL AND SIEVERT

We have already made the acquaintance of x-ray pioneers Sjögren and Stenbeck, and we have seen that the young Gösta Forssell assisted Stenbeck in his Institute for Radiation Treatment at Mäster Samuelsgatan 63 at the turn of the century. However, the person who is credited with actually having initiated a well-organised radiological business in Stockholm is a surgeon, *John Berg* (1851-1931). According to the National Encyclopaedia, 'Father Berg' was 'a prominent doctor and a pioneer within aseptics and surgery in Sweden, and his organisational achievements within healthcare and medical treatment were substantial'. One of these achievements was the creation of Radiumhemmet.

Photographs of Berg from around 1910 show a powerful man with a full, white beard who resembled King Oscar II. At the start of the century, Berg had found that the possibility of treating cancer had not been improved in spite of progress in surgery and ever more drastic operations. On the other hand, Sjögren's and Stenbeck's successful progress had shown that x rays could be effective, and there was also talk of using the gamma radiation from radium, the new substance that was still thought to possess mysterious properties.

In 1905 at Berg's suggestion, the almost 100 year-old Swedish Society of Medicine got the then National Board of Health to send a questionnaire to all Swedish doctors asking them for information on the cancer patients who had been examined over a three-month period. 97 per cent of Sweden's doctors participated in this survey. The responses were analysed in 1906 by a special committee and a report was presented at a meeting with the Society of Medicine in 1907. The report was published in German in 1909 in *Zeitschrift d. Krebsforschung* under the eloquent German title of 'Bericht über die von der schwedischen Ärztgesellschaft veranstaltete Sammelforschung über die Krebskrankheit in Schweden während der Zeit vom 1. Dezember 1905 bis 28. Februar 1906. Erstattet von dem Krebsforschungskomitee der Gesellschaft'.

According to Moritz Simon, the Seraphim General Hospital had already received an x-ray apparatus in 1901. However, *Elis Berven* (1885-1966), head of Radiumhemmet from 1927-1950, dates the first x-ray laboratory there to 1903. *Åke Åkerlund* (1887-1958), the well-known head of Karolinska sjukhuset's x-ray diagnostics department from 1941-1952, says that Berg started an x-ray laboratory at the Seraphim General Hospital in 1905 next to the surgical facility. The information need not be contradictory.

On 1 July 1906, Gösta Forssell was employed as Director of the x-ray laboratory. *Lars Edling* (1878-1962), Professor of Radiology in Lund, writes about his business there in *Svensk Uppslagsbok*:

> The Seraphim General Hospital's x-ray institution, a model institution equipped with equipment cleverly designed by F. Here, he carried out intensive research work, particularly within the x-ray anatomy and physiology of the alimentary tract; his work on the movements of the gastric and intestinal mucosa during digestion has been pioneering. F's numerous and valuable investigations also include noteworthy work during his youth on the movements of the wrist and essays within the radiography of the skeletal system and urinary organs.

Something ought to be mentioned about Gösta Forssell's background. There are two well-known 'Fors(s)ell' families, and in Gösta Forssell's time they each had their well-known representative: *John Forsell* (1868-1941), the temperamental opera singer, and Gösta Forssell, 'the father of Swedish radiology'. John Forsell's family came from Västergötland while Gösta's family came from Järvsö in Hälsingland. Gösta's father, curator *Abraham Forssell,* had four sons who became successful enough to be mentioned in Swedish encyclopaedias: *Carl Forssell* (1881-1973) became Professor of Structural Mechanics at Kungliga Tekniska Högskolan, or the Royal Institute of Technology (KTH). *Gerhard Forssell* (1882-1964) became Professor of Surgery at Veterinärhögskolan, or the Royal Veterinary College of Sweden. *Arne Forssell* (1887-1974) became city architect in Stockholm and the father of author *Lars Forssell* (1928-2007). Gösta was the oldest brother, born in 1876.

It was 1902 when engineer Bror Edvard Järnh took over 'John Anderssons Elektro-Tekniska Byrå'. The company's name was then changed to 'John Anderssons Elektro-Tekniska Byrås Eftr.', and Järnh expanded the business to also include electric power and lighting installations. The increase in interest shown in x-ray equipment meant that Swedish doctors asked Järnh to import such equipment and to directly contact foreign manufacturers. This introduced a unique cooperation between the x-ray industry and the Swedish radiologists that was beneficial to both groups.

The cooperation between Järnh and Gösta Forssell was particularly rewarding in that Forssell had numerous ideas for new designs for technical tools, including a famous fluoroscopic stand and a view-box for x-ray plates. Over dinner at the home of the hospitable Järnh they discussed the need for a society in which the radiologists would have the opportunity of exchanging experiences. This led to six radiologists forming the Swedish Radiological Society in 1907. The six radiologists, including Forssell, were *Fredrik von Bergen* (1877-1951), who later became consultant at Sahlgrenska Hospital in Gothenburg; Ivar Bagge; *Patrik Haglund* (1870-1937), a certified physical training instructor and orthopaedic radiologist, later to become Professor at Karolinska Institutet; *G. Holm*, who is unknown to me; and Tage Sjögren.

Very little is now known about the activities of this society except that Messers Bagge, Forssell and Sjögren drew up an 'average rate' in September 1907 according to which the following charges were proposed:

Fluoroscopy	20 Swedish kronor
Photograph of hand or foot	15 Swedish kronor
Pelvic x ray	30 Swedish kronor
Bladder stones	40 Swedish kronor

In spring 1908, Gösta Forssell was called to the Royal Palace at the request of Queen Victoria who had heard from her mother, Archduchess Louise of Baden, that radium had been successfully used in the treatment of cancer. The queen really wanted to see such treatment methods in Sweden. Forssell was not unknown to the Palace - he had previously been invited to be governor to Prince Erik, Victoria's youngest son (1889-1918), but declined. The queen now proposed that Forssell and John Berg take a trip to study the new treatment methods, and she offered to pay the travel expenses. This was the way in which radium treatment was introduced in Sweden as a complement to x-ray treatment.

The organisation that the enthusiastic John Berg then took the initiative of forming in his campaign against cancer was to become more robust than the Swedish Radiological Society. The Swedish Cancer Society was formed on 7 May 1910. The intention was for this organisation to continue the work that had been started by the Society of Medicine's cancer committee whose report had been published in 1909. Berg formulated the following principles for the Association's activities:

1. Practical humanitarian measures to improve the treatment and care of cancer patients;
2 Cooperation with private doctors in their statistical research;
3. Economic support for scientific and popular documents on cancer problems;
4. In-depth studies and use of ongoing investigations into cancer in other parts of the world.

The Swedish Cancer Society was immediately affiliated to the International Association for Cancer Research. Berg, Forssell and *Gunnar Nyström* (1877-1964), secretary of the Cancer Society and later Professor of surgery at Uppsala, were Sweden's participants at a cancer congress in Paris in October 1910. It became increasingly clear that radiation treatment would start complementing and in many cases perhaps replacing the surgery aspect of cancer treatment.

After C.F. Liljevalch's deceased estate donated Swedish kronor 20 000 for the purpose, the Cancer Society started a statistical research project on cancer in 1911. A questionnaire was sent out to 1 300 doctors and no fewer than 1 243 responded. As the study continued, the level of interest fell, however: in 1912, 1 018 doctors responded to the questionnaire but just 432 in 1913. The responses were processed by Gunnar Nyström, although he did not publish the result until 1922.

On 2 June 1910, scarcely one month after the parent organisation had been formed, a local section of the Cancer Society was established in the Stockholm area with John Berg as chairman and Gösta Forssell as secretary. The powerful status of the management of the Stockholm section meant that it took over almost the whole of the initiative. The parent organisation met just a few times until 1919 when it was dissolved and the Stockholm section took over its tasks under the name of the Cancer Society in Stockholm. It was the period following that renunciation that was referred to when the name of 'Cancer Society' was used for the sake of simplicity.

Both Berg and Forssell understood the radiation treatment options, but also that it required greater resources than those that were available at the Seraphim General Hospital. In 1909, Berg and two friends had therefore made available 40 000 Swedish kronor to establish a special radiation treatment institute as an experiment. Two floors on Scheelegatan 10 at Kungsholmen in Stockholm almost opposite the town hall were rented for this purpose in April 1910. The new institute had 8 rooms containing 16 patient beds, 6 for men and 10 for women. It had an x-ray apparatus plus 120 milli-grammes of radium that had been procured through private donations. The radium was divided into 11 preparations.

The new institute was opened on 1 August 1910 and was given the name of 'Radiumhemmet'. According to Berven, this name was chosen to 'encourage the impression that the patients were in a home where their disease would be regarded with sympathy and understanding'. The first doctors to be employed were Forssell, Berven and gynaecologist *James Heyman* (1882-1956). Forssell remained head of Radiumhemmet until 1927 when he was succeeded by Berven. In 1911, Radiumhemmet was transferred to the Cancer Society in Stockholm which was responsible for running it until 1937.

Berg's initiative and money had created Radiumhemmet. Forssell, Berven and Heyman made it world-famous with the inspiration coming from Forssell. The cooperation with Forssell meant that Järnh's electrotechnical company rapidly developed to become Sweden's first specialist x-ray company. In 1917, the company was reorganised into a limited liability company and was then named 'Järnhs Elektriska Aktiebolag' with Bror Edvard Järnh as the Managing Director. The business activity was run between 1914 and 1939 in Sundbyberg. In 1939, a completely new factory was erected at Industrivägen 23 in Hagalund.*

Järnh sold imported x-ray equipment until the First World War but the wartime blockades meant that it became more and more difficult to import them. Swedish radiologists then convinced Järnh to start producing x-ray equipment himself, even though x-ray tubes and valve tubes still had to be imported. Good quality and ingeniously-designed technical tools were produced primarily in cooperation with Gösta Forssell and Åke Åkerlund. One of the biggest export successes of the 1920s was a spiral diaphragm designed by Åkerlund.

Radiumhemmet's activities on Scheelegatan were successful and continuing donations meant it grew so much that the premises became inadequate. In 1913, a grant of half a million Swedish *kronor*

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* Another early company for the production of x-ray equipment was started in 1924 by *Georg Schönander* (1894-1958) which also played an important role in the technical development, including through the *Lysholm Grid*, a microgrid for a secondary diaphragm which won international acclaim after 1928. And the participants in the Radiology Congress in Stockholm in 1928 included *Wilhelm* and *Gustav Weber*, father and son, representing 'A.-B. Elema', the company that would go on to occupy a leading position.

was received from the city of Stockholm from the Forsgrenska Fund and an additional half a million through private fundraising initiated by Queen Victoria. They therefore looked around for larger premises and, in November 1916, were able to move to a building that had previously housed the Frans Schartau Business Institute. There were two buildings on Fjällgatan in Södermalm. One of them had premises for day patients on the ground floor with a waiting room and examination and treatment rooms. The two upper floors accommodated treatment wards for 34 patients and a room for radium treatment. The other building contained an x-ray laboratory with four apparatuses on the ground floor, housing for nurses and assistants on the two upper floors and a kitchen and an office in the basement.

Radiumhemmet remained on Fjällgatan until 1937. There were early concerns about insufficient quantities of radium and outdated x-ray equipment, but Parliament contributed 200 000 Swedish kronor in 1917 for the purchase of radium and an additional 35 000 Swedish kronor in 1919 for two x-ray machines. In 1917, Radiumhemmet was divided into two departments: a general department with Elis Berven as head and a gynaecological department under James Heyman.

Diagnostic radiology within healthcare and medical treatment was obviously also developing rapidly and this sparked a need for a more efficient discussion forum than the 1907 Swedish Radiological Society which was not particularly active. Gösta Forssell therefore sent out a circular on 8 May 1919 which read as follows:

> I hereby have the pleasure of inviting you to a meeting for the formation of a Swedish society for radiology. The meeting will take place on Saturday 17 May at the Seraphim General Hospital's Röntgen Institute at 7 p.m. precisely. If on this occasion you propose to give a lecture or demonstrate something of interest, this would be very welcome. An evening meal is being arranged after the meeting. Notification of any lecture and your participation in the meeting and the evening meal should be sent as soon as possible to Dr. E. Berven, Radiumhemmet, Fjällgatan 23, Stockholm.

The host and 23 others attended the meeting on 17 May 1919.* Forssell was elected as chairman of the meeting and Berven as secretary. It was agreed that a society would be formed which, at Forssell's suggestion, was given the name of the Swedish Society for Medical Radiology. The society was largely a scientific society, and there was no discussion about also handling trade-union matters (in 1920, the society did submit a communication to the then Royal General Hospital Regulations Committee concerning the terms of employment for the radiologists, however). Forssell had already drawn up regulations that were largely approved. Forssell was then elected as chairman, Berven as secretary and Heyman as treasurer. The annual fee was fixed at 10 Swedish kronor. The society would be open to Swedish doctors, engineers and scientists active in the development of radiological research.

The Radiological Society (as it was usually called) soon took up a proposal from Forssell to create a radiology journal, not least since the war had made it difficult to get articles published internationally. The journal would include papers in English, French or German and accounts of important articles in other media and of lectures and discussions within the Radiological Society. If possible, it would also become a forum for the other Nordic countries. The Society declared its fundamental approval and, once Forssell had succeeded in securing adequate financial support from elsewhere, *Acta radiologica* came to fruition in 1921.

When the Radiological Society was formed in 1919, the war meant that it was difficult to establish international contacts. Finland had declared itself independent in 1917 and had then been plagued by a civil war. Things were easier for radiologists from Denmark, Norway and Sweden and they kept in contact with one another. Three Nordic radiologists had the opportunity of discussing the Nordic

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* Z.G. Asplund, I. Bagge, E. Berven, C. Carlström, E. Edberg, F. Edling, G. Forssell, N. Hansson, S. Hasselroth, J. Heyman, B.E. Järnh, A. Karsten, T. Klason, H. Laurell, G. Lundgren, B. Lundquist, W. Moberg, G. Runström, J. Schauman, M. Simon, T. Sjögren, S. Ström, A. Troell, H. Waldenström and Å. Åkerlund.

cooperation at a surgical congress in Gothenburg in 1916: *S. A. Heyerdahl* from Rikshospitalet in Kristiania (the former name for Oslo), *H. J. Panner* from Rigshospitalet in Copenhagen, and Gösta Forssell. It was agreed that a Nordic society was needed but that it was difficult to create one during the war.

The next surgical congress would be held in Kristiania (Oslo) in 1919, by which time the war was over. The Norwegian radiologists (there were only eight, and some said seven) had doubts about the responsibility of also holding a radiology conference, but Forssell was eager. 'How's it going with the new society? We'll come when we're invited. There's no need for anything grand' he wrote to the Norwegians. Severin Heyerdahl (1870-1940), who was the leading radiologist in Norway, gives an account (according to Erik Poppe in the 60th anniversary letter from the Nordic Radiological Society):

> It was not that we had delightful things to offer, we radiologists in Kristiania in 1919. Our only public x-ray institute to offer teaching was Rikshospitalet's x-ray radium institute, and the conditions here were 'pitiful' to put it mildly. There were only 4 radiologists in Kristiania in 1919, but our argument was that one is one, two are two but 4 is a community. We overcame our qualms and sent invitations to all Danish, Finnish and Swedish radiologists and to the three Norwegian radiologists outside Kristiania.

No Finns were able to come to Kristiania in 1919. Eight participants hailed from Sweden: Bagge, Berven, Edling, Forssell, Heyman, Simon, Ström and Åkerlund. Just two came from Denmark: *C I Baastrup* (1885-1950) and *J F Fischer* (1868-1922).

The society was formed on 2 July 1919 and was named the Nordic Society for Medical Radiology. Heyerdahl was its first President, and the decision was that in future, the President would be elected from the country in which the society's congress was to be held. The next Nordic radiology congresses were held in Copenhagen in 1921 and in Stockholm in 1923.

In 1920, the Norwegian Society for Medical Radiology was formed and also the Finnish Society for Medical Radiology and the Danish Radiological Society in the same year. Currency difficulties meant that the Finnish radiologists were unable to participate in the Nordic radiology congresses at the outset, but the Nordic congress was held in Helsingfors in 1925.

In around 1920, another important person entered the scene, namely *Rolf Sievert* (1896-1966). Sievert's father, Max Sievert, was one of three brothers (Max, Georg and Ernst) who emigrated from Sachsen to Sweden at the end of the 1800s. Their father, *Heinrich Theodor Sievert* was a confectioner in Zittau, a small town near the border of the former Czechoslovakia. Among other activities, *Max Sievert* (1849-1913) ended up devoting his time to agency work for 'Zentkers Maschinenfabrik' in Berlin. He predicted that there would be good business opportunities in the Scandinavian countries, and moved to Stockholm where he started 'Max Sieverts Maskineaffär' in 1881. He became a Swedish citizen in 1885 and married *Sofia Carolina Pancheen* in 1894.

The introduction of the telephone in Sweden by *Lars Magnus Ericsson* (1846-1926) required the expensive importation of telephone cables, and Max Sievert's company was one of the importers. Ericsson suggested that Sievert take up production himself, which Sievert did firstly as an experiment. This led to his starting a more professional cable production company in 1888 together with his much younger brother *Ernst* (1863-1941), who followed him to Sweden in 1887. The third brother, Georg, who had now also moved to Sweden, was given tasks within Max' agency business. In 1895, the cable company was restructured into a limited liability company that went by the name of Max Sieverts Fabriks Aktiebolag. In that same year, his daughter *Lisa* was born, followed by his son Rolf the year after.

Max Sievert was an energetic industrialist who did not seem to have a great deal of time to spend with his children. Photographs of him show a powerful and single-minded man, not unlike Rolf at the same age. Lisa and Rolf seem to have had a peaceful childhood, though, particularly during the summers at the family's country house by Vallentunasjön. During the winter period, the Sievert family lived at Riddargatan 45 in Stockholm.

When Max Sievert died, Rolf was just 17 and had not yet left his secondary school, Nya Elementarskolan on Slöjdgatan behind Hötorget. His fellow students there in 1914 included *Herbert Tingsten* (1896-1973), *Olof Svanberg* (1896-1975, subsequently Professor of Agricultural Chemistry at Uppsala), *Viking Tamm* (1896-1975) who would later become a general, and *Guido Valentin* (1895-1952, later a well-known author, journalist and theatre man, and an uncle of Jack Valentin's). Rolf's grades as a student were not brilliant – the death of his father had probably had a negative impact.

After his school leaving exams, Rolf and his sister Lisa became heirs to a big estate, but Rolf was not interested in following in his father's footsteps. He proceeded by trial and error instead. In autumn 1914, he began studying medicine at Karolinska Institutet but soon grew tired of that. Since he was interested in technical matters, he instead applied to the Royal Institute of Technology (KTH), but his student grades were not sufficient, and he had to do a supplementary examination in mathematics, physics and chemistry. This was so successful that he was accepted into KTH's electrotechnology department in autumn 1915. He might have anticipated 'elective' study, but the rate of study at KTH was strict and demanding and Rolf grew tired after just a few months. He then hoped that student life at Uppsala would be less arduous and went there to study for a Bachelor of Arts Degree, which he succeeded in passing in 1919 in the subjects of astronomy, meteorology, mathematics and mechanics.

Rolf Sievert returned to Stockholm as 'fil.kand.' (the approximate equivalent of BA Hons.) and was now living in Lärkstaden. He appears to have carried out various experiments in his own laboratory in the basement where he was visited by the younger physicist Sven Benner, who would later become his colleague. Rolf had now decided to continue his studies at 'Stockholms Högskola' (Stockholm University), this time in physics, for a Licentiate Degree.

At the same time, he became deputy of the board at 'Sieverts kabelverk', as the company was usually known, but he does not seem to have shown any actual interest in the company. During 1926-1928, he was an ordinary board member, but in 1928, he and his mother and sister sold the 1 000 shares they owned to the other shareholders. Ernst Sievert then had 1 824 of 2 400 shares and was able, that same year, to decide that the company would be taken over by L.M. Ericsson.

In 1919 we find that, while studying for his Licentiate Degree, Rolf Sievert was working as an assistant to senior lecturer *Tycho Aurén* at the Nobel Institute for Physics.* Aurén, who was a Senior master at Statens högre lärarinneseminarium (the State Higher Schoolmistress' Seminary), was also doing research and studying x-ray absorption. At the Nobel Institute, Sievert was contacted by one of Radiumhemmet's doctors, *Gunnar Lundgren*, who asked a question that would determine his future. Lundgren later became head of the x-ray department of the Danderyd General Hospital. Following Sievert's death, I wrote to Lundgren and asked if he remembered the meeting. I received the following answer on a postcard:

> With regard to Rolf Sievert, I remember that my boss at Radiumhemmet, Prof. Gösta Forssell, was unable to find a physicist who wanted or was able to devote himself to calculating the doses of radiation.
>
> At some stage I heard about Rolf Sievert who studied at the Nobel Institute; who from I can't remember. I contacted Forssell, who asked me to speak to Sievert and assess his suitability. I gained a very good impression and asked Sievert to see Forssell. Sievert was employed immediately. It was so much easier since the financial side didn't matter to him.

A tape recording of a lecture by Sievert at his Institute of Radiophysics in 1964 brings the following memory to mind:

> I happened to be an assistant at the Nobel Institute in 1919 for senior master Aurén who was doing research into x-ray absorption when along came a Doctor Lundgren,

* Sometimes called the Academy of Sciences Research Institute for Physics, and later the Manne Siegbahn Institute, in Frescati north of Stockholm..

who worked at Radiumhemmet, and declared his need for cooperation with a physicist, saying that he had had no success in finding anyone for such cooperation. This was actually the start of my interest in this. Then, in 1920, I was on a study trip in America and happened to meet Forssell and we travelled together and looked at devices for the production of emanation preparations, and also at radiotherapy installations, and this helped me to find my footing with this work.

Forssell himself gives an account of the same meeting (in English) in a tribute for Sievert's 50th birthday:

I met Rolf Sievert for the first time during a study trip to the United States in the summer of 1920. He was there in order to survey the latest advances in the field of physics in America, and I flitted like a bee from flower to flower in the radiological garden of the New World. He was deeply impressed by the possibilities for development in radiation physics, and I in my turn by the importance of team-work for medical radiology. And so, it came about that our paths crossed in Schenectady, the stronghold of radiological technology in America. There, in a hotel room high up among the stars, we sat throughout an entire night and compared our observations and our plans, and there were laid the foundations of a friendship and a companionship in the service of radiology which has lasted ever since.

At 'Radiumhemmet' we could only dispose of one small room for physical control work,[*] but Sievert placed himself and his own private physical laboratory at the disposal of Radiumhemmet for radiophysical research work. For four years, Radiumhemmet had - as it is recorded in the annual report of the Cancer Society - »the benefit of voluntary and unpaid collaboration» with this radiation physicist, »who assisted the Home in controlling the roentgen dosage and constructed for this purpose extremely valuable instruments, and also carried out research at Radiumhemmet on the dosage problem». In 1923, the Cancer Society of Stockholm decided to enlarge Radiumhemmet with a building which would contain – as well as a laboratory for pathological anatomy, a library and a museum - a small department for medical radiation physics.[**] This was opened on July 5th, 1924. Sievert conjured forth 'anonymous donors' who provided 30 000 Swedish Crowns 'as a contribution to the upkeep and expenses of the physical institute for the next five years, with the provisio that the Cancer Society should procure funds for complete equipment of the institute, and also guarantee its maintenance and upkeep for a period of five years'.

Another early addition to the research at Radiumhemmet was pathologist *Olle Reuterwall* (1888-1956) who was employed in 1921 and became head of a separate pathological unit in 1923. Reuterwall later became head of the separate radiopathological institution which was created in 1937, and became Professor of Radiopathology at Karolinska institutet in 1941.

From 1 July 1924, Sievert was employed as 'head of Radiumhemmet's physics laboratory' and each year sacrificed his pay and much more in support of the research activities at the laboratory for as long as he was employed by the Cancer Society.

Sievert's own memory of the meeting with Forssell and the first period at Radiumhemmet is noted down direct from his lecture in 1964. He begins with what happened in the USA:

I had already pointed out to Forssell that cooperation between medics and physicists was an unavoidable necessity. Actually, this was incredibly cheeky since I was no more than a young whippersnapper at the time, but I stuck to my guns, saying

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* The first premises that Sievert had available at Radiumhemmet was a 5 m^2 partition of the photographic laboratory.

** During 1924-1929, the physical laboratory's total area was approximately 50 m^2 and it consisted of an x-ray laboratory that took up the majority of the area, one small dark-room and a room approximately 10 m^2, which was used as an office and a laboratory for measuring radium preparations.

> that physicists should have a position alongside medics as consultants and not be subordinated to the medics. I believe that this has been a principle and I have tried to stick to that principle over the years, and I think it is a very important principle.
>
> In 1920, we did some work to clarify the doses around radium preparations. The method that was used a great deal at the time was to apply the radium preparation in one of these Kerr masses, plastic mass that was soft at 70°-80° and that was already hard when it reached body temperature, and where you positioned the preparation and performed irradiation as evenly as possible over the diseased site, over the tumour. Even gynaecological treatment was done this way. In these cases, it was a matter of inserting into the genitalia preparations of different sizes. With this surface treatment with radioactive substances there was no dosage, actually no dosage principle at all. You used to talk about milligram-hours, but it was actually only done on instinct. And the only person who really could do extraordinary things there was Professor Berven.
>
> We then designed an instrument to be able to measure the doses. [...} By then, my first assistant Paul Haglund, had also entered the scene,* and he did experiments up in the laboratory we had been given at Radiumhemmet. It measured five square metres. He sat there and persevered day in, day out and measured the radioactive substances. He must have received an awful lot of radiation, which did not seem to have caused him any harm.
>
> [One experience was that] when Berven wanted to administer the same dose to the surface, he also largely succeeded in doing so. It was of course his experience of the reaction of the skin and the experience he gained by having done so much of it himself and observed so many results himself. It was absolutely marvellous that he gave correct doses, i.e., equal doses where he wanted to have equal doses. It was actually really impressive. It was no doubt one of the reasons why it didn't take long for Radiumhemmet's results to become remarkably good.

Sievert's first appearance in radiophysics took place at a meeting of the Radiology Society at Lund General Hospital on 2 October 1920. Linköping radiologist *Torbern Klason* (1889-1979) writes about this in his history of the Radiological Society: 'you may remember a lecture on this area of treatment by the young and at that time slim physicist Sievert, on *The production of radioactive preparations for medical purposes through the concentration of radium emanation*'. The lecture, which was published in *Acta radiologica* in 1920 as Sievert's first publication, had been inspired by his visit to Failla in New York. Klason's allusion to the increase in Sievert's carcass with the passing of years is just one of many. A popular anecdote in this context originates from a celebratory dinner when one of the tribute speakers, Professor of Physics *Gustav Ising* (1883-1960), introduced his speech with the words 'Rolf Sievert has become similar to King Gustav II Adolf over the years...' and he then left a rhetorical pause while everyone glanced furtively Sievert, before continuing with '... surrounded by good colleagues!'

Sievert's first major task was to physically and mathematically describe the dose distribution around the radium preparations that were placed on patients for the treatment of superficial tumours. To calculate the radiation doses it was necessary to evaluate an integral that could not be arithmetically evaluated but had to be graphically estimated; it would be called *Sievert's integral*.** In his first paper on the intensity distribution of gamma radiation around radium preparations of different geometrical forms (*Acta radiol.* **1** [1921], pp. 89-128), Sievert shows a table of this integral. More comprehensive tables appear in his doctoral thesis in 1932. Richard Mould's *A Century of X rays and Radioactivity in Medicine* says that the evaluation of Sievert's integral 'was a turning point for theoretical dosimetry'.

In his paper, Sievert had discussed differences between his own results and those that were published by German physicists *Walter Friedrich* (1889-1979) and Otto Glasser. This led to an

* Sievert's first colleague, *Paul Haglund,* was employed in 1923 and was still working there and present when Sievert held his lecture in 1964.

** For anyone who may be interested, it is worth mentioning that the integral is $\int e^{-a/\cos\varphi} d\varphi$

exchange of views where Glasser maintained that Sievert used some formulae used by Glasser himself in his doctoral thesis in 1919, a thesis to which Sievert had not had access. However, one does get the impression that Sievert's argument had the upper hand in the correspondence.

In 1923, Sievert published an interesting work on secondary beta and gamma radiation (*Acta radiol.* **2**, pp. 268-300). After that, he began to concentrate less and less on the theoretical investigations and moved over to more practical work with instrument design and radiation measurement. 1926 was a year in which he was struck by several good ideas.

In a paper entitled 'A simple, reliable device for measuring deep doses', Sievert described the principle of the *condenser ionisation chamber* for the first time (*Acta radiol.* **5** [1926], pp. 468-470). Until that time, all radiation measurements were based on the capacity of x and gamma radiation to ionise air and had been taken using fairly cumbersome instruments. They consisted of an *ion chamber* in which an irradiated volume of air became electrically conductive, plus a measurement instrument to register the electric current or charge that could be transported through the chamber as a consequence of the irradiation. For measurements on patients during radiation treatment, the ion chamber had to be connected with the measurement instrument by means of a long cable so that the measurement instrument could be positioned in a protected place.

Sievert's idea was to connect the ion chamber to a capacitor (condenser) instead of to the measurement instrument. The ion chamber and the capacitor could then constitute one unit that needed no cable connection. Before the measurement, the capacitor could be supplied with an electric charge whereupon the voltage over the capacitor could be measured. When the ion chamber became electrically conductive during the irradiation, some of the charge from the capacitor was diverted. The extent to which the capacitor had been discharged could be ascertained after the irradiation by feeding it to an electrometer and reading off the remaining electric voltage. The change in voltage which reflected the loss of charge provided a measurement of the radiation dose.

The design was simple yet ingenious. The ion chamber capacitor could be made very small and handy and it could even be sent by post to be irradiated in remote places. However, when Sievert wrote his first paper on the new measurement method, it had not yet practically speaking reached its peak and was far from being 'simple and reliable', which it would gradually go on to become. The detailed description of the method, with a number of examples of different types of condenser ionisation chamber, came with the doctoral thesis in 1932. The production of a condenser chamber was not easy – it required substantial precision work and the parts that would carry the electric charge had to be insulated from the chamber using small amber insulators. The work was carried out by a particularly skilful instrument maker, *Ragnar Scheer.*

In the same year, Sievert reported the results of a number of ionisation measurements at different Swedish radiation treatment clinics (*Acta radiol.* **7** [1926], pp. 461-472). He measured the 'radiation dose' expressed in 'röntgens' as the unit had been defined by the German Röntgen Society (the international definition of the same unit did not appear until two years later at the international radiology congress in Stockholm in 1928; see Chap. 14). Sievert investigated how many 'Rs' corresponded to an erythema dose (SED) using three different radiation qualities determined by the filtration of x rays. For the softest radiation (very little filtration), he found that the values ranged from 220 to 600 R. For the medium-hard radiation, the range was 230-815 R and for the hardest radiation (filtered with half a mm of copper or zinc) it was 320-1400 R. There were thus strongly differing views on what a erythema dose was.

Sievert's conclusion was that the Swedish radiation treatment clinics needed their dosages supervised, as well as their possibilities of satisfactorily measuring the voltage and current strengths of x-ray tubes. He therefore created an outpatient measurement department to standardise the irradiation during x-ray treatment. In 1926, Sievert's first academically-educated colleague, *Robert Thoraeus* (1895-1970) was employed for this purpose.

In the following year, a large amount of Sievert's time was spent on preparations for the international radiology congress in Stockholm in 1928, where he would participate actively in both of the newly-formed committees, the 'ICRP' and the 'ICRU'.

While Sievert was finding his feet, radiology was developing quickly. Radiumhemmet gained a good international reputation through Forssell, Berven and Heyman, and radiation treatment of cancer became an esteemed treatment method. When King *Gustav V* (1858-1950) reached the age of 70 in 1928, Gustav V's Jubilee Fund was set up on the basis of a gift in excess of 5 million Swedish kronor collected from the Swedish population. The Jubilee Fund would provide support for the combating of cancer and for cancer research.*

So, 1928 was an eventful year in many respects. As regards Radiumhemmet, which strongly influenced Sievert's work options, two new phases were introduced. A new building was purchased on the opposite side of Fjällgatan as an annex, while the establishment of the Jubilee Fund made it possible for him to start planning a completely new, separate building.

A ward with 24 new beds and an x-ray treatment ward were set up in the annex on Fjällgatan. Sievert's laboratory now had one full floor of the annex with eight larger or smaller rooms. Thoraeus' outpatient measurement activities gained access to an x-ray laboratory in the basement in order to standardise measurements. Sievert was now able to employ more personnel. In 1929, *Arne Forssberg* (1904-1975) was employed as radiobiologist and biochemist and physicist *Sven Benner* (1900-1986) was employed one year later. The Benner - Forssberg - Thoraeus trio would be Sievert's most important colleagues for a quarter of a century.

However, in spite of the new annex, Radiumhemmet's premises were still far too small although the Jubilee Fund's resources did create hope for some improvement. King Gustav V had appointed a committee with the task of suggesting the way in which the Fund's finances could best be used for the stated purpose. In turn, the committee realised that the Cancer Society's board possessed the best expertise and asked it for proposals. The board again appointed a work group consisting of Forssell, Berven, Heyman, Reuterwall, Sievert and the Society's treasurer *Falkman*. The Jubilee Fund's committee gave this work group its blessing to contact the 1926 hospital study, which had plans for a big new hospital by Norrbacka (what would become 0Karolinska sjukhuset). The intention was for the new hospital to have a radiation treatment clinic right from the start. The joint discussions led to the possibility of this clinic being set up in cooperation with Radiumhemmet. The proposed name was *King Gustav V:s Jubileumsklinik*, or King Gustav V's Jubilee Clinic.

Following additional investigations and yet another committee, the King approved a 'Report and proposal for the use of King Gustav V's Jubilee Fund' in May 1929. According to the proposal, the fund would initially be used to create *Jubilee Clinics* for radiation treatment in Stockholm, Lund and Gothenburg.

In order to facilitate the research, King Gustav V's Jubilee Clinic in Stockholm would consist of a treatment and care clinic called Radiumhemmet as well as research institutions for radiophysics and radiopathology. The Jubilee Fund would contribute 1 million Swedish kronor for the radiation treatment clinic, 360 000 Swedish kronor for the research departments and 1.6 million Swedish kronor for the purchase of radium. The Cancer Society promised to contribute 1 million Swedish kronor for the clinic to be built and 150 000 Swedish kronor for the research departments. Radiumhemmet would be owned by the Swedish State and constitute the radiation treatment clinic at the planned Karolinska sjukhuset. The research institutions would on the other hand be owned by the Jubilee Fund. The complicated organisation required time-consuming negotiations between the State, the Jubilee Fund, and the Cancer Society and it took until 1933 for them to reach agreement.

The foundation stone for the new Radiumhemmet was laid by King Gustav V on 8 October 1934. This was unfortunately followed by a year-long strike that prevented the construction, so it was not possible to commission the new clinic until October 1937! The research institutions for radiophysics and radiopathology were finished in April 1938.

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* Not to be confused with *The Cancer Fund – The National Society against Cancer.* The Cancer Fund was formed in 1951 as a voluntary organisation to support research and education. It is financed by contributions and membership fees and now supports most of the Swedish research projects in cancer.

During the long preparation period 1929-1937, Sievert ran his activities in Radiumhemmet's annex on Fjällgatan. Following the radiology congress in 1928, his international contacts had been made and strengthened, and he had become well-known abroad. His new colleagues were also making good progress.

In 1931, Sievert published a report with Arne Forssberg on an investigation into whether different irradiation times to administer one and the same radiation dose also created differences in biological impact. They had irradiated eggs from fruit flies (*Drosophila*) and observed the frequency of eggs killed following irradiation. If the eggs were irradiated with 165 r, no difference was noticed for irradiation times varying from 2 to 1 800 seconds.

Thoraeus designed a *standard chamber* for the calibration of measurement instruments in the new unit 'r'. According to the definition of the unit, all of the ionisation caused by the electrons and secondary electrons released by the ionising radiation in a well-defined air volume had to be measured. A standard chamber for this purpose must therefore first and foremost provide a well-defined air volume. This can be done by using a diaphragm to limit the x-ray beam that is used for the irradiation, while determining the length of the section of the beam used by limiting the length of the electrodes collecting the released electric charge. The electrodes consist appropriately of two concentric cylinders, one with a small and one with a large diameter. The x-ray beam that is to be used for the irradiation falls parallel to the common axis of the cylinders but at a sufficient distance from both of the electrode cylinders so that no released electron will reach there. All ionisations thus take place in air as required by the definition 'r'. The distance that is required means that the exterior chamber - cylinder must have a large diameter, in Thoraeus' case approximately 40 cm for the chamber to be able to cope with the x radiation at tube voltages of up to 200 kV.

When Thoraeus had finished his standard chamber, he was prepared to write a doctoral thesis about it. Sievert who, as always, had many irons in the fire at the time and preferred to deal with the things which interested him at that moment, became concerned. He has told me that Thoraeus' dissertation plans were an element of irritation - after all, he had not defended a thesis himself. It did not look good, he thought, if one of his employees did this before the boss did, but he had no great desire to sit down and write a thesis, observing all of the academic requirements that were normally in place. However, he reluctantly began to organise writing a thesis and for a time period he devoted all of his resources to that purpose.

He passed Thoraeus on the finishing line. Sievert's thesis is published as *Acta radiologica's* Supplement 14, 1932 whereas Thoraeus' thesis constitutes Supplement 15 in the same year. Sievert's thesis is inventive and provides a serious introduction to the condenser chamber, with detailed accounts of its properties and examples of its use. The thesis was written in German and the table of contents on four pages bears witness to the in-depth report of all aspects of the subject.

With ion chamber measurements with very small volumes of air, the measurement results are dominated by the ionisation generated by electrons from the chamber walls. Since the walls do not have the properties of air and the release of electrons from the wall material depends on the quality of the examined x rays in a manner other than the electron release from air, a small ion chamber depends on the wavelength of the x rays in a different way from a large chamber and the measurement result cannot simply be stated using the röntgen unit. Such disruptions can be reduced by carefully selecting the material in the walls of the ion chamber so that it is more or less 'air equivalent'. A few years later, Sievert designed a small ion chamber with insulators made of amber (**b**ärnsten in Swedish) and walls of **g**raphite, which was therefore called the 'bg-chamber'. Thanks to the small volume of air, it was relatively insensitive and could therefore be used to measure high radiation doses during the radiation treatment of patients. These chambers became very popular and are known abroad as 'Sievert - chambers'.

Thoraeus' thesis was principally a detailed description of the standard chamber. Thoraeus was a man of detail without Sievert's imagination and inventiveness. His slightly pretentious and very accurate report of the calibration of a stopwatch put smiles on faces. One might happen to think that his description of the standard chamber failed to carry much weight, but there were other good points in the thesis, namely the account of the selective absorption of x rays in metal filters of different compositions. The investigation led to the *Thoraeus filter*, a metal filter in which layers of tin, copper and aluminium in said order filter out unnecessarily soft radiation during x-ray treatment.

Following the defence of said theses, Sievert's time was mainly taken up with preparations for the move to Norrbacka and with the plan drawings for the new building where the top floor would be Sievert's private apartment. Planning the layout of a new institution with an architect was something that appealed greatly to Sievert. He was very anxious to always be able to give an account of proposals that had been worked out as thoroughly as possible, preferably with complete architectural drawings, before others were to make decisions.

In 1937, the final year on Fjällgatan, Sievert's group consisted of ten people, himself included. In addition to the already mentioned Benner, Forssberg, Haglund and Thoraeus, a photograph from that time shows *Ragnar Bernstedt, Vera von Cronsteen, Ulla Forssberg* (Arne Forssberg's wife), *Anne-Marie Holm* and *Lily Wiksén*. Just before the move, an engineer by the name of *Axel Berggren* (born 1901) was employed, who would go on to be of invaluable help to Sievert in realising different design ideas.

In spring 1938, Sievert's group was able to take possession of the new building. As opposed to the building for radiopathology, it had no subterranean link to Radiumhemmet (or later to Karolinska sjukhuset). This was deliberate: Sievert did not want to take the risk of radon gas from Radiumhemmet leaking out over to his laboratory and contaminating sensitive measurement instruments. Not until 1996 would such a link be established.

13. INTERNATIONAL RADIATION PROTECTION AND RADIATION MEASUREMENT BEFORE 1928

As early as 1896, Wolfram Fuchs issued very reasonable radiation protection recommendations in the American *Western Electrician* magazine. Another American who was giving good advice and observations early on was *Elihu Thomson* (1853-1937), firstly a chemistry and mechanics teacher at the Central High School in Philadelphia and then going on to be one of the founders of the early electrotechnical industrial company Thomson-Houston Co., which was subsequently taken over by General Electric. Also in 1896, Thomson published his observations that radiation damage appeared to be unavoidable if the exposure was powerful, that the degree of damage increased with the scope of the exposure, and that the intensity of the x rays diminished in inverse proportion to the square of the distance.

Early in 1906, the British Röntgen Society held a meeting in London to discuss the need for a radium standard* as a basis for the standardisation of measurements of x radiation. The intention was for the constant gamma radiation from radium to be a suitable reference when someone wanted to state radiation doses for treatment with x rays. Following the meeting, the Röntgen Society appointed a committee consisting of Frederick Soddy, *Sir Oliver Lodge* (1851-1940)**, Sir William Crookes, Professor Rutherford and Professor *C.V. Boys*, who was chairman of the Röntgen Society at the time, to establish a unit for the activity of a radioactive substance.

The committee submitted a report in 1908 proposing that 1 milligramme of pure radium bromide should be seen as the standard and that the ionisation that the gamma radiation generated therefrom - after filtration using 5 millimetres lead - could be used as a measurement. This would mean that the ionisation generated by the x rays could be expressed using the radioactive standard and it would be possible to treat x radiation more quantitatively than before.

The international development then took over. At the international congress for radiology and electricity in Brussels in 1910, an International Radium Standards Commission was appointed under the chairmanship of Rutherford for the purpose of producing a radium standard, and Marie Curie undertook to produce one. This standard preparation was ready in 1911 and was stated on 21 August to consist of 21.99 milligrammes pure radium chloride contained within a thin glass tube. In March 1912, the Commission met in Paris and compared Marie Curie's standard with three similar standard preparations produced by *Otto Hönigschmid* (1878-1945) in Vienna, and found that they tallied within a margin of error of 1:300. The decision was then made to accept Marie Curie's preparation as the international radium standard and that it would be stored at the international bureau for weights and measures (Bureau international des poids et mesures, BIPM) in Sèvres. One of Hönigschmid's preparations, containing 31.17 milligrammes of radium chloride, would be retained as the secondary standard in Vienna.

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* 'Standard' was the word that was used; we in Sweden now usually say 'normal' or 'equivalent' in similar contexts.

** Sir Oliver, Professor of Physics and rector of the University of Birmingham, was a known popular science author. After 1909, he, like Sir Arthur Conan Doyle, began to devote himself to spiritualism and believed, after the war, that he had contact with the spirit of his son who was killed.

It was also agreed that there would be an arrangement for the production of national standard preparations for countries that wanted these. They would be produced at the *Institut für Radiumforschung* in Vienna. One of the preparations (no. 3) went to England to be stored at the National Physical Laboratory (NPL).

The radiology congress in Brussels in 1910 also proposed a unit for the quantity of radon, which was not as easy as radium to state in grammes. In honour of Marie Curie, the unit was called the *curie* and 1 curie was defined as 'the quantity of radon that is in radioactive equilibrium with 1 gramme of radium'.

The work plans for NPL for the years 1914-1915 again pointed to the need to be able to 'measure x radiation' with the following comments, according to *E.E. Smith* (1975):

> If tests could be devised which would serve as a means of securing an accurate measure of X-ray energy, they would be of great service to the medical profession. The problem of a practical unit of X-ray energy is not unconnected with that for radium gamma rays and, doubtless, if suitable tests for X-rays could be devised, they would, ultimate at any rate, be conveniently expressed in terms of the gamma rays from a radium standard.

The concept of 'radiation dose' did not occur. There was not yet that much interest in what happened in physical terms in the irradiated body – the interest was primarily in having a measurement of that which irradiated it. At the same time, i.e., just before the war, the British Röntgen Society appointed a 'Committee on Röntgen Measurement and Dosage' in which physicist *G.W.C. Kaye* (1880-1941) represented the National Physical Laboratory. Kaye had come to NPL from the Cavendish Laboratory just before the First World War but left the laboratory for military service when the war broke out, and did not return until 1920. The committee was revived in 1920, however, and was extended in 1923 to become a joint committee with the Physical Society and with Sir *William Bragg* (1862-1942) as chairman. The committee included Professor *F.A. Lindemann* (later Lord Cherwell)*, Professor *A.W. Porter,* G.W.C. Kaye, Professor *Sidney Russ* (1879-1963) and *E.A. Owen* (born in 1887). Owen, who had come to the NPL in 1912, became secretary of the committee. It was first called the British X-ray Units Committee but finally the British Committee on Radiological Units (BCRU) and came to play a significant role in the continued international development.

The 1951 Swedish Radiation Protection Committee meeting (a report on the organisation of radiation protection in Sweden) states in its report (SOU 1956:38) that Denmark was probably the first country to have introduced a licence requirement for work with x-ray equipment. It was prescribed as early as 1907 that a licence from an electric commission established by the State would be required. However, this regulation appears not to have been observed to any noteworthy extent. Denmark was still first to legislate in the area through 'The use of Røntgen Rays Act' of 15 April 1930 which authorised the Minister of the Interior to issue regulations on x-ray installations and the use of radium. It fell to *Sundhedsstyrelsen* (the Danish Health and Medicines Authority) to check that the Act and special regulations were being complied with.

In 1913, the German Röntgen Society (Deutsche Röntgen Gesellschaft), mentioned here as 'DRG', gave a number of radiation protection recommendations in a 'Merkblatt' (*Merkblatt über den Gebrauch von Schutzmassregeln gegen Röntgenstrahlen*). My free translation of the DRG's recommendations is as follows:

> 1. Repeated irradiation of the human body, irrespective of which body part, is dangerous and has already on many occasions led to serious damage and even death among radiologists. It is therefore essential for such persons themselves

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* An interesting account of a later conflict between *Sir Henry Tizard* (1885-1959) and *Frederick Alexander Lindemann* (1885-1957) is given in C.P. Snow's book called *Science and Government* (1960), in which Lindemann is depicted as inferior in scientific judgement and a poor advisor as regards the strategic bombing of Germany during the Second World War.

> and their superiors and employers ensure that adequate safety devices are available at the workplaces and that all these persons are properly instructed in the need to use these devices. The latter-mentioned can best be taken care of by providing this information sheet openly at all such workplaces.

Recommendations were then given on how thick the lead in effective radiation screens should be, and indicated that 'since lead is poisonous, the screen must be covered on both sides with material such as wood, paint or similar'. The following was added:

> 4. Even when you use such a protective screen, it is advisable, particularly when the protracted irradiation is being carried out, to keep as good a distance as possible away from the x-ray tube being used.

Finally, they said:

> 9. Every assistant, student or apprentice, every nurse and every other member of the assisting personnel is entitled to refuse to participate in the radiography work if the protection arrangements are inadequate. Such a refusal must never be used as grounds for dismissal. The same applies also to the personnel at factories and shops that produce or sell radiographic equipment, tools and x-ray tubes.

These early recommendations from DRG are surprisingly ambitious and would have completely prevented radiation damage among the personnel if only they had been followed. It is interesting to note the warning about the toxicity of lead and the statement regarding the right of the personnel to refuse to work if the protection was not satisfactory.

In England, Sidney Russ brought the development and the damage among x-ray personnel to the attention of the British Röntgen Society in 1915. The prevailing world war had heavily increased the use of x-ray examinations in fields and at military hospitals, often with personnel who knew far too little and who used risky devices. That year, the Society held its annual meeting on 1 June at The Cancer Hospital on Fulham Road in London under the chairmanship of *Sir A. Pearce Gould*. The subject of the discussion was how people who used x-ray apparatuses could be protected, and it was proposed that the Ministry of Defence should loan the Röntgen Society an x-ray apparatus so that it could supply the apparatus with suitable protection devices and thus demonstrate what ought to be done.

Two technical matters were discussed in particular depth. The first concerned lead glass. It had been found that the thickness of the lead glass was not a crucial indicator of its protection capacity; a thinner glass could sometimes provide considerably better protection than a thicker one. What made the difference was the quality of the glass and the weight by volume. The second matter was the necessity for self-protection against the secondary radiation that could come from a completely different place rather than the anode in the x-ray tube.

Agreement was reached regarding the statement that 'in view of the recent large increase in the number of x-ray installations, this Society considers it a matter of the greatest importance that the personal safety of the operators conducting the x-ray examinations should be secured by the universal adoption of stringent rules'.

The need for rules was made evident by the memory depicted by Kaye in the article entitled 'The Story of Protection' in the *Radiography* magazine in 1940: 'The elementary concepts of protection through distance and shielding are of course self-evident; the noteworthy factor is the length of time that it took to for them to be applied. [...] I remember very well many departments where you could see the bones in the hand against a portable fluorescent screen almost everywhere in the x-ray laboratory'. However, those who noted the Röntgen Society's statement were not civil authorities but the British Ministry of War's X-ray Committee which issued instructions for military x-ray personnel during the war.

In the early 1920s, the British public was concerned about newspaper articles about deaths that affected a number of well-known radiologists as a consequence of radiation damage that they had incurred during the war in spite of all instructions. This led to *Dr. Robert Knox* (1867-1928), chairman of the British Röntgen Society, writing to *The Times* on 29 March 1921 and proposing the appointment of an all-round committee to investigate the conditions. The proposal was effected almost immediately, which led to the establishment of the British X-ray and Radium Protection Committee, with *Sir Humphry Rolleston* (1862-1944) as chairman. Sir Humphry was Doctor of Medicine but Regius Professor of Physics at Cambridge.* One of the two secretaries was *Dr. Stanley Melville* (born in 1869), who would become Secretary-General of the 1st international radiology congress in London in 1925. Melville was Doctor of Medicine and working at St. George's Hospital in London. This X-ray and Radium Protection Committee also included G.W.C. Kaye.

The committee worked remarkably quickly and submitted a preliminary report in July 1921 and Memorandum No. 2 in December. Eric Smith's history of the events has the following to say about this:

> [T]here appears to be no direct link between the 1915 and 1921 recommendations that were produced in Britain, although there are common factors such as the Roentgen Society and Prof. Russ. The 1915 initiative was taken up by the Admiralty and War Office but seems to have died in the stress of the war until it was revised by Robert Knox's letter to the Times on 29 March, 1921. Presumably the earlier work enabled the newly constituted committee to work with incredible speed but that is about the limit of the link.

In the preliminary report there was an appeal for assistance for the continued work. The initial remark thereafter was:

> The danger of over-exposure to x-rays and radium can be avoided by the provision of efficient protection and suitable working conditions.
>
> The known effects on the operator to be guarded against are:
>
> 1. Visible injuries of the superficial tissues, which may result in permanent damage.
>
> 2. Derangements of internal organs and changes in the blood. These are especially important, as their earlier manifestation is often unrecognized.

Then followed recommendations on the limitation of working hours and extended holidays:

> The following precautions are recommended:
>
> 1. Not more than seven working hours a day.
>
> 2. Sundays and two half-days off duty each week, to be spent as much as possible out of doors.
>
> 3. An annual holiday of one month or two separate fortnights. Sisters and nurses, employed as whole-time workers in x-ray and radium departments, should not be called upon for any other hospital services.

This was followed by a number of technical recommendations for different types of radiation work. Dose limits were still an unknown concept, but very detailed recommendations were given regarding shielding and enclosure of the radiation sources. Bearing in mind the risk of electric shocks, it was recommended that concrete floors be covered with insulating carpets, that electric cables be insulated rather than left to hang loosely, and that all metal parts on the apparatuses must be earthed.

* Regius Professor (at Oxford and Cambridge) is usually a Professor whose seat of learning was once established by King Henry VIII.

In 1921-1925, a great deal also happened in other countries. In Norway, the Norwegian Radiological Society approached the government in 1923 with a request for protection provisions for radiological work to be issued. This took place when, on 16 May 1922, the Society had established a work group to put forward proposals for protection measures for x-ray and radium work. The group consisted of five people: *P. Amundsen, E. Berle, Ellen Gleditsch,** S.A. Heyerdahl and *L. Vegard*. Its recommendations were extremely comprehensive and must have been of great help when the Swedish Radiological Society took corresponding steps in 1928.

Provisions were issued in Italy on 18 July 1925 regarding protection for both radiologists and patients. In the Soviet Union on 9 September 1925, the People's Commissar established regulations for protection during x-ray work.

The development had now progressed so far that an international federation of radiologists was needed. However, the pressure did not come primarily from the radiation protection problems but the need to agree on the way in which radiation doses and the intensity of the x radiation could be stated. A proposal had already been made at the x-ray congress in Berlin in 1905 on the tenth anniversary of Röntgen's discovery to establish an international committee to select an appropriate unit that stated the intensity of the x rays, but the opinion was that the time was not ripe so they agreed to keep an eye on the matter instead.

When the borders were opened after the First World War, international contacts were resumed and physicists and radiologists made extensive trips. The meeting between young Rolf Sievert and Gösta Forssell on a trip to the USA in 1920 is just one example. The new contacts made it all the more evident that it was necessary to standardise the measurement methods where x rays were concerned.

Many different methods had been used to measure something that we would now call a radiation dose. However, it was possible to distinguish three different physical-chemical principles: chemical dosimetry, the blackening of photographic film and the measurement of the ionisation generated by the radiation in air.

The most commonly-used method for chemical dosimetry had been proposed by Austrian radiologist Guido Holzknecht and was based on the fact that barium platinocyanide tablets changed colour following irradiation. The colour was comparable with a colour standard which stated a dose. At the NPL, these colour changes were compared (the original Sabouraud tablets were used - see Chap. 10) with the ionisation caused by the radiation in a gold leaf electroscope. It was in turn possible to compare this ionisation with the ionisation generated by the gamma radiation from a known radium preparation in the same electrometer.

Before now, the simplest way of stating the intensity of the x rays had been to state the current strength and voltage of the x-ray tube and work on the basis that the radiation intensity decreased with the distance according to the inverse square law. The rough estimate was that the intensity was proportional to both the current density and the square of the top voltage over the tube (see Chap. 10).

More direct use of the ionisation as a measurement of a 'radiation dose' did not take place at the NPL. On the other hand, 'dose' units based on the ionisation of air had already been proposed in France in 1908 by Paul Villard (who discovered gamma radiation) and in the USA in 1914 by *William Duane* (1872-1935) at Harvard University (Duane had been a scholarship researcher at the Curie laboratory in Paris in 1907-1913).

The question of measurement methods for the intensity of the x rays and of the 'radiation dose' arose with great emphasis when the radiologists of the world gathered in London for the first international radiology congress in 1925. The British Röntgen Society played an important role when the congress was implemented. Kaye was a member of both the organisational and the programme committee, and Owen was secretary of the physics section. The congress gave the British Röntgen Unit Committee the task of creating an equivalent international committee. This would be what is now known as the ICRU, the International Commission on Radiation Units and Measurements, which was

* Ellen Gleditsch (1879-1968) was a well-known radiochemist who had previously worked for Marie Curie and written the book called *Radium och radioaktiva processer (Radium and Radioactive Processes)* with Swedish lady Eva Ramstedt in 1917.

thus 'born' in London in 1925. The discussions held during the congress led to a decision to discuss a dose measure based on ionisation in air at the next international radiology congress which was to be held in Stockholm in 1928 thanks to the good reputation of Gösta Forssell.

During the congress in London, preparations were also agreed for the establishment of a radiation protection committee to draw up recommendations that could be adopted during the congress in Stockholm. Sievert has written the following about the work with the preparations for this in a report on the history of radiophysics in 1960:

> In 1927-28, a great deal of the work at Radiumhemmet's physics laboratory was devoted to preparations for the 2nd international radiology congress in Stockholm in 1928. The preparations included drawing up an initial proposal for protection provisions in cooperation with a number of radiologists for work with Röntgen rays and radioactive preparations.

However, Sievert was also active in the matter of the standardisation of measurement methods and units. In May 1926, he and with Manne Siegbahn were appointed to the International Commission on Units and Measurements, the decision on the formation of which had been made in London the previous year.

The lack of generally accepted physical dose measurements meant that the radiologists had thus far worked on the basis of clinical experience. The reaction of the skin to irradiation became the primary indicator and guideline for dosing. The unit that was used as a measure was designated in Swedish as 'HED' (for HudErytemDos, cf. Chap. 12) and with the corresponding letters in other languages, such as 'SED' in English for Standard Erythema Dose. So, 1 SED was the radiation dose that only just caused erythema, i.e., a reddening of the skin.

In the early 1920s there were eager discussions about the need for limit values for the radiation that personnel in radiological work could be allowed to expose themselves to without the risk of serious injuries. One early proposal for a limit value was put forward in September 1924 by American physicist, *Arthur Mutscheller* (born in 1886) in a lecture at the annual meeting of the American Roentgen Ray Society, entitled *Physical Standards of Protection Against X-Ray Hazards*. The lecture was published in 1925 in the *American Journal of Roentgenology.*

Mutscheller introduced a 'tolerance dose', i.e., a radiation dose that ought to be possible to be subject to without a risk of injury, of the magnitude of 0.01 SED per month. In 1925, Sievert realised that a yearly dose of 0.1 SED ought to involve no danger. In 1928, English radiologist *Alfred Barclay* (1876-1949) and his assistant *Sidney Cox* published a paper (in the *American Journal of Roentgenology*) in which they estimated the tolerance dose at 0.08 SED per year. Since none of these authors appear to have known the others, the information tallies surprisingly well, particularly bearing in mind that all estimations were based on very unsafe hypotheses. With today's measurements and knowledge, 0.1 SED per year ought to correspond to between 600 and 900 millisieverts per year (to compare this with today's recommended annual dose limit of an average of 20 millisieverts over a longer period for those who work with radiation sources).

Another important step forward in the 1920s, which would eventually come to influence the international radiation protection work, was the discovery in 1927 by geneticist *Hermann Muller* (1890-1967) that x rays could generate changes, *mutations*, in hereditary properties. Muller gave an account of this at the 5th international genetics congress in Berlin in the same year. His experiments were performed on banana flies and came to initiate extensive genetic mutation research. Muller won the Nobel Prize for Medicine in 1946 for his discovery.

Not until the 1950s would the capacity of radiation to influence hereditary properties come to influence the endeavours to protect against radiation. Muller's methods were on the other hand put to use at an early stage within heredity research, particularly in Sweden through endeavours made by *Gert Bonnier* (1890-1961), Professor of Hereditary Genetics at Stockholm University, and within plant breeding *Åke Gustavsson* (1908-1988), originally Professor at the State Forest Research Institute and the University of Lund, poet and essayist. Both Bonnier and Gustavsson would later cooperate with Sievert on matters concerning radiation protection.

14. ICRP, ICRU AND NCRP: IN THE BEGINNING

The grand old man of American radiological protection was *Lauriston S. Taylor* (1902-2004), who was still full of the joys of spring at the ripe age of 100. Taylor had come into the field in 1927 when he was employed at the National Bureau of Standards (NBS) in Washington. In that same year, he was visited by Kaye from the National Physical Laboratory in England. Kaye had been asked by the radiology congress in London in 1925 to try and find a number of physicists who might conceivably be interested in radiation protection to come to the congress that was about to be held in Stockholm in 1928. He knew that the NBS was the American equivalent of the NPL and he initially looked for a suitable American physicist there. This resulted in Taylor being given the task of travelling to Stockholm to represent the NBS.

Kaye, a careful planner, asked Taylor to visit a pair of German physicists, Friedrich Theseuer and *Gustav Grossmann* (1878-1957) in Berlin before coming to Stockholm. Grossmann had been employed by Siemens & Halske AG in Berlin since 1911 and had been head of research at Siemens-Reiniger-Veifa Co. (which developed electro-medical apparatuses) since 1919.

At a symposium on the subject of Fifty Years of Radiation Protection in French Lick, Indiana in September 1978, Lauriston Taylor gave a lecture from which the following has been obtained (see Tayl., 1980):

> Germany had a problem somewhat similar to the one we had in the United States. There were at least two groups in Germany, each developing some philosophy toward radiation protection, never quite getting their heads together, so they had no unified position ready to present at the meetings in Stockholm. That was also the position of the United States. There were at least three groups in the United States that had different thoughts with regard to what protection recommendations should be made, so there was no consolidated position in Germany or the U.S. Puzzling to me, as far as the U.S. was concerned, was that there were indeed three committees here concerned with radiation protection and measurements. Really, I should put it the other way around, measurements and protection, because measurements were really the big problem at that time. Until you solved measurement problems, you could not deal very quantitatively with protection.
>
> Anyway, among these three committees, there were divided up some nine or ten individuals representing the total number of people in the United States who had any background or interest in the field. So there was a crisscross among committees making about 75 percent overlap, yet all three sometimes came up with different recommendations with regard to radiation protection standards and that was the front that we presented in Stockholm.
>
> So I met with Grossman. Grossman was a very likeable person and I visited in his home and talked about many things. However, we did not reach any very satisfactory set of agreements as to what we might present as positions in Stockholm. I do not remember the details of that, but I do remember about that evening. Because I was a young visitor from the United States, going to have dinner in their home, they felt they should do something special for the occasion. So they did: they served corn-on-the-cob. The thing was, they had not learned at that time in Germany that there are two kinds of corn-on-the-cob, one for cattle and one for people. If you want some fun, just

munch down a whole ear of good, tough cattle corn, and then thank your host for it. However, he had to get it down, too.

We finally got to Stockholm, where we caught up with Kaye and Stanley Melville, two people who were designated apparently to be the Honorary Secretaries of some international protection body, if one was indeed formed. Mind you now, there was no official body at this time. We had to form one. Prior to the meetings, the British had presented a rather complete set of proposals for Congress adoption. These were based largely around those that had been adopted in 1921 or so and were also the American ones. It has always been uncertain in my mind as to the latter's significance; these were the recommendations that were attributed to the American Roentgen Ray Society, but they were, with the exception of the last paragraph or two, identical with the British ones, and they were contained in a sort of lecture by Pfahler.* So I was never quite sure of whether they represented something official or not. Then the Swedes** had some fairly detailed recommendations which also closely followed the British. Thus, as far as the meeting in Stockholm was concerned, there was some degree of accordance, at least between the British and the Swedes.

I will continue to quote Lauriston Taylor, who speaks from the heart about the meeting at which he and Rolf Sievert met for the first time.

Incidentally, I should mention in connection with the proposals attributed to the American Roentgen Ray Society, there were two people in this country of that early period to whom we probably owe more than any other two people I can think of in the field of radiation protection. One is Dr. George Pfahler, a radiologist from Philadelphia; and the other was Dr. Shearer,*** a physicist from Ithaca, New York, and Cornell. These two individuals began really pushing hard on radiation hazards and protection, beginning about 1916; and it was they who heavily pushed in this country for some kind of action on the matter of radiation protection standards.

Well, to go back to Stockholm, we now had Kaye, Melville, Grossman, and me. Sievert was involved, but he was also involved in the units committee meeting at the same time, so there was little personal contact with him until after the ICRP had actually been formed and he was named as a member. The four of us were trying to thrash out some of the differences to see what agreement we could arrive at, made up of the British and the Swedish proposals. At the time, Grossman was being a little difficult. Now Grossman, of the group of us, was the only one who happened to have his wife along. The four of us were having an informal session in the corner of the Great Hall in the Stockholm City Hall where a large opening reception was going on. Grossman was obviously very uneasy with his wife being left by herself while we were talking shop. Finally, Kaye sensed that he was restless and concerned about his wife and suggested that maybe if we could do something to ease his concern, we could get along better with our conversation. I do not remember exactly how we put it, but as I had met her before and knew what I was doing, I asked her if she would like to dance for a while. She was an absolutely delightful dancer and we danced to the music while Grossman settled down. Pretty soon I got a signal from Kaye that everything

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* *George Edward Pfahler* (1874-1957) was one of the USA's best-known radiologists and was active in Philadelphia. He was very interested in radiation protection and had already introduced diaphragms and collimators in 1903 to avoid the unnecessary irradiation of patients and lead filters to filter out unnecessary soft radiation. In 1919, he introduced photographic film as a dosimeter for the personnel.

** On 31 March 1928, the Swedish Society for Medical Radiology wrote to the Medical Board and suggested that an enclosed proposal for provisions be formalised and that a special inspector be appointed with the support of the Workers' Protection Act. The Society returned on 27 April of the same year with a request for the Medical Board to set special requirements for those who used x-ray radiation and radium for medical purposes.

*** *J.S. Shearer* (1865-1922) was a physicist who was doing x-ray work at Cornell University. He subsequently dedicated himself to an x-ray school for the army in New York.

> was all right and so I came back with Mrs. Grossman. Dr. Grossman was delighted and pleased and later took me out to dinner. From then on, things moved along very well.

The radiology congress was held in Stockholm on 23-27 July 1928 with Gösta Forssell as President and Lars Edling and James Heyman as Deputy Presidents. The Secretary-General was *Axel Renander* (1895-1968), radiologist at Sahlgrenska sjukhuset in Gothenburg at the time and subsequently hospital doctor in Västerås. Forssell (as chairman), Renander, Edling, Heyman, Berven and Sievert became part of the congress' Board of Management. A special exhibition had been arranged in the Liljevalch art gallery where no fewer than 45 different companies showed apparatuses and instruments for medical radiology.

The congress was opened with a ceremony in Stockholm's concert hall with the presence of Crown Prince Gustav Adolf. Professor Forssell gave a welcome speech in German (while the Crown Prince spoke English) and had the following to say about x rays and radium (as translated somewhat freely by me):

> It is a well-known fact that a very considerable danger for the professional is associated with the handling of humanity's new benefactors. This was especially the case from the outset before sufficient experience had been gained of the danger to life that is associated with them. Many valuable lives have been lost - many diligent workers within Radiology have been slowly led toward their deaths by this invisible benefactor and enemy. [...] May we honour their memory by following in their footsteps and supporting their efforts.

The radiology congress in Stockholm was attended by 964 participants from 46 countries or regions. It may be of interest to see the names of some of the participants from countries other than Sweden. The following 38 congress participants have all played a role in the history of radiology and radiation protection. It was not that long after the Stockholm congress that I myself met twelve of them and got to know five of them well (Mayneord, Jaeger, Failla, Holthusen and Taylor) and two of them very well (Failla and Taylor). Lauriston Taylor and Val Mayneord (who came to play a major role within the ICRP after the Second World War) were both just 26 years of age and, according to what Taylor told me, were referred to as the Congress Babies after a few days. Rolf Sievert had reached the more mature age of 32.

Denmark:	P. Flemming Møller	Germany:	G. Grossmann
England:	A.E. Barclay		H. Holthusen
	G.W.C. Kaye		R. Jaeger
	W.V. Mayneord		H. Küstner
	S. Melville		H. Meyer
	E.A. Owen		B. Rajewsky
	R. Paterson	USA:	C.B. Braestrup
	H. Rolleston		G. Bucky
France:	A. Béclère		W. Duane
	C. Regaud		G. Failla
	I. Solomon		O. Glasser
Holland:	A. Bouwers		B.H. Orndoff
Italy:	F. Perussia		C.F. Potter
Japan:	M. Nakaidzumi		E.H. Quimby
Norway:	S.A. Heyerdahl		A. Soiland
Switzerland:	H.R. Schinz		K.W. Stencurrent
Germany:	H. Behnken		L.S. Taylor
	F. Theseuer	Austria:	G. Holzknecht
	W. Friedrich		R. Kienböck

The radiology congress' executive committee and thereafter the congress in its entirety approved the formation of an International X-Ray and Radium Protection Committee, the organisation that would later be called the ICRP.* The minutes from the congress' general meeting are published in *Supplement 3* of *Acta radiologica* (1929). According to the minutes, the new committee was made up of the following:

Rolf Sievert, Stockholm, chairman
G.W.C. Kaye, London, honorary secretary
Stanley Melville, London, honorary secretary
Giulio Ceresole, Venice
G. Grossmann, Berlin
I. Solomon, Paris
L.S. Taylor, Washington

Even through Sievert, as one of the hosts in Stockholm, enjoyed the courtesy of being appointed as chairman of the committee, it was Kaye who actually steered it in practice as a consequence of ambition, age and experience and Taylor, as the youngest, who took notes and acted as secretary.

The recommendations that were agreed are depicted in the 1929 supplement to *Acta radiologica.* There are four sections with a total of 41 short paragraphs. The introductory paragraph reads as follows:

> 1. The dangers of over-exposure to X-rays and radium can be avoided by the provision of adequate protection and suitable working conditions. It is the duty of those in charge of X-ray and radium departments to ensure such conditions for their personnel. The known effects to be guarded against are:
> (a) Injuries to the superficial tissues;
> (b) Derangements of internal organs and changes in the blood.

The first section consisted of a paragraph containing recommendations on the restriction of working hours:

> 2. The following working hours, etc. are recommended for whole-time X-ray and radium workers:
> (a) Not more than seven working hours per day;
> (b) Not more than five working days a week. The off-days to be spent as much as possible out of doors;
> (c) Not less than one month's holiday a year;
> (d) Whole-time workers in hospital X-ray and radium departments should not be called upon for other hospital service

The reason for wanting to restrict the working hours was partly because this would also reduce the exposure to radiation and partly because the most feared damage - destruction of the blood-forming organs with aplastic anaemia as a consequence – were considered to increase the risk of infections and other diseases. If you could have time off and be outside in the fresh air, you were expected to achieve a better level of resistance. We should remember that one month's holiday in the 1920s was a considerable holiday extension for most people and that even a five-day week was something new. We

* ICRP now stands for the International Commission on Radiological Protection, a non-government organisation that has been active since 1928. The ICRP is the international body that issues recommendations regarding a fundamental radiation protection policy. Its recommendations form the basis of the more detailed instructions that have been drawn up by special bodies such as the World Health Organisation (WHO), the International Atomic Energy Agency in Vienna (IAEA), the International Labour Organisation in Geneva (ILO) and the OECD's Nuclear Energy Agency (NEA) and the provisions that are issued by national authorities such as (in Sweden) the National Radiation Protection Institute. The sister organisation (the ICRU - International Commission on Radiation Units and Measurements) has corresponding tasks as regards the orders of magnitude, units and measurement methods for radiation and radioactive substances.

now realise that the recommendations in the 2nd paragraph are a reasonable requirement for *all* work, not just radiation work.

The next section contained seven general paragraphs on the layout of the premises for work with x-ray apparatuses. The same thinking as that which lay behind the working hour restrictions came to the fore again: it was considered to be beneficial to have light and fresh air. It was recommended that all x-ray departments should lie above ground and that all rooms, including dark spaces, should have windows ‘offering good natural illumination’ and ‘sunshine and fresh air whenever possible’. All rooms should also have ventilation devices that replaced the air at least ten times an hour. Air inlets and outlets should be positioned so that the air current crossed the rooms.

The rooms in the x-ray departments should be large enough to allow the appropriate positioning of apparatus. The x-ray rooms should be at least 25 m^2 and dark rooms at least 10 m^2. The height of the ceiling should be at least 3.5 metres. A room temperature of 18°C was desirable in the x-ray rooms. If possible, the high voltage aggregate should be positioned in a different room from the x-ray tube.

This section was followed by a third section containing 13 paragraphs of recommendations on radiation protection during x-ray work. Still no dose limits were stated - the unit for a ‘radiation dose’ had only just come out. The operator of the x-ray apparatus did not have to expose himself unnecessarily to the primary x rays (it was understood that this could sometimes be necessary). The operator would maintain as great a distance from the x-ray tube as possible. The radiation screening would be good enough for an eye that was well adapted to the dark to find it impossible to detect ‘any appreciable’ fluorescence on a fluorescent screen that was positioned where the operator usually stood. The x-ray tube would as far as possible be surrounded by a material whose protection capacity was stated in the recommendations.

During diagnostic x-ray work, the operator would be protected against the spreading of x rays through an extra protective screen with a protection capacity corresponding to 1 mm of lead. During radiation treatment, the operator would remain out of the treatment room altogether behind a wall with a protection capacity corresponding to at least 2 mm of lead. It was recommended that penetration work (where the radiologist directly viewed a fluorescent screen behind the patient) should be carried out as quickly as possible and using the smallest possible diaphragm and the least possible radiation intensity. The lead glass between the fluorescent screen and the observer should have the same protection capacity as the lead surrounding the x-ray tube (requisite thicknesses were stated in a table). During radiation treatment, there should be a safety device to render the use of the x-ray tube impossible without a metal filter to filter out the softest radiation.

The fourth section consisted of five paragraphs on protection against the high electric voltage. The floor should be covered with an insulating material such as wood, rubber or linoleum. Permanent high voltage leads should be positioned at least three metres above the floor and be designed so as to generate no corona (i.e., discharge through ionisation of the surrounding air). Parts of the equipment that were not live should be earthed. Circuit breakers should be fast-acting and the electric fuses must not be slow. A voltmeter should state the tube voltage.

The final section contained nine paragraphs on work with radium and five on work with radon. It was initially ascertained that the protection must refer to beta radiation against the hands as well as gamma radiation against the whole of the body. The best protection against the hands was considered to be sufficient distance, which is why radium preparations should be handled using long tongs, preferably made of wood. During transportation, radium should be carried in boxes that had at least 1 cm of lead around the preparation and a long carrying handle. All work should take place in the shortest time possible.

When the radium preparation was not being used, it should be stored in a storage area at as great a distance as possible from all personnel. In the storage area, each preparation should be contained in a separate block of lead with a thickness of 5 cm for each 100-milligramme preparation. A special room should be available for work with putting together applicators consisting of several preparations. Measurements should be taken in another separate room where radium should be present only while the measurements were actually being taken.

Nurses and assistants would not be able to stay in the same room as the patients during their treatment. All unqualified work that can be learned in a short space of time ought to be carried out by temporary staff, although these ought not to be used for such work for periods of more than six months. This should apply especially to nurses and the people putting together applicators. Radium salt being sent by post ought to be distinguished still further.* If there were small quantities, the container ought to be surrounded by at least 3 mm of lead. Larger quantities ought preferably to be transported by hand in special transportation containers.

The subsection on radon initially stated that people must protect themselves against both beta and gamma radiation in the same way as with radium. It also recommended that the radon be handled in its 'relatively inactive condition' to start with; radon itself emits neither beta nor gamma radiation. These first types of radiation come from radium B (lead-214) which has relatively insignificant activity for the first ten minutes; the significant beta and gamma radiation come from radium C (bismuth-214) and after half an hour has still reached only a small fraction of its maximum intensity).

A warning was issued about the leakage of radon (called 'emanation' at the time), and the recommendation was for the room in which the preparation was produced to have good ventilation. The radium from which the radon was pumped away for use in enclosed capsules would be stored in a special room separately from the 'pump room'. The radium ought preferably to be stored in a lead box whose walls were approximately 17 cm thick for the quantity of approximately one gramme, which was the usual quantity for the purpose.

As mentioned, Rolf Sievert also participated in the meetings of the International X-Ray Unit Committee (later known as the ICRU) about which a decision was made in London in 1925 and to which Manne Siegbahn and Sievert were appointed as members in 1926. At the proposal of the congress' executive committee, Siegbahn was elected as chairman of the ICRU,** with E.A. Owen and Hamburg radiologist *Hermann Holthusen* (1886-1971) as honorary secretaries. The new unit committee was significantly larger than the radiation protection committee: it had no fewer than 24 members, which shows the level of interest in the issue of the unit at this time:

L. Arntzen	Denmark	H. Holthusen	Germany
H. Behnken	Germany	M. Nakaidzumi	Japan
R. T. Carreras	Spain	N. Nemenow	The Soviet Union
J. Grau Casas	Spain	E.A. Owen	England
A. Dauvillier	France	M. Ponzio	Italy
W. Duane	USA	V. Posejpal	The Czech Republic
E.C. Ernst	USA	E. Pugno-Vanoni	Italy
N.S. Finzi	England	H.R. Schinz	Switzerland
K. Gawalowski	The Czech Republic	M. Siegbahn	Sweden
R. Gilbert	Switzerland	R.M. Sievert	Sweden
H.M. Hansen	Denmark	F. Sluys	Belgium
S.A. Heyerdahl	Norway	I. Solomon	France

Of these, only Holthusen and Sievert would play any major role in the development of radiation protection after the war. Sievert and Solomon were members of the ICRP and the ICRU.

* Radium preparations in the post could cause concern even though there was a very small risk of anyone being harmed. In his history of Swedish radiophysics (1974), Sven Benner tells of the simultaneous early postal transport of radium needles and Sievert's new ionisation chamber for measuring radiation doses at the start of the 1930s: 'The small, spherical chambers were used for radiation measurements by post, probably the first in the world. Occasionally it could go wrong, as one day when I sent some radium needles to Lund (there were no regulations against this!) and the following day a box containing a small chamber. For some reason, the radium package was not released on the day it arrived so the chamber package was lying next to the radium in the hospital's postbox. That measurement had to be discarded'.

** Later on, I will use the present designations of ICRU and ICRP for the two committees although the new names were not established until 1950.

In 1928, the unit committee's recommendations were considerably more concise than the radiation protection recommendations, just eight paragraphs. The first three paragraphs were commendably short. The recommendations were:

1. That an International Unit of X-radiation be adopted.
2. That this International Unit be the quantity of X-radiation which, when the secondary electrons are fully utilised and the wall effect of the chamber is avoided, produces, in one cubic centimetre of atmospheric air at 0° C and 76 cm mercury pressure, such a degree of conductivity that one electrostatic unit of charge is measured at saturation current.
3. That the International Unit of X-radiation be called 'The Roentgen' and that it be designed by the letter small 'r'.

The new unit was thus not a unit for a radiation dose in the current sense but a unit for a measure of the capacity of radiation to ionise air. This magnitude would later be known as 'exposure'. Its nearest equivalent among more usual physical orders of magnitude can be said to be energy flux density, i.e., the amount of energy per unit area that flows towards the irradiated body. The exposure would be a direct measure of energy flux density if one and the same amount of energy per unit area always ionised the same number of air molecules, which is not the case. Exposure was chosen as the magnitude because the ionisation of air was significantly easier to measure than the energy flow or the energy that was absorbed by the irradiated body.

However, there was no success in explicitly defining which physical magnitude was referred to by 'the quantity of x radiation that...'. It was indirectly called the 'dose' (also 'dosage' in English) by recommending that the measurement instrument be called dosimeters (English 'dosage meter', French 'dosimètre', German 'Dosismesser'). The word 'dose' thereby retained the same importance as it had within medicine, and the word originates from the Greek *dosis*, which means 'as much as is given'. In other words, in the case of radiation, 'how much radiation' collided with the irradiated body at each point, measured through the air ionisation that the radiation would have produced in 1 cm^3 air at that point.

However, there was one essential difference compared with a dose of a medicine. If the medicine in taken by mouth, on each occasion it will be distributed in the same way in the body's different organs and each dose is a dose for the whole body. Two equal-sized doses therefore amount to the same as one dose that is twice the size. As with mass and energy, therefore, a dose of medicine is an *extensive* magnitude and such orders of magnitude can be added up.

The radiation dose, to the extent that it does not always concern the whole body, is on the other hand, as with temperature, an *intensive* magnitude. Such orders of magnitude cannot be added up (you do not say that you have been given 75° C if you received 37° one day and 38° the next day). Radiation doses can certainly be added up from day to day if the dose always concerns the same body part, but if you have irradiated a hand with 1 r and also a foot with 1 r, you cannot say that 'the body has received 2 r', nor even that 'the hand and foot' have received 2 r, only that each of the two body parts has received 1 r.

So, when we say radiation dose, we are not now saying that which has radiated in towards the body. Instead, we mean the energy that has been taken up (absorbed) per mass unit in the part of the body that has been irradiated. The unit that is used at the moment for such an *absorbed dose* is joule per kilogramme (J/kg) but, where ionising radiation is concerned, this unit has been given the special name of the *gray* (Gy).* Roughly speaking, it can be said that an exposure of 1 r gives an absorbed dose of slightly less than 0.01 Gy, i.e., 1 Gy corresponds to slightly more than 100 r. There are now several dose concepts for radiation protection purposes and the unit name of *sievert* (Sv). It is still too early to

* After the English biophysicist *Harold Gray* (1905-1965).

discuss this; it is enough to say that 1 sievert for x rays is received through an absorbed dose of 1 gray, and that 1 r therefore corresponds to approximately 10 millisieverts (mSv), now a common designation. Before the 'gray' was introduced, the unit for an absorbed dose was the *rad*, so 1 rad = 0.01 Gy so that an exposure of 1 r gave an absorbed dose of slightly less than 1 rad.

Since the relationship between exposure and absorbed energy depends on the energy in the photons of the x rays, it was ascertained that each stated 'dose' would be incomplete if the quality of the x rays were not also stated at the same time. It was understood that 'the quality of radiation' was not a well-defined concept, it was also realised that it could be enough to state the top voltage over the x-ray tube and which filter and high voltage source had been used. In practice, it was sufficient to state the 'half-value layer' of the radiation, i.e., the thickness of a copper or aluminium layer required to reduce 'the quantity of radiation' to half. With reference to the fact that measurement methods were developing very rapidly, it was recognised that the stated recommendations ought to seen as provisional.

In this connection, the assignments given to the ICRP and ICRU committees were actually executed, and there were still no plans for continuous activities. On the other hand, the committees counted on being revived at the next international radiology congress. However, Dr. Kaye recommended that the members of the radiation protection committee attempt to organise national radiation protection work in their own country when they returned to their respective countries. This advice was taken *ad notam* by Lauriston Taylor who, on arriving home, contacted American radiologists and radiology societies with a proposal to appoint an advisory group for this purpose. Such a group met for the first time in West Baden, Indiana in connection with a meeting of the American Roentgen Ray Society. Taylor has given an account of this (see Tayl., 1980):

> This whole thing started off rather nebulously and I never have been quite sure, after a name was chosen - the Advisory Committee on X-Ray and Radium Protection - as to whom this committee was advisory. As we grew older, and by that I mean perhaps a year older, we began to realize it was probably just as well that we not define it. The original thought was that since I was to be the representative from [USA] to the next meeting of the ICRP, this "advisory" group would be put together to advise me as to what position to take at this particular meeting. However, the several radiological societies became so enamored of the general idea that they wanted to be advised, too, and they wanted to have a hand in it. So, we agreed we would work with the radiological societies also and, in fact, when the committee was set up, it was made up of people recommended by each of the four radiation medical groups in the country plus the x-ray manufacturers – two people from each. It stayed that way for many years, and these groups looked upon the Advisory Committee as advisory to them. Then the question came up as to how you operate a thing like this. Well, radiation protection was part of the official objectives of the Bureau of Standards at that time. It was part of the line item in the appropriation bill (fiscal year 1926, I believe) where the Bureau of Standards was to 'engage in the matter of radiation measurements and radiation protection' for which the line item would include the annual sum of $30,000. This was an incredibly large sum in those days. In fact, for the first year we could not spend it.
>
> The radiological societies took on an attitude that "you, Taylor, are in Washington, our Bureau of Standards has a mountain of money for radiation work, so why don't you handle the paper work and the labor for this and act as secretary or something." It sort of got started that way quite informally. Later on, I might say, the Bureau of Standards began to wonder if we were advisory to the Bureau of Standards. Beginning at that point we began to make it very clear that no, indeed, we were not advisory to the Bureau of Standards; we were an organization made up of people outside of the government with the exception of myself. The bureau of Standards could make what use it wanted to of our recommendations, but we were not going to let ourselves be entrapped and become beholden to any government organization. This was left in a very fuzzy way, which was grossly misunderstood as time went on, particularly as the Bureau of Standards continued to publish our reports for us as *Handbooks of the*

> *National Bureau of Standards*, but which officially carried no weight whatever. Mainly, it provided us with a means for publication and distribution.
>
> I have mentioned the early starting of this committee at the meeting here in West Baden. The first official meeting of the committee, after it had been organized, was in September, 1929, a year later. I think that meeting was in Toronto, but I do not remember much about the Toronto meeting except that at every meeting of every [radiological] society we would have a meeting of our Advisory Committee.* [...]
>
> I said the first meeting of this new 'advisory' committee was held in September, 1929. W. D. Coolidge, who was one of the delegates to the meeting representing the manufacturers, was designated as Chairman. He presided for two meetings and then decided that since the government was paying for much of this 'monkey business' at the time, they had better make me Chairman. That they did, and once the thing having gotten so started, it was awfully hard to get out from under. Many of you know this from personal experience. We began formal meetings in September, 1929.
>
> The first recommendation of the Advisory Committee (we now refer to it as the NCRP**) were put out in the spring of 1931, less than two years after the first meeting. It shows how simple life was in those days: pull a group of people together, pull together some ideas, develop a report, and have it published in less than two years - that is an incredible achievement.
>
> In that particular report, we went into considerably more detail than had the ICRP reports and this difference remained until 1950. The ICRP reports were sort of skeletons giving bare essentials and the NCRP reports gave rather substantial details.

In 1931, the League of Nations appointed a committee to study the problems with the risks of radiation and protection against radiation, a type of precursor to the scientific radiation committee (UNSCEAR) that would be appointed by the United Nations 24 years later. Two German radiologists, *Hermann Wintz* (1887-1947) and *Walther Rump* were tasked with writing a report which has now practically been forgotten: *Protective Measures Against Dangers Resulting from the Use of Radium, Roentgen, and Ultra-Violet Rays*. It is a well-arranged, well-written, reliable and exhaustive report of 114 pages, and it was published as a League of Nations report in CH-1054 in August 1931. It can be said to have been before its time: it described the problems that existed, the knowledge that was lacking and the protection policy that was recommended. Lauriston Taylor has called it absolutely superb and, in 1978, recognised that it could equally well have been written then. You cannot disagree with Taylor.

In the 1930s, three international radiology congresses were held: in Paris in 1931, in Zürich in 1934 and in Chicago in 1937. The ICRP was not active in the meantime but met in connection with each congress. The initial number of members was in practice limited to six since the Italian Ceresole did not appear to have participated; nor was he in Stockholm in 1928. 'I don't remember ever having seen him,' says Taylor. The number of members was kept deliberately low owing to the poor experience of the ICRU, which had consisted of 24 members in Stockholm in 1928. The ICRU's rules at the time allowed each country to have two people participating in its meetings. At the meeting in Paris in 1931, this resulted in the ICRU meeting being attended by 40 people of whom, according to Taylor, just 6-8 knew something about the subject. The fact that a group with such a composition is not particularly effective speaks for itself.

On 9 June 1931, Dr. Kaye sent the following invitation to those who had participated in the ICRP meeting in Stockholm in 1928 (according to Lauriston Taylor's big history of 1979):

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* This is where Taylor has misremembered the place, however. In his thick book on the ICRP, ICRU and NCRP, he says that the first meeting took place on Wednesday 18 September 1929 at the Pennsylvania Hotel in New York. The participants in this meeting were W.D. Coolidge, G. Failla, R.R. Newell, H.K. Pancoast, L.S. Taylor, W.S. Werner and F.C. Wood.

** Now the National Council on Radiation Protection and Measurements, formerly and initially the US Advisory Committee on X-Ray and Radium Protection, and then the National Committee on Radiation Protection and Measurement; the NCRP has long been the American equivalent of the ICRP.

> It is proposed that the International X-Ray and Radium Protection Committee shall meet at Paris on the occasion of the third International Congress of Radiology, with a view to considering in what respects, if any, the Recommendations promulgated at Stockholm should be modified. It appears to be the view that in the main these Recommendations have proved adequate for the purpose, and it is not suggested that they should be altered in their general outlines. Broad principles are aimed at rather than detail and it is suggested that if considerable detail is desired, it might properly appear in the various supplementary recommendations which most countries have drawn up to suit their particular requirements.

The committee met in Paris with two new participants: *Dr. R. Ledoux-Lebard* from France (who, out of courtesy, acted as chairman as Sievert had done when the committee had met in Sweden) and *E. Pugno-Vanoni,* Professor of Radiation Physics and Electrotechnology at the University of Milan.* Pugno-Vanoni succeeded Ceresole. Other participants included Grossmann, Kaye, Melville, Sievert, Solomon and Taylor.

The meeting resulted in a few insignificant changes to the 1928 recommendations and the following more important addition:

> X-ray and particularly radium workers should be systematically submitted, both on entry and subsequently at least twice a year, to expert medical, general and blood examinations. These examinations will determine the acceptance, refusal, limitation or termination of such occupation.

This was the first international recommendation for the medical examinations that would be so controversial later on. The original intention was to be able to use examinations of the blood to detect the start of any destruction of the blood-forming organs. During the 1930s, there were doubtless still many who were working with sufficiently poor protection to enable the result to be observed during the blood examinations. One reason for performing an examination before employing someone was to obtain the reference values for comparison against any subsequent deterioration in values. One reason to justify this was the fact that, from the employer's perspective, no-one wanted to employ someone whose blood was already such that its composition or other properties could later point to radiation damage and lead to a claim for compensation.

The full text to the modified radiation protection recommendations is depicted in *Acta radiol.* **12** (1931), pp. 586-594. The ICRU meeting that took place in Paris at the same time did not lead to any essential results, perhaps largely because of the large number of participants. Sievert had made fruitless endeavours to gain an audience for a proposal for a new unit for the intensity of radiation from a radium preparation called 'Imc' for 'Intensity millicurie', which he described in an article in *Acta radiol.* **12** (1931), pp. 300-304.

The year before the congress in Paris, the International Radium Standards Commission had expanded the curie unit to apply not just to radon but to all decay products of radium. On the other hand, people did not want to use the curie to state the quantity of the radioactive substances outside the decay chain of radium-226, e.g., mesothorium (radium-228) or thoron (radon-220). The *rutherford* (rd) unit was introduced for such nuclides, which would be the quantity of the radioactive nuclide that dissociates with a million disintegrations per second. Both the curie and the rutherford were still measures of the *quantity* of the radioactive substance and only indirectly a measure of its activity, i.e., the number of disintegrations per second.

If someone wanted to know the activity in a curie of the substance, that person could work on the fact that, in terms of definition, it was equal to the number of radioactive disintegrations per second in one gramme of radium. That number could be directly calculated as $((N_A/A)\cdot\ln 2)/T$ where T is the

* In 1928, Pugno-Vanoni, who was born in 1899, appears to have proposed the idea of a condenser chamber that was subsequently realised by Sievert, but Sievert probably new nothing of Pugno-Vanoni's proposal at the time.

half-life for radium-226 expressed in seconds ($5 \cdot 10^{10}$ s), N_A Avogadro's number ($6.02214 \cdot 10^{23}$ mol^{-1}) and A, the atomic weight, i.e., 226 for radium-226. If the half-life of radium was assumed to be 1600 years, this means that one gramme of radium-226 had an approximate activity of 37 billion disintegrations per second ('becquerels' as we now say).

The disadvantage of defining 1 curie as the quantity of a nuclide that had an equal number of radioactive disintegrations per second to that of one gramme of radium was that the number of radioactive disintegrations calculated per unit time ('the activity') would be dependent on the last estimation of the half-life of radium. The International Radium Standards Commission therefore already recommended the establishishment of an exact number for the activity of 1 curie in 1930 (the proposal was 37 billion disintegrations per second). Unfortunately in 1948, another organisation, the International Union of Physics and Chemistry, would recommend another value: 36 billion per second. The confusion ceased in 1950 when the number was agreed at 37 billion disintegrations per second, which has been the definition of 1 curie since then. 1 curie of radium-226 is therefore no longer exactly 1 gramme of radium.

The rutherford unit survived until the 6th international radiology congress in Copenhagen in 1953 when the use of the curie unit was extended to apply to all radioactive substances - both the artificial radioactive and the parent nuclide radium-226. The historical development has gone from the time when 1 curie meant the *quantity* of radon that was in radioactive equilibrium with 1 gramme of radium to the present time when we have had the curie and then the becquerel as measures of the *activity* of the radioactive substance, i.e., the number of radioactive disintegrations per unit time. The original concept of quantity lives on in everyday terms in regularly-used expressions such as 'an emission of 1 curie of iodine-131' or (which is now the same thing) 'an emission of 37 billion becquerels of iodine-131', as though the activity still referred to the quantity of the radioactive substance that had been emitted. Since the activity is now the number of disintegrations per unit time, one ought instead to say 'an emission of a quantity of iodine-131, such that its activity is 1 curie', etc., but this is a bit long-winded of course.

The actual quantity (weight) that corresponds to a specific activity, e.g., 1 curie (i.e., 37 billion becquerels) or 1 becquerel, varies with atomic weight (A) and half-life (T). It can be calculated as $8.86 \cdot 10^{-17}$ A·T kg/curie or $2.4 \cdot 10^{-27}$ A·T kg/Bq if the half-life, T, is stated in seconds. This is illustrated by the following example:

Nuclide	**Half-life**	**Weight at 1 Ci**	**Weight at 1 Bq**
iodine-131	8.1 days	7.5 microgramme	$2.2\ 10^{-16}$ grammes
radium-226	1 600 years	1 gramm	$2.7\ 10^{-11}$ grammes
uranium-238	4.56 billion years	3 tonnes	82 microgrammes

The quantity of matter in an activity of 1 becquerel is thus incredibly small.

An important step forward concerning the knowledge of measurement problems and measurement methods was taken in 1933 with the publication of the textbook called *Grundlagen und Praxis der Röntgenstrahlendosierung,* [The Basis and Practice of X-ray Dosage] written by Hermann Holthusen and *R. Braun*.

The next ICRP meeting, in Zürich in 1934, would be more important than the one in Paris since a radiation dose limit value for radiological work was now being recommended for the first time. I have already mentioned them before, similar to the proposals from Mutscheller and Sievert for a 'tolerance dose' expressed as 0.01 SED per month or 0.1 SED per year. Before the agreement on the 'dose' unit for x rays (r) in Stockholm in 1928, it had been difficult to translate SED into a 'dose' since many different proposals for dosage units appeared. Following the ICRU's Stockholm meeting, however, the investigation into how many Röntgen corresponded to 1 SED began and, on that basis, to state a dosage limit expressed in Röntgen instead of SED.

In 1927, German radiologist *Hans Küstner* had already estimated that 1 SED corresponded to approximately 550 'röntgen' as he defined the unit. In 1933, the American Advisory Committee (NCRP) reckoned that Küstner's estimation could also apply to the ICRU's new röntgen but rounded off the value to 600 r. Mutscheller's estimation of a tolerance dose of 0.01 SED per month thus corresponded to 6 r per month. 25 working days per month were then assumed and this came to 0.24 r per day, which was rounded off to 0.1 r per day. This was done partly to exercise caution (in correspondence with Taylor, Mutscheller had actually proposed that they work on the basis of 4 r per month, bearing in mind that the tolerance dose would apply also to softer radiation for which 1 SED corresponded to less than 600 r) and partly because they realised that there was such uncertainty that even 0.2 r per day would seem to be far too precise. The value of 0.1 r per day was recommended by the NCRP in 1934. This corresponds to approximately 250 millisieverts per year, which is comparable with the dose limit of 20 millisieverts which was recommended by the ICRP in 1990 for the average annual dose over five-year periods.The difference in size between that and the current dose limit is explained by the fact that in the 1930s, no-one had been bargained on any great risk of cancer or hereditary injuries. The proposed dose limit guaranteed protection against the injuries that were known of at the time and do not necessarily reflect any lesser protection ambition.

Prior to the meeting in Zürich, Dr. Kaye sent a letter to the members of the ICRP on 10 April 1934 with almost the exact same wording as the letter he had sent before the Paris meeting in 1931, and on 28 June he sent a summary of the proposals that he had received in response. In the latter communication, he referred to the committee as the 'commission', a step in the direction towards the current name. Kaye proposed that the following paragraph be added to the start of the recommendations (according to Taylor's history):

> The evidence presently available appeared to suggest that under satisfactory working conditions a person in normal health can tolerate exposure to x rays to an extent of about 0.2 international roentgens (r) per day. On the basis of continuous irradiation during a working day of seven hours this figure corresponds to a dosage rate of 10^{-5} r per second. The protective values given in these recommendations are generally in harmony with this figure under average conditions. No similar tolerance dose is at present available in the case of radium gamma rays.

The tolerance dose in the ICRP recommendation was not equally strongly rounded off like the value given by the NCRP (0.1 r per day) but it was derived from the same data, i.e., actually approximately 0.25 r per day, rounded down to 0.2 r per day (corresponded to approximately 500 millisieverts per year instead of the present 20 millisieverts per year). The need to state a corresponding dose rate came from the need to be able to state screening off requirements. Although one spoke of a 'tolerance dose', it was implicit that the radiation dose should not exceed 0.25 r per day. The fact that a longer period for the dose restriction (a week or a year) was not chosen, then or later, was probably because that medical radiological work could be assumed to be the same on one day as the next.

The meeting in Zürich took place on 26 July 1934. Of the former ICRP members, Kaye, Pugno-Vanoni, Sievert, Solomon and Taylor were present. Stanley Melville had passed away. Since the meeting was held in Switzerland, it had been assumed that the chairman would be Swiss, but as a general surprise, Dr. Leboux-Lebard arrived and took it for granted that his chairmanship from the Paris meeting still applied. At the same time, the Swiss had appointed two honorary chairmen, Professors *R. Bar* and *F. Tank.* They had also made a mental note of the ICRU's rule of allowing two representatives from each country to be present (although this was not assumed to apply to the ICRP), which meant that two additional Swiss were present.

Dr. Grossmann had vanished. In his place came *Hermann Behnken* (1889-1945). Behnken was responsible for radiation measurements at the Physikalisch Technische Reichsanstalt in Berlin-Carlottenburg and, at the end of 1920s, had designed several ionisation chambers for the standardisation of the measurement of x radiation and was thus a well-known person. Taylor had visited him there for almost a whole month in 1931. Behnken had been an officer in the First World War, which was something that he often mentioned with pride. He had complained to Taylor about the fact that, as he saw it, Jewish finaciers were responsible for all of the economic and industrial problems in Germany. When Taylor met him again in 1934, Behnken did not conceal his satisfaction with regard to the fact that the national leadership was now taking measures against the Jews. He showed with satisfaction the startled Taylor a playground where the Jewish children were kept separate from the 'Arians' and how they were forced to wear yellow bands. He told Taylor that Grossmann had been succeeded by him in the ICRP because Grossmann was Jewish and had been involved in 'Jewish activities'.

Taylor had found Mr. and Mrs. Grossmann to be pleasant people, and he looked for them. This was not easy in 1934, however - Germany was seething following the Röhm massacre, and it was not popular to ask the whereabouts of Jewish people. No-one who was asked said they knew anything about Grossmann or his wife, and Taylor noted that his questions were met with some awkwardness. He feared the worst.

It was obviously unreasonable for 4/11 of the commission to now consist of Swiss and for German authorities to have decided who was 'suitable' for the ICRP. A decsion was therefore made to clarify the membership rules with clear statutes. The new statutes said that the commission would consist of one representative from each country who were appointed *by the commission* to send a representative, plus a representative of each national laboratory invited *by the commission* to appoint a participant. This established the ICRP's opportunity to exercise a fair amount of independence in appointing its own members, something which has made the commission very efficient but which has also aroused criticism.

At the same time as the radiology congress in Zürich, a meeting of the international committee on illumination was held in Saint Moritz. Since the Bureau of Standards' member of this committee was prevented from attending, Lauriston Taylor was asked to participate in the meeting. Taylor has given a vivid account of the journey there:

> …normal transportation from one to the other would have been by train. However, Dr. Solomon from Paris had driven down with his wife and invited Kaye and his wife and me to ride with him to San Moritz. Not having ridden in a car with Solomon before, we accepted gladly and within five miles began to regret it. He had a large and heavy car but he was a veritable wild man at the wheel and we had to proceed along cork-screw roads over the Chur Pass across the Alps. He drove at break-neck speed and never hesitated to go around on the inside of a blind curve. When one considered the fact that there were probably Italians driving their cars equally wildly in the other direction you can imagine the state of our feelings by the time we had had half a dozen near misses. When we finally arrived in San Moritz, Kaye, who was a large and very sedate Englishman, got out of the car, sat down on the curb and burst into tears. He told me later that he would never ride in another automobile driven by a Frenchman.

Taylor found nothing to lead him to believe that the ICRP had any reason to concentrate on infrared or ultraviolet radiation. The ICRP's attitude to non-ionising radiation has continued to survive since then.

On the basis of the new rules for membership, it was decided in Zürich that in future, the ICRP's membership would be as follows:

R. Ledoux-Lebard	France (chairman)
G.W.C. Kaye	England (honorary secretary)
H. Behnken	Germany
E. Pugno-Vanoni	Italy
R. Sievert	Sweden
I. Solomon	France
F. Tank	Switzerland
L.S. Taylor	USA

The ICRP was still not active between the meetings, assuming that we discount the questionnaires sent by the energetic Kaye to its members months before the next meeing, which would be held in connection with the 5th international radiology congress in Chicago in 1937. One question that Kaye emphasised in his letter to the members on 16 February was the protection for the operators during distance treatment, i.e., with a radiation source outside the body comprising radium amounts in the gramme range.

The only proposal to Kaye that has been documented came from Sievert and Taylor. Sievert's proposal came too late to be discussed in Kaye's agenda for the meeting. Sievert realised that the dose unit 'r' was not suitable for radium dosimetry and prescribed the unit 'Imc' which he had proposed in 1931. It is perhaps slightly strange that Sievert took up this matter at the ICRP rather than the ICRU. It was evident that the 1928 definition of 1 'Röntgen' had referred only to x rays and that there was no corresponding unit for gamma radiation.

However, Kaye himself took up the matter within the ICRU of which he was also a member. In Chicago in 1937, this led to the ICRU changing the definition of what is now called the 'röntgen' with a small r. It instead ended up reading as follows:

> The röntgen is 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 designation 'r' for the unit was retained. One result of the new definition was that there was no longer any doubt that the recommendation of a tolerance dose of 0.2 r per day also applied to exposure for gamma radiation from radium.

Following the congress in Chicago, the clouds darkened once more and the Second World War would soon break out. Before the war, and realising that the international radiation protection work would cease for a while, Kaye, who would pass away in 1941, contacted Lauriston Taylor and asked him to act as 'chairman in preparation' until the situation returned to normal and the international work could be resumed. When this succeeded, of the original ICRP members, only Sievert and Taylor remained.

Taylor initially believed that Grossmann, who had disappeared since 1933, had been one of the Jews killed by the Nazis, but later said that Grossmann had moved to Turkey. However, I have found other information about Grossmann in German biographic reference books, information that increases your curiosity as to what did actually happen to him. It is said that he left his job with Siemens 'for personal reasons' in 1932, but we know from Taylor that the correct year was 1933, when Hitler came to power. The last sign of life that Taylor had from Grossmann was a letter of 23 June 1933 (Hitler had been installed as Chancellor on 30 January). The reference books also state that Grossmann worked as a 'private researcher' in Germany in 1932-1942. The reference books say that while doing so he developed a tomograph (apparatus for building an x-ray of specific layers of the body), which was marketed in 1934 by Sanitas in Berlin, but this achievement may have occurred before he left Siemens. However, you do find Grossmann's name in literature much later on. In the *Strahlentherapie* journal for 1938, there is an article by 'Dr. G. Grossmann, Berlin-Zehlendorf' on how to present spectra of x rays. He suggests there that it is better to state the distribution of the radiation intensity as a function of the quantity of energy than – as before - a function of the wavelength.*

In 1942, the reference books say that Grossmann returned to Budapest where he was born. If this is true, he jumped out of the frying pan into the fire. In the same year, the Germans demanded that the Hungarians tighten their anti-Jewish measures and, one and a half years later, the Arrow Cross leader Szálasi became head of the government, the persecution of the Jews culminated and Raoul Wallenberg began his life-saving activities. But Grossmann appears to have survived against all odds and created a new future for himself in his old age. The reference books do say that in 1954, at the age of 76, he

* When, while working on this book, I found this article by Grossmann from 1938, it was even more interesting since I actually wrote an article about the same thing fifteen years later. At the time, I did not know of Grossmann's article and believed that I was the first to come up with the proposal. My article had a great impact and was quoted in a few popular textbooks, but it is now evident that Grossmann's apparently completely forgotten article was the first and that he should be given the credit for it.

became head of cancer research in Budapest. A remarkable life story. He is said to have died in Budapest in 1957.

Other reference books say that Behnken 'fell in the final battles in Berlin', i.e., in 1945, and the same information is repeated by *Robert Jaeger*, the colleague of Behnken at PTR who later became active within the ICRP. Behnken must also have had some good sides since Jaeger, who was anti-Nazi, remained loyal to him. The information on his death in Berlin can be questioned, however. Taylor has told me that Scandinavian sources informed him that Behnken was killed in Norway by members of the Norwegian resistance when he led an occupation group. It is now probably far too late to find out the whole truth about either Grossmann or Behnken though.

Taylor says that Solomon – who was also Jewish – avoided the Nazis by dying in 1939 but that his wife, son and daughter were murdered by them. Melville had already died before the Zürich meeting in 1934. Pugno-Vanoni died at the end of the 1940s.

The radiology congress in Chicago would be the last for thirty years. In 1939, the Second World War broke out.

15. RADIATION DAMAGE AND DEATH RAYS

Bearing in mind that a good number of people were injured by x rays during the first few decades following Röntgen's discovery, many have been surprised that Röntgen himself avoided all injuries, especially as he had no knowledge of the risks of radiation to start with. He intimates the explanation for this in his second publication from March 1896 where Röntgen describes the way in which he positioned the measurement devices and himself in a metal hut partially covered in lead in order to protect his sensitive apparatuses against destructive radiation.

However, it was not long before reports of radiation damage were published. Radiation damage occurred even before x rays had been discovered since there were several others as well as Röntgen who experimented with cathode ray tubes. Emil Herman Grubbé experimented with a Crookes discharge tube in 1895 and suffered a troublesome erythema as early as January 1896. Grubbé lived until 1960 and reached the age of 85, but had to undergo a hundred or so operations on his hands. It was his own early erythema that gave him the idea of attempting, albeit unsuccessfully, to treat a case of breast cancer with radiation on 29 January 1896.

Wolfram Fuchs (who may have given the very first radiation protection recommendations) said that the 'röntgen burn' was no more dangerous than normal burn injuries and that 'when the röntgen rays collide with the skull for a longer period, the hair falls off but it does grow back with no unpleasant aftereffects'. This effect of x rays aroused early interest and was a phenomenon which could be used to indicate that the exposure was starting to become unpleasantly high. Hair loss, *epilation*, occurs with radiation doses of around 3 gray (I am using the current dose unit for ease of comparison). This used to be used to treat children suffering from 'ringworm', an affliction caused by parasitic fungi in the hair follicles. The head was irradiated with x rays at radiation doses of approximately 3 gray to the skin. When the hairs became loose, the fungi went with them.

The higher the radiation dose, the longer it is before the hair grows back and, at very high radiation doses, the hair loss is permanent. What happens with the hair therefore provides some indication of the level of the radiation dose to the skin.

However, the higher radiation doses also cause injuries other than hair loss. At radiation doses of between 4 and 8 gray, reddening of the skin starts to develop slowly approximately 4 weeks after the irradiation. The time delay is explained by the fact that many cells are not killed directly by the radiation, but have their reproduction capacity damaged so that they cannot generate viable cells when they subsequently divide.

This was the consequence of irradiation which the radiologists used to determine the dosage for radiation treatment. They sometimes tried the treatment methods by irradiating their own skin until it turned red so that they could then calculate the length of time for which they needed to irradiate a patient to destroy a tumour. The first reference point for 'radiation dose' was also 1 SED, the 'skin erythema dose'.

It was soon discovered that the skin was capable of enduring significantly more than 1 SED if the irradiation was spread out over a longer period. If you first administered ½ of an SED and then after a few weeks another ½ of an SED, the skin did not redden. The cells appeared to have a capacity to repair some of the damage produced by the first dose of radiation.

In 1944, *Magnus Strandqvist* (1904-1978), radiology consultant and later Professor at Sahlgrenska Hospital in Gothenburg, showed in a paper that attracted considerable attention (in Suppl. 55 of *Acta* *radiol.*) that he could derive an empirical mathematical connection between the radiation dose required for a certain degree of effect on the skin and the period over which the irradiation was spread. Since then, far more sophisticated formulae (such as the Ellis formula and the Kirk formula*) have been stated to describe the connection with both the irradiation period and the number of fractions into which the radiation dose is divided.

If the radiation dose is administered all at once but exceeds 1 SED (i.e., 4-8 gray), the skin reddens far earlier, and the effect on the skin becomes far more serious with an increased dose to be followed by blisters, fluid secretion and the formation of ulcers at radiation doses of between 10 and 20 gray. At approximately 20 gray, the likelihood of these injuries healing within one and a half months is approximately 50%, and the dose may therefore be seen as the skin's tolerance dose for serious damage. At even higher doses, the death of tissue (necrosis) is sufficiently comprehensive to cause the appearance of ulcers that are very difficult to heal. Their healing process is hampered by damage to the blood vessels which leads to a poor blood supply. Cancerous tumours can arise in the tissue that has been damaged by radiation.

Before the First World War, it had been possible to obtain a fairly clear idea of the risks of the injuries that were limited to the superficial parts of the body - primarily hands and fingers. No radiation sources were yet available that could expose the whole of the body to deadly radiation doses in a short period. The x radiation that was used had a relatively low penetration capacity and intensity. The whole world's stock of radium (had it been possible to collect it in one place) would not have been sufficient to generate deadly radiation doses for someone who was no more than a few metres away within the space of one hour. However, there was a great increase in the number of serious injuries to fingers and hands. According to the American radiologist *Marshall Brucer*, the first widespread fear of radiation among the public was in 1905.

There has been a memorial stone to the 'radiology martyrs' in the garden of St. Georg's Hospital in Hamburg since 4 April 1936. The initiative for the memorial stone was taken by Professor Hans Meyer, publisher of *Strahlentherapie.* At the unveiling ceremony, the memorial stone showed the names of 169 people who, while working with x rays or radium, had been injured so severely that they had died of their injuries. All of the biographies of the deceased were printed in a book which was issued as a parallel supplement to *Strahlentherapie*.

More and more names were added as time passed by. In 1959, a second edition of the collection of biographies of the radiation martyrs was published under the name of *Ehrenbuch der Röntgenologen und Radiologen aller Nationen.* By then, the number of names had increased to 359.

The names in *Ehrenbuch* do not constitute a scientific selection. The list primarily includes people who had been able to create a name for themselves. This means that the total number ought probably to be increased by a number of nurses and assistants whose fates were not noted in quite the same way. On the other hand, when casting a critical glance, it lists a good number of names that ought not to have been included: people who died 'anaemic' up to the age of 80 are not necessarily radiation martyrs. However, the book does offer interesting if dismal reading and you can obtain some illuminating statistics from it.

The most common cause of death in *Ehrenbuch* is the severe consequential effects of x-ray damage to fingers and hands and sometimes also the head. Cancer often originated in the damaged areas and the severe injuries meant that people were forced to amputate, firstly fingers and then maybe an arm. The cancer could be stopped temporarily but it usually returned with daughter tumours in the lymph nodes under the arms. The injuries involved considerable suffering but many of those affected appear to have heroically continued with their work assignments.

Ehrenbuch makes heavy reading and we will not dwell on its dismal content for longer than necessary. But the first epoch of serious radiation damage was a tragic one, and it would not give a fair picture of the development were we not to invite you to actually comprehend the consequences that x

* Ref.: F. Ellis *Clin. Radiol.* 20 (1969), pp. 1-7; J. Kirk *et al. Clin. Radiol.* 23 (1972), pp. 344-350.

rays can have if you disregard radiation protection. However, I will reproduce just one of the many descriptions of the ill-fated destinies:

> *Sister Blandina*, with the secular name of *Maria Ridder*, born on 4 May 1871 in Anreppen, Kreis Düren (the Rhineland), had been working since 1898 as a senior x-ray nurse in the newly-established x-ray department at the privy council of the Bardenheuer Citizens' Hospital in Cologne. Unaware of the dangers associated with long-term exposure to x rays, Sister Blandina initially held patients, mainly children, still with her hands unprotected to make it possible to obtain sharp pictures with the long exposure times of that period. She tested the penetration capacity of the radiation by holding her own hand in front of the platinocyanide screen.
>
> After one and a half years of work, she was surprised to see her hands swell and the skin become red. Widespread inflammation soon occurred followed by blisters and the formation of abscesses on the backs of both hands, particularly on the left hand. The skin changes even moved up as far as her lower arms. Treatment with salves and bathing showed no results.
>
> Since the pains were insufferable and since tissue samples showed that x-ray cancer had occurred, the worst affected fingers on the left hand were amputated. Some months later, the whole of the left hand had to be amputated at the wrist along with the thumb on her right hand. Not that these operations stopped the serious disease; it was therefore necessary to remove the whole of her left arm up to the middle of the upper arm in 1909.
>
> With the help of prosthetics and with four fingers remaining on her right hand, Sister Blandina was now working in the x-ray institute's photographic department where, during the first few years of the war, she very skilfully produced photographs, copies and slides of the many war injuries intended for scientific purposes.
>
> At the end of 1915, Sister Blandina complained about severe shortness of breath. A fluoroscopy showed a comprehensive, even shadow over the whole of the left side of her chest; the stump of her left upper arm swelled rapidly and strongly. She was plagued so much by the pains that she demanded to have the stump of her arm surgically removed, which was something that the surgeon just could not bring himself to do. During that time, a large ulcer arose which covered the whole of the left of her chest and half of her back. Sister Blandina spent months sitting upright in bed with the support of cushions and finally passed away on 22 October 1916.
>
> Although Sister Blandina was severely handicapped in the use of her hands owing to the many operations, she still devoted herself to her x-ray work with unabated eagerness and unselfish devotion, and it was only her insistent requests right from the start that convinced people not to move her to a more merciful department. The author worked with her at the x-ray department for a few years, and the author's own observations bear witness to the fact that she bore all of her torments with heroic composure and remarkable patience. Her tragic fate led all doctors and nurses working at the x-ray department at that time and later on to observe particular caution when handling x rays.

Another common cause of death in 'Ehrenbuch' was *aplastic anaemia*, the disease that would kill Marie Curie under circumstances which mean there was a great likelihood that the radiation had caused the disease. In these cases, blood-forming organs had been so comprehensively destroyed that they no longer functioned normally.

Rolf Sievert said the following in a lecture in the 1960s:

> The low radiation intensity that people [i.e., at the start of the century] worked with at this time went some way towards reducing the risks of damage compared with those that would have occurred had it been possible to generate intensities as high as those used today. This and the circumstance that x rays were initially used mainly by doctors doubtless constituted the most important reasons as to why no more people were injured by radiation than the number who actually were. A study of that which was published about the pioneers within x-ray diagnostics who suffered serious radiation

> damage, often with death as a result, gives a dismal view of the insufficient radiation protection in the initial years. We all have reason to be grateful that the use of atomic energy did not become possible until a point in time when there was moderate knowledge of the risks associated with working with ionising radiation.

In his eventful book, *The Trail of the Invisible Light*, Grigg reports the result of an in-depth study of the time distribution of the first deaths as a consequence of radiation. The following list shows when the first death occurred (according to him) in different countries:

Germany	1900	Friedrich Claussen
England	1902	Barry Blacken
USA	1904	C.M. Dally (Edison's assistant)
France	1905	Jules Rhens
Italy	1912	Emilio Tiraboschi
Switzerland	1913	Henri Simon
Australia	1913	J.F. Clandinnen
Chile	1915	Eckwall (Swedish physiotherapist acc. to Grigg)
Hungary	1918	Julyus Schröder
Spain	1919	Felipe Cariazo
Finland	1920	Anna Lönnbeck
Denmark	1922	J.F. Fischer
Japan	1923	Schichiro Hida
The Soviet Union	1925	Peter Baumgarten
Czechoslovakia	1925	Rudolf Backer
Israel	1925	Josef Freud
Austria	1931	Alois Czepa
Portugal	1935	Carlos dos Santes
Yugoslavia	1939	B. Bressan
Poland	1950	Robert Bernhardt

As you can see, neither Sweden nor Norway appears on this macabre list, whether by chance or by virtue of good radiation protection. Our world-renowned radiotherapist Elis Berven may belong there; he passed away from cancer in 1966 and his fingers were hideously attacked by the radiation. Berven was one of those who tested treatment methods on his own skin. Perhaps some earlier deaths as a consequence of anaemia among Swedish x-ray nurses should be on the list instead.

All of the injuries (apart from skin cancer) that I have described here occurred because such a large number of cells were destroyed that the affected tissue could no longer function. However, a sufficiently high radiation dose is required for so many cells to be destroyed. Lower radiation doses also kill cells, but not enough. It is said that there is a radiation dose *threshold value* that must be exceeded for this to take place. It may well be that the question of whether or not a given individual cell will be destroyed is determined by chance. However, 'the law of large numbers' means that what is random at cell level is systematic for the body overall. It is as though someone were to run around in a small community randomly shooting a weapon. Chance determines who is hit. If only a few shots are fired, some individuals may die, but the community can still continue to function. If on the other hand the madman continues for a long time and fires loads of shots, the eventual level of damage will mean that the community collapses.

So, high radiation doses destroy so many cells that damage is unavoidable. Such injuries are known as *deterministic*. The seriousness of a deterministic radiation injury such as damage to the skin also increases with the radiation dose because the larger the dose of radiation, the more cells are destroyed.

The existence of a radiation dose threshold value was crucial to the choice of radiation protection strategy. For example, you could ignore all low radiation doses if there were sufficiently few for them in total not to exceed the threshold value. It could also be said that the situation was 'quite safe' if no radiation doses exceeded the threshold value. It was possible to prevent the occurrence of deterministic injuries by stating a dose limit with a good margin to the threshold value and ensuring that the dose limit was not exceeded.

There were storm clouds on the horizon, however. Hermann Muller's discovery in 1927 that x rays could generate mutations, i.e., changes to the genetic makeup, pointed to a damage mechanism that did not require a specific number of cells to be affected and therefore could not be assumed to have a threshold value. The random damage of the particular gamete that would take part in fertilisation and generate a new individual would appear to be random not just at cell level but also at individual person level. People would later start to speculate as to whether or not cancer could also be based on similar damage to body cells, i.e., *somatic* cells. If this were the case, cancer, like hereditary radiation damage, would not be an inevitable, deterministic, consequence of irradiation but would also be determined by an element of randomness. Such random injuries were eventually called *stochastic* radiation damage (from the Greek *stokastikos* = comes under conjecture).

But how, from the radiation protection point of view, would we handle injuries for which there was no threshold value for the radiation dose? At the end of the 1930s, the American NCRP, which in 1934 recommended a dose limit of 0.1 r per day, began to think about how it could also be possible to take into account the possible genetic risk. This led Failla, who would now play an even greater role in the formulation of a radiation protection policy, to write in a letter to Taylor in June 1941:

> I feel that this is a mistake for the following reasons: 0.1 roentgen per day is certainly a safe dose insofar as systemic changes are concerned. If we bring in the genetic criteria then there is no limit at all and 0.02 roentgen per day is just as arbitrary as 0.1 roentgen per day. To be sure, the smaller the dose the less the genetic damage but the possible damage from 0.1 roentgen per day is so slight that one can just as well stop at this point.

In contrast to the speculations of the radiation protectionists as to how people would protect themselves against the most tangible risks of the most macabre injuries, the mass media were speculating about 'death rays' in the 1930s. Perhaps enough time had passed since the First World War for people to stop experiencing the scare as something that was purely negative. Maybe their titillation value was once again being maintained by tension and the fear of the unknown. There may also have been a tremendous feeling of anxiety in the background that the carefree oblivion of the 20s was starting to give way to the depressions of the 30s and world crises that were lurking in the background. But why speculate? Newspaper quotes from this period speak for themselves:

> A few years ago, an English engineer by the name of Matthews created a lot of fuss. He let the world know that, in his laboratory, he was harbouring forces that could kill everything that was alive at a distance of many miles. The journalists besieged the laboratory, but the inventor did not feel inclined to lift the veil from the gorgon's head.* He was in negotiations with the British government about 'the death rays' and was neither willing nor able to say anything. However, the government said it was not interested, and a demonstration finally came about. A blazing fiasco! 'The death rays' were not even capable of taking the life of a rat a few metres away. From that day on, nothing was heard about Matthews, but plenty was heard about 'the death rays'. This had taken people's minds by storm, and imaginative storytellers and fortune tellers used the suggestion almost *ad nauseum*. We now know all about 'the death ray' - the question is, does it actually exist.

On 26 December 1933, *Nya Dagligt Allehanda* wrote the following under the heading 'Mysterious radiation power discovered by English Professor':

* 'The Gorgon's head' means the lopped off head of Medusa, one of the three Gorgons. The gaze of the Medusa head was said to turn something into stone.

> A short while ago, a sensational demonstration took place here in England of a thus far unknown mysterious radiation power. The new rays have been discovered by Professor O.A. Newell, who is the leader of the British Ministry of Healthcare's research department. He says that rays can be characterised in such a way that, during times of war, they could be a terrible weapon whereas, during times of peace, they could save the lives of many people and support the entire agricultural production. The rays are considered to be capable of creating development opportunities for bacilli, which cause deadly diseases. They also appear to be able to obliterate all life within the areas at which they are aimed. People, animals and plants will die if they are exposed to the dangerous influence of the rays.
>
> But, at the same time, they could - so their inventors maintain - neutralise all possible plagues that ravage agriculture throughout the world, and allow the revival of animals and people where possibilities of life previously did not exist.
>
> Representatives of the army and the airforce were present at Professor Newell's demonstration of the mysterious rays plus a number of scientific researchers from the various universities.
>
> The rays are 'wireless', and they have been produced using bacilla which are found in patients with various deadly diseases, says the Professor. The rays emitted from these sick people would in some cases be strong enough to cause a powerful deflection on an electric galvanometer. These radioactive emanations were conscientiously measured, their wavelengths measured, whereupon rays were reproduced and transmitted, just like with normal radio tranmissions. The first disease that was successfully induced was anthrax. The experiment then continued, with the abovementioned results.
>
> Volunteers from the army and the airforce had offered their services for the continuing experiments and allowed themselves to be subjected to various diseases that were dangerous to a greater or lesser extent in order to facilitate the confirmation of Professor Newell's findings.
>
> The military experts appear to have arrived at the result that, in practice, an advancing army must be stoppable if the means by which to transmit sufficient rays towards it can be successfully found. This is where the discovery appears to be somewhat lacking in perfection, however.

The slight irony that is discernable in the last sentence can probably be taken as confirmation that the author of the article was well aware that he was depicting a cock and bull story. Many readers no doubt also understood this but some were still scared by the dishonest journalism (dishonest in that, probably against his better judgement, the author of the article gave the impression that the article could be true). On 6 June 1934, *Nya Dagligt Allehanda* endeavoured once more to convince its readers that the death ray existed, this time with reference to a telegram from Omaha in Nebraska:

> The American government has forbidden all public demonstrations of and experiments with the 'death rays', which are said to be able to destroy all life in that they decompose the blood of living beings. Some demonstrations that were to take place at the American science congress, and which have been compiled here, have had to be cancelled for that reason. The American press and the public have followed the experiments performed thus far with the greatest of interest. At the congress, however, several scientists have given an account of some experiments with the 'death rays' at which they were present and that were carried out by a scientist, Dr. Longoria. During these experiments, he used a tube-shaped apparatus. The rays that were emitted from the apparatus would have immediately killed the laboratory animals which consisted of dogs, cats and rabbits, although the irradiation took place many hundreds of metres away. When the dead animals were examined, it was ascertained that their blood had decayed. The same effect was ascertained for flying doves, which fell to the ground dead after having been exposed to irradiation, this time also several hundreds of metres away.

We now of course know that this was a duck. In 1934, there were no technical conditions for transferring deadly radiation energies over great distances. However, at the time, the reader could not be certain that the military researchers had not made a new discovery. The impendng First World War showed that there were no death rays in the military arsenal; it was possible to create quite enough misery without them, and the discovery of nuclear fission was actually even more incredible than the mysterious death rays.

Inventive although probably inadequately-informed inventors were cogitating over the possibility of using the gamma radiation from radium as death rays for the purposes of war. Had they known better, they would have been able to work out that the total quantity of radium that had been purified in 1934 - approximately 760 grammes in all - would need to irradiate a human for one hundred hours to deliver a deadly dose at a distance of ten metres. They were presumably also unaware of the inverse square law, which says that the required time over a distance of 100 metres must be 10 000 hours, i.e., more than a year, and the radiation is also less effective over such a long period.

Not everything that was available to read about 'death rays' in the newspapers at the end of the 1930s and the start of the 1940s (before the atom bomb was dropped on Hiroshima and Nagasaki and made the truth worse than could be imagined) was madness. In *Aftonbladet* on 1 August 1940, there was a discerning article about death rays by *Carl-Bertil Holmberg.* He said that Professor *Abraham Esau* in Jena, a groundbreaking physicist as regards the production of electromagnetic radiation with a short wavelength, had killed mice and rats with his radiation. This is the radiation that we now use in microwave ovens and the impact on the animals was quite simply that they were heated up. Holmberg wrote: 'In America, where everything is done on a large scale, this method was successfully used to massacre dogs, monkeys and even a bull. The lattermentioned victim did not die until around 90 seconds later, however. As can be seen, the deadly effect of these rays is not exactly immediate; they increase the temperature of the blood and cause certain physiological impairments that could lead to a fairly quick death'. Professor Esaus' yearning for death rays would subsequently make him one of the main people involved in the German endeavours to create an atom bomb.

However, shortwave radiation was effective over a short distance only and by no means at hundreds of metres as was sometimes claimed. Holmberg continues to tell us of other alternatives to death rays, such as Professor Lenard's cathode rays that can be made to leave the discharge tube through a thin window and 'immediately kill small animals such as flies that cross their path'. But nor is the scope of cathode rays adequate to earn them the name of death rays. There is yet another type of radiation, soundwaves that have a very high frequency, i.e., *ultrasound*. Such soundwaves could destroy blood cells, but they also have an inadequate range.

The fascination about 'death rays' is by no means unique – it appears that people have always been able to find something to worry about. Where there have been no tangible events such as war and epidemics, people have found other sources of trepidation. Radiation is such a source of worry owing to its invisibility and demonstrably dangerous properties; the first real fear of radiation ('psychosis' is the popular saying, although that is the wrong word) appears to have been in 1905 – but radiation is not the only thing to have aroused corresponding fear. We have been worried about Agent Orange and mercury and, at the start of the century, arsenic. This worry may be well founded and lead to necessary clearing up, but it can also be painful and detrimental. It is not easy for the layperson to determine when it is legitimate. In the case of arsenic at the start of the century, it was legitimate to some extent: the pigment containing arsenic, Scheele's green, occurred in homes in wallpaper, curtains and articles of clothing. There was no justification for the fear of the death rays, however.

16. DANGERS OF RADIUM AND RADON

While humans were putting radium to use as a healer, it gradually became evident that the life-giving radiation could also lead to misery and death. It was actually its deadly effect on malignant tumours which caused radium and radiation treatment with x rays to give hope of life. It became clear at an early stage within radiology that the thing which prevented the treatment of tumours inside the body using radiation was the damage brought about when the radiation killed cells in the skin. The intensity of the radiation was higher there since the skin lay closer to the source of the radiation so very little of the radiation energy had been lost through absorption and dissemination in the tissues between the skin and the tumour. The radiation damage to those who worked with x rays and radium in the first few years was skin damage.

However, what soon became obvious to the radiologists was not known to the same extent among the public. The concept of radium had etched itself pretty quickly as a life-giver. Even then it was erased by 1954. When Radiumhemmet was to receive its first shipment of a strong, artificial source of radiation consisting of cobalt-60, *Rune Walstam* (later Rolf Sievert's successor as Professor of Radiophysics in Stockholm) and I travelled to Stadsgården to watch the big transport box containing the source of the radiation being unloaded from the vessel. When we arrived, the box was already on the quay and a small group of longshoremen were thronging around it. We emphasised that it might not be appropriate to sit on the box since some of the radiation could conceivably penetrate out, and we pointed to the warning labels on the box. 'That's exactly why we want to sit on the box,' answered the longshoremen. 'Radium is a life-giver!'

The hazardous properties of natural radioactivity made themselves known long before Becquerel had discovered the phenomenon. The history begins as early as the 1400s in Erzgebirge ('the Ore Mountains') on the border between the former Czechoslovakia and Germany where, in 1481, the silver mining town of Schneeberg was founded on the Saxon side owing to mineable silver finds. In 1516, rich veins of silver were also found on the Bohemian side which led to the establishment of another mining town, Jáchymov ('St. Joachimsthal, or the Valley of Saint Joachim') in what is now the Czech Republic.

As little as ten years after the foundation of Jáchymov, a young German, *Georg Bauer* (1494-1555), was employed as the town doctor. Bauer is known under the Latinised form of his name: *Georgius Agricola*. He had studied medicine in Leipzig and Italy, but his work in Jáchymov led him to show an interest in the science of rocks, and he has been referred to as the father of mineralogy and metallurgy. He is best known for his masterpiece called *De re metallica*, which was published the year after his death. In the seventh book of this work, Agricola wrote about the work conditions in the silver mines:

> [...] the dust has corrosive properties, it taxes your lungs and leaves consumption in the body [...] You find women who have wed seven men, all of whom have been taken through untimely death from this terrible consumption.

The miners themselves called the disease rock disease ('Bergkrankenheit') and thought it was caused by evil underground hobgoblins. One of Agricola's contemporaries thought otherwise, however. It was a Swiss by the name of *Theophrastus von Hohenheim* (1493-1541), better known under the name of *Paracelsus* with the significance that, in learning, he surpassed the Roman author *Celsus* who lived in the time of Emperor Tiberius and who wrote the acclaimed work *Om Läkekonsten*

[On the Art of Healing] as a series of eight books. Paracelsus was a doctor and a natural philosopher and had a substantial influence on medical thinking. He saw the human body as a chemical laboratory and diseases as the consequence of chemical disorders. He realised that the rock disease was caused by breathing in metal vapours that are deposited in the lungs in a different way. But what types of 'vapour' *were* these?

Agricola, however, believed more in dust than vapours. He found methods of ventilating the mine galleries using ingenious systems of bellows driven by horses or water power. He also encouraged the women to make fine-meshed breathing masks for their husbands.

A few hundred years would pass before the knowledge was expanded, and the silver mines in the Ore Mountains had long since petered out. The new insight came through *Martin Klaproth* (1743-1817), a pharmacist and later Professor of Chemistry in Berlin. Klaproth was interested in mineralogy, and when he analysed the black ore from Jáchymov - the ore that we now know as pitchblende (German 'Pechblende') – in 1789, he discovered the oxide of a previously unknown element. Swayed by the discovery eight years earlier of a new planet that had been given the name of Uranus, Klaproth called the new substance Uranium. The first to purify uranium was Frenchman *Eugène Peligot* in Paris in 1841. Klaproth later discovered additional elements: zirconium (1789), titanium (1792), strontium (1793) and cerium (1803).

So, Becquerel discovered that uranium was radioactive, Marie Curie discovered radium and the radioactive decay chains were researched. Each and every one of the three most important radioactive elements, uranium, radium and radon, were put to extensive use, for better or for worse.

The rock disease remained a mystery for a long time. Following Klaproth's discovery, it was found that the addition of uranium gave glass unusual colours. The Bohemian glass industry made use of this fact and brand names like 'Annagelb' (yellow) and 'Annagrün' (green) - after the name of the glass manufacturer *Josef Riedel's* wife - became well known. The mining activities at Jáchymov were therefore resumed in 1840, and, following Marie Curie's successes, pitchblende and uranium were obviously highly sought after. The popularity of radium led to in-depth studies of its properties, and a radium institute was set up in Jáchymov where a man by the name of *August Pirchan* became head - doctor in 1926.

Pirchan found that this rock disease was still claiming victims. Legal obstacles meant that he could not perform post mortems on miners to examine the nature of rock disease more closely, but when an employee of the radium institute also fell ill, Pirchan was able to diagnose lung cancer and have this confirmed by means of a post mortem. Pirchan then decided to examine the health of retired miners and found that many were very ill. In 1929, he succeeded in obtaining permission to do post mortems on 13 deceased miners, and found that 9 of them had cancer in their lungs. All that remained now was to establish the cause which, to all appearances, was the inhalation of dust containing radioactive decay products of radon. However, a few more decades would pass before it became possible to quantitatively determine the connection.

Damaging effects of uranium were observed early on. In 1824, *Christian Gmelin* (1792-1860) at the University of Tübingen described in his *Handbuch der Chemie* [Handbook of Chemistry] the results of experiments with uranium salts on dogs and rabbits. His conclusion was that uranium is weakly toxic if you consume it, but kills very rapidly if injected intravenously.

Uranium became popular among registered doctors and homeopaths at the end of the 1800s, and uranium preparations were sold at pharmacies for a long time. As late as 1930, Hager's *Handbuch der Pharmaceutischen Praxis* [Handbook of Pharmaceutical Practice] discussed a 'Vin urané' consisting of one part uranyl nitrate and 50 parts glycerol in 1 000 parts red wine. However, in the end, *Torald Sollman* requested in his *Manual of Pharmacology* in 1936 that the medical use of uranium salts be stopped.

Unfortunately, a lot of quackery was going on. The professional use was based on the obvious fact that the radiation from the radioactive substances killed cells and was thus capable of destroying tumour tissues. The less scientifically-founded use was based on more dubious effects. Where radon was concerned, there were primarily two observations. It had been noted that the miners in Jáchymov rarely suffered from rheumatism (the fact that they died of lung cancer instead was less popular a fact to be reminded of); and they also thought they noticed that the water from spas was beneficial primarily if you drank it at the source, while bottled water that was consumed long after being tapped did not appear to be equally beneficial. Both observations supported the hypothesis that it was the radon that brought the salubrious effects.

It became particularly popular to breathe in the radon. A number of manufacturers offered ‘inhalers’ by means of which people could breathe in radon that emanated from a solution of a radium salt, usually radium bromide. Natural mineral water sources were examined for their content of radium and radon and advertisements for the radon content began. Special baths were opened in Jáchymov in 1906 where the water, as you might expect, contained extra radon. Bad Gastein, with its 18 hot sources south of Salzburg in Austria, had been known since the Middle Ages but enjoyed an era of greatness through the radon. A hundred or so ‘Spa hotels’ (after the seaside resort of Spa in Belgium) offered medical baths in the basement and hot water to the rooms directly from the hot sources so that you could also breathe in the ‘beneficial’ radon that was released there. At the end of the 1930s, Hermann Göring had mine galleries built deep underground at Bad Gastein for obscure reasons - some say in the hope of finding gold while others say to store stolen art treasures. Later on, very high levels of radon were found there, which led to a commercial development of ‘Heilstollen’, a tunnel of 20 000 m^3 hot air containing a high level of radon. Special wagons were built to bring the patients into treatment stations that had beds in air at different temperatures and that had different levels of radon. In Austria, this treatment was still seen as beneficial while in other countries, the thinking had moved more towards to the risk of the radiation.

As late as 1952, an article in *Life* described the reflected health effects of radon. The article appears to have attracted thousands of rheumatics to abandoned mines to breathe in the sought-after radon. ‘Spas’ sprung up in various places in the USA and some have continued their activities, such as Hot Springs in Arkansas, the ‘Merry Widow Health Mine’ in Montana and the Sunshine Radon Health Mine in the same state. According to *Catherine Caufield*, cures are still offered against ‘arthritis, sinusitis, migraine, eczema, asthma, hay fever, psoriasis, allergies, diabetes and other afflictions’.

Water from ‘health sources’ was bottled, and details of the radon content were expressed in Mache (see Chap. 11) on many mineral water bottles way into the 1960s. In 1920, there was even a toothpaste called ‘Radiogen’, which contained radium and was said to prevent tartar by virtue of the stated radon.

Even radium (radium-226) and mesothorium (radium-228) were marketed irresponsibly. Cloths containing or treated with radium to place on the body and mud containing radium for mud baths were among the less offensive fraudulent articles. In the USA, ‘Aqua Radium’ was sold to the public until the 1930s. Worse still was a patent medicine called ‘Radithor’, manufactured in the 1920s by Bailey Radium Laboratories in New Jersey. The company was run by *William J.A. Bailey* (1884-1949) who came from a poor family in Boston and, for financial reasons, was forced to discontinue his studies that he had begun at Harvard in 1903. Bailey later claimed to have graduated from Harvard and to have a Doctorate from Vienna, but neither of these appears to have been true. At the end of the 1910s, he was accused of and sentenced for fraud. His interest in radioactive substances was aroused in 1921 when Marie Curie made a quick trip through the USA. He formed a company in New York called Associated Radium Chemists, Inc., which sold patent medicines containing radium such as ‘Dax’ (for coughing), ‘Clax’ (for influenza) and ‘Arium’ (for poor digestion). The business was quickly stopped by the authorities, however, due to the fraudulent advertising.

After having returned with a couple of new, equally shady companies in New York and succeeded in getting some favourable publicity, Bailey moved to East Orange in New Jersey where he opened Bailey Radium Laboratories and began selling Radithor. It was sold as a small bottle containing approximately one and a half decilitres of a liquid containing one microcurie (37 000 becquerel) of radium-226 and radium-228. Bailey sent advertising brochures to all registered doctors in the USA and succeeded in substantially increasing his wealth in the process. Between 1925 and 1930, he sold 400 000 bottles of the wonder drug which he promised would cure stomach afflictions, high blood pressure, impotence and more than 150 other diseases.

The use of Radithor did of course lead to injuries. One single bottle of the wonder drug contained more than twenty times the maximum annual intake of the two radium isotopes which ought to be permitted according to the first international recommendations for the protection of those working with radium. But some of its users drank several bottles a day – during the 1920s there were no recommendations for limiting the intake of radium.

The most startling case concerned *Eben M. Byers* (1880-1932). Byers was a well-known millionaire with homes not just in New York but also in Pittsburgh, Rhode Island and South Carolina, and stables for racehorses in New York and England. He was a powerful sportsman, a great shot and had become the American Amateur Golf champion in 1907. This was also the sport that indirectly made him a victim of Radithor through what happened on a chartered train on the way back from a sports event. Following a night of partying, he fell from his bed on the train and injured his arm so badly that it subsequently affected his golf. Since no doctor has succeeded in helping him, Byers was finally advised to try Radithor.

In early December 1927, Byers drank several bottles a day and immediately felt strengthened and rejuvenated. He was so pleased with the result that he sent boxes of Radithor to his friends and even let his racehorses taste the drink. Between 1927 and 1931, he drank more than 1 000 bottles. Radium gradually accumulated in his skeleton and it has been estimated that the absorbed dose in his bone tissue in 1931 alone amounted to 10 gray from α radiation.

Quackery with regard to radium was not the only cause of radium injuries in the 1920s. Tragic events also affected a large number of women who had worked with radioactive luminous paint. The report on these events had led to a demand for American authorities to intervene against the abuse of radium. Following powerful campaigns by consumer organisations in 1928, the healthcare authorities also included the matter in the recommendations on things such as wash rooms for radium workers and routine medical examinations. The Federal Trade Commission decided to review the validity of Bailey's Radithor promises. On 5 February 1930, a communication was submitted accusing Bailey of misleading advertising and of having called Radithor harmless.

At the same time, Byers began to be plagued by aches and pains and he told his doctor that the previous feeling of rejuvenation had disappeared. He started to lose weight and complained of headaches and toothache and became seriously concerned when his teeth began to loosen. Summoned experts who examined Byers more closely found similarities with the suffering that had affected the luminous paint workers. In September 1931, Byers was called as a witness as a part of the Trade Commission's investigation. Since by then he had become far too ill to travel, a lawyer was sent, *Robert H. Winn,* to his home on Long Island to hear him. Winn later described the visit (according to R.M. Macklis, 1993):

> A more gruesome experience in a more gorgeous setting would be hard to imagine. We went to Southampton where Byers had a magnificent home. There we discovered him in a condition which beggars description. Young in years and mentally alert, he could hardly speak. His head was swathed in bandages. He had undergone two successive jaw operations and his whole upper jaw had been removed. All the remaining bone tissue of his body was slowly disintegrating, and holes were actually forming in his skull.

On 19 December 1931, the Commission forbade Bailey Radium Laboratories to continue selling Radithor. Byers died at the age of 52 on the morning of 31 March 1932. The man who had previously been so powerful weighed just 42 kilos. His death led the American Food and Drug Administration to demand greater powers. Various medical societies condemned the radioactive patent medicines and these rapidly disappeared. Recommendations regarding the limit values for the intake of radium were also called for.

Bailey was not accused of Byers' death and he refused to accept that Radithor was the cause. He himself had drunk more Radithor than anyone, he said. His life went on to be varied: he wrote books on politics and health matters, he was an observer during the Second World War, he invented a method of teaching soldiers how to swim and instruments to detect submarines. He died in 1949 of bladder cancer at the age of 64.

The history of the luminous paint workers is even more tragic than the fate of Byers, but the person who created the problem was considerably more innocent and far more responsible than William Bailey. It was an Austrian by the name of *Sabin A. Sohocky* (1882-1926) who had studied medicine in Moscow before the war. At an early stage, he had the idea of making instrument pointers and scales visible in the dark by painting them with a luminous paint that was activated by the radium that was mixed in. The method was first used on German submarines during the First World War.

Sohocky then came to the USA where he participated in the establishment of the Radium Luminous Materials Company whose name was eventually changed to the US Radium Corporation. The company was situated in West Orange, New Jersey, close to Edison's laboratory where the latter had laboured with his fluoroscope and Clarence Dally had been injured through radiation. The production of watch dials began where numbers and pointers were painted with luminous paint containing radium and zinc sulphide. Sohocky called the product 'Undark' and pronounced his visions of the future: 'There will no doubt come a time when you will have rooms in your homes that are illuminated only with radium. [...] The light that comes from the radium paint on the walls and ceiling would be like soft moonlight in colour and lustre'.

Sohocky's prophecies were not unique. The radioactive luminous paint had also challenged the imagination of many authors of adventure stories. In 1920, one of the better known authors, *Edgar Rice Burroughs* (1875-1950), the man who created Tarzan, also produced a series of novels about John Carter in around 1920, commander on Mars where he often returned to radium as a source of light:

> The passageway was dimly lighted by occasional radium bulbs, the universal lighting medium of Barsoom. These same lamps may have been doing continuous duty in these subterranean chambers for ages, since they require no attention and are so compounded that they give off but the minutest of their substance in the generation of years of luminosity.

Sohocky was the first technical director and later became head of the factory which, as well as making watch dials, also produced self-luminous crucifixes and knobs for light pull cords. When the USA entered the war in 1917, the business was expanded to include aircraft instruments and weapon sights. Up till 250 workers sat at long rows of work benches. They were mainly women and young girls down to the age of twelve and they had no concept of any danger. On the contrary, they amused themselves in between by painting the nails on their hands and feet with the luminous paint so that they could make an impression in the dark.

It was not the work that was done as intended which carried the great risk, but the special method developed by the workers themselves. One of the physicists who later engaged in investigating the conditions, *Robley Evans*, has described this (see Caufield, 1990):

> In painting the numerals on a fine watch, for example, an effort to duplicate the shaded script numeral of a professional penman was made. The 2, 3, 6 and 8 were hardest to make correctly, for the fine lines which contrast with the heavy strokes in these numerals were usually too broad, even with the use of the finest, clipped brushes. To rectify these too broad parts, the brush was cleaned and then drawn along the line like an eraser to remove the excess paint. For wiping and tipping the brush the workers found that either a cloth or their fingers were too harsh, but by wiping the brush clean between their lips the proper erasing point could be obtained.

Each time they put the brush between their lips, they ingested an insignificant quantity of radium, but each worker painted hundreds of watch dials or instrument panels each day and might put the brush into their mouths thousands of times a day, day after day, year after year. There were a good fifty similar luminous paint industries with more than 2 000 workers in all in the USA alone. In 1920, 4 million watch dials were produced there using luminous paint containing radium, but even fishing bait, dolls' eyes, weapon sights and 'locator buttons' (luminous dots) of radium paint that could be affixed to anything you wanted to find in the dark, from lamp switches to door handles.

The result of this 'radium-licking' was popularised in Catherine Caufield's *Multiple Exposures* which I have used as a source for my story about the luminous paint tragedy. In the first few years of the 1920s, a number of luminous paint workers died surprisingly early but for no apparent common reason. The death certificates mentioned causes of death as being anything from stomach ulcers, syphilis, phosphorous poisoning, anaemia and necrosis of the jaw. One step towards an explanation was taken when in autumn 1923, a well-known New York doctor and dentist, *Theodore Blum*, examined a luminous paint worker who had an infected jaw bone. When Blum heard what she worked with, he thought that the luminous paint could perhaps be the reason for her difficulty. When Blum later published an article on oral surgery in the *Journal of the American Dental Association* in September 1924, he therefore included a footnote which mentioned the case of the luminous paint worker and that her difficulty might be 'caused by a radioactive substance used in the production of self-luminous watch dials'.

This single footnote began to unravel the causal connection. It was read by a doctor, *Harrison Martland*, district doctor for Essex County, which is where the US Radium Corporation was located. That footnote would lead Martland to devote twenty years of his life to carefully investigating what had affected the luminous paint workers. His first decision was to attempt to do a post mortem on the next luminous paint worker who died and to find out what really had happened.

The many cases of death among the workers did not go unnoticed. Both US Radium and the local healthcare committees decided to investigate the matter. US Radium did so privately in March 1924 by asking some researchers at the Harvard School of Public Health to examine the work conditions. Although the company was assured that the radium was not at all harmful, the supervisors were simultaneously instructed to ensure that the workers did not lick their brushes. The healthcare committee asked New Jersey's consumers' association to look at the work situation. The group that visited the factory found that the work conditions were good and New Jersey's labour inspectorate, which also checked the situation, agreed. However, that was not the case with the Harvard researchers, who found that the workers were heavily contaminated with luminous paint and that the gamma radiation level in the factory shrouded packets of sealed dental film within two or three days. None of the blood samples taken from the 22 employees was normal.

The Harvard researchers reported these finds to US Radium in June 1924, and they wrote that 'it looks as though we have to reach the inevitable conclusion that the injuries described were caused by radium'. US Radium rejected the report and forbade its publication under threat of legal action.

Although there was nothing to note as regards the work conditions, the consumers' association still suspected that something was wrong and contacted the national consumers' organisation for advice. This meant that *Dr. Frederick Hoffman*, statistician for the Prudential Life Insurance Company, was asked to do his own investigation. Hoffman then reported the result thereof at the American Medical Association's (AMA) annual meeting in May 1925. There were far too many deaths and cases of disease, said Hoffman, for it to be a result of chance: 'We're dealing with a completely new work injury here, probably a result of radium poisoning.'

The Harvard researchers had urged US Radium to also present its report at the AMA's annual meeting, justifying this by saying that it would depict the company in a better light it could show that it was taking the problem seriously. US Radium refused, however, and continued to state outright that the quantities of radium were far too small to be able to create any damage. Dr. Martland received the same decision when he wrote to US Radium.

In spring 1925, Martland had the opportunity to examine two unwell luminous paint workers who died soon after. He then obtained permission to do post mortems on the bodies and now had the help of Sohocky who, in protest against what had happened, left US Radium, the very company he had been involved with starting up himself. Martland and Sohocky found that the bodies of the dead women, particularly the skeletons, contained large quantities of radium. Martland gives a detailed account of his observations in an article in the *American Journal of Cancer* in 1931. Examinations of 18 dead workers showed that they had between 10 and 180 microgrammes of radium in their skeletons.

Despite US Radium's endeavours to keep the problem secret and make light of it, the events began to arouse publicity. On 10 March 1925, the *New York Times* carried an item about the first luminous paint worker who was claiming compensation from US Radium. In the same spring, US Radium's chemist *Edwin Lehman* died of aplastic anaemia, the same disease that would kill Marie Curie. The newspapers wrote more and more about the fates of the unlucky luminous paint workers and more and more claims were made against US Radium for compensation. Since the Harvard researchers had not given the company what it had hoped for, it turned in desperation to a new consultant, *Dr. Frederick Flinn* at Columbia University, a specialist in occupational hygiene. In December 1926, Flinn reported that 'there was no occupational risk when painting luminescent watch dials'. It took until 1928 for him to admit that he had got it wrong.

The women who claimed compensation had to wrestle with legal problems. US Radium wanted to maintain that they had no proof of the cause and effect. Their lawyers took another line, however, pointing out a regulation which said that compensation for people with occupational injuries could be paid out only if a claim had been made within two years of the time when the disease presented. This stirred up protests among the public and the media: how could a claim for compensation be made within such a timeframe when it took several years for the injuries to be detected? The court negotiations took several years in some cases. The case that had the most publicity was the one the press called Five Women Doomed to Die, where five former US Radium employees claimed compensation together. All five had horrific injuries. One had undergone 20 jaw operations and had her legs paralysed through injuries to her spine. They were so injured that they had to be carried into the courtroom during the court negotiations, and one of them was unable to even lift her hand to swear her oath. Following long, drawn-out proceedings, US Radium went along with an amicable settlement 'for humanitarian reasons' but denied any legal liability.

Martland took care of Sohocky in his final year. By then, the man was so full of radium that the air he exhaled caused a zinc sulphide screen to fluoresce. His jaw and hands were destroyed by the radiation, but the immediate cause of death was aplastic anaemia.

During their studies, Martland and his colleagues found that the radium that entered the body was never directly excreted as had been thought. Instead, some was taken up in a similar way to calcium, i.e., in the skeleton where it could generate bone tumours and harm the production of blood by irradiating bone marrow. This was knowledge that would be useful when the handling of artificial radioactive substance would begin a decade later.

Martland found that it was not just luminous paint workers who were affected by injuries from radium. Radium had deliberately been given to thousands of patients in the hope of curing them from different diseases, from rheumatism to schizophrenia. Particularly worthy of note are the attempts to treat schizophrenia with hundreds of microgrammes of radium-226 which were carried out at Elgin State Hospital in Illinois on a good thirty patients in 1931: 'Previous examinations of the speed of excretion, excretion paths and the completeness of the excretion of radium from the body convinced us that our dosing was not dangerous. We were therefore sure that we would not harm the health of the patients.' Those were the words of ignorance and lack of knowledge.

In the USA, some of these treatments were sanctioned by the AMA until 1932. One interesting conclusion was drawn from all this radium treatment: it seemed as though the first symptom of radium poisoning, as in the Byers case, was an increase in well-being which, unfortunately, was soon succeeded by incurable injuries.

The knowledge amassed until the 1930s concerning the danger of radium was also used in less serious contexts, i.e., in adventure stories. The best known from the 1920s was written by the British man, *Cyril McNeile* (1888-1937), under the pseudonym of Sapper with a hero called Bulldog Drummond. Rolf Sievert (and many alongside him) read the Sapper books with delight. A corresponding Swedish author was the baron *Axel Klinckowström* (1867-1936), known as 'Klinckan' and who, as well as adventure stories, wrote travelogues, poetry and pretentious Old Norse and had an education in natural science; he had been a lecturer in zoology at the University of Stockholm. In 1926, Klinckowström wrote the novel called *Guldsaxen*, an adventure story in which the hero, Consul Jonas Viborg (a pretty insufferable fellow), has to tussle with the fiendish queen of female criminals Irma Brand (Sapper's equivalent baddie was also called Irma). Like Bulldog Drummond, Consul Viborg is supported by a crowd of semi-fascist 'lively youths' who, in Klinckan's version, are dressed in marksmen's uniform.

Now, suspecting nothing untoward, Consul Viborg is the host of a respectable dinner when all of a sudden one of the guests, Baron Fabian Klewenhüller, gets up and whacks him over the head so that he falls down into his chair unconscious. Consul Viborg's son who, as luck would have it, happens to be studying to become a dentist and is perfectly legally carrying pliers in his pocket, is afraid, but the Baron roars:

'Second premolar in the upper jaw - the one with the new filling - for heaven's sake, boy, don't stand there gawping – it's a question of your father's life!'

A fuse had gone and darkness had fallen in the room. The quick-thinking Baron had then noticed that the Consul's precious stone-adorned toothpick had given off a 'fleeting greenish phosphorescent shimmer' each time he put it near his mouth. The Baron of course immediately understood that this was because the satanical Irma Brand had bribed the Consul's dentist by filling one of his teeth with a radium salt.

> Courageous though he was, the Consul felt cold sweat break out by his temples.
>
> '*Radium?*'
>
> The baron nodded:
>
> 'Yes, exactly – radium - nothing more, nothing less, and in no small quantity either. Unless I'm very much mistaken, this tooth underneath the gold filling contains at least ten to fifty thousand *kronors*-worth of top class radium salt – enough energy, *were* it to be released, to run your Packard lap after lap around the globe or blast this building into the air.
>
> 'Year in, year out, century after century, this radium filling will transmit its living power in the form of rays - rays that cause diamonds to shine in the dark like small suns and *the human body's living cells to waste away and die....*'
>
> 'Tell us, Fabian, what would the result have been had that hellish tooth remained in Jonas' mouth?'
>
> 'Today, nothing, tomorrow and the day after, still nothing - within a week maybe slight jittery toothache; within fourteen days of all the suffering, necrosis in the jaw bone and tongue – and within a couple of months, *death*, since half of his face would have rotted away while he was alive.'
>
> [...]
>
> Consul Viborg mopped the cold sweat from his brow using the bloody serviette and stared at the tooth lying in the middle of the table as if it were the head of Medusa.
>
> 'But - but what will happen to me? I've already been carrying this damned piece of matter around for two to three hours at least!'
>
> 'And you will doubtless pay for that in due course, Jonas – calm down. Henri Becquerel was carrying a radium tube in his left pocket for around the same length of time and the result after fourteen days was a nasty burn on his stomach – that's how the physiological effect of radium was discovered.'
>
> 'Nice discoveries – people like that should be hanged!'*

And that is where we take our leave of the irate Consul Viborg, that is after first noting that Irma eventually died when her submarine foundered and that Consul Viborg, 'the plucky sportsman,

* The technical details invite a few calculations. Radium probably cost Irma approximately SEK 500 per milligramme. If, to go by Baron Klewenhüller's conjecture, the filling contained radium to a value of ten to fifteen thousand *kronor*, there ought to have been 20-30 milligrammes. The Consul had carried the radium for approximately 3 hours. Thanks to the the resourceful tooth extraction, the radiation dose ought to have been just enough for temporary damage to the gum and the tongue next to the tooth while a few days of irradiation would have led to severe injuries. The quantity of radium was thus well chosen by both Ms. Brand and the author. On the other hand, the information on the energy contained in the radium was incorrect. One gramme of radium emits 0.1 watt of power; the radioactive filling ought therefore to have emitted 2-3 milliwatts. 1 milliwatt is approximately 1/735 000 horsepower, so the power from the filling ought to have been no more than 4 millionths horsepower. The average life of radium is approximately 2 300 years. The energy would thus have been sufficient to drive a car with 4 millionths horsepower for 2 300 years (what a bizarre thought!) and a car with, say, 50 horsepower for 4/50 millionths of this time, which is not much more than one and a half hours.

probably endured long-lasting suffering from the burn injuries he obtained during that short period, his tongue and gums having been exposed to the deadly radiation from the radium filling'.

We are also able to state that Baron Klinckowström must have had good advisors before he described the possible consequences of the irradiation from the radium; they are fairly correctly described. He may have had direct advice from 'the father of Swedish radiology', Professor Gösta Forssell, since in the book, Baron Klewenhüller mentions Forssell as one of the three Swedish people whose scientific skills are comparable to those of Irma Brand (the other two, the Baron realised, were Svante Arrhenius and 'The' Svedberg).

17. THE NEW THEORETICAL PHYSICS

The next chapter depicts the events that led to knowledge of the structure and properties of the atom - properties that are crucial to its capacity to emit and absorb radiant energy. But let's start by meeting a few people and hearing about a few events that will act as a reference framework for the following through the structure of the theoretical mathematical tools that were required to create models to describe physical observations that were difficult to explain. We already know of the three giants within physics of the early 1900s: Rutherford, Einstein and Bohr. It is interesting to know how they placed in a chronological context:

Rutherford:	1871 - 1937
Einstein:	1879 - 1955
Bohr:	1885 - 1962

For comparison purposes, we see the corresponding list for some of the physicists who crop up in this chapter. Everyone except for Wien would survive the Second World War.

Planck	1858 - 1947
Lenard	1862 - 1947
Wien	1864 - 1928
Sommerfeld	1868 - 1951
Stark	1874 - 1957
Meitner	1878 - 1968
von Laue:	1879 - 1960
Born	1882 - 1970
Schrödinger	1887 - 1961
de Broglie	1892 - 1987
Szilard	1898 - 1964
Pauli	1900 - 1958
Fermi	1901 - 1954
Heisenberg	1901 - 1976
Wigner	1902 - 1995
Frisch	1904 - 1979
Segré	1905 - 1989
Oppenheimer	1905 - 1967
Teller	1908 - 2003
von Weizsäcker	1912 – 2007

The story of these people concerns very theoretical research that requires considerable mathematical knowledge to understand. It goes beyond the framework of this book and beyond the limits of my own ability to 'explain' this research. What I can do is provide an idea of its direction and give an indication of the methods that were used.

In 1913, the 28-year-old Niels Bohr had completed his essay on the structure of the atom in which he proposed that the electrons around the atomic nucleus could attract only discrete levels of energy.

Bohr had returned to Copenhagen from Manchester but was communicating with Rutherford through letters, and had sent him his essay to obtain points of view and help with publication. Rutherford wrote that the essay was better than previous versions but far too long; could Bohr delegate Rutherford to shorten it? Bohr became wary, travelled to Manchester and, through stubborn arguments, succeeded in grinding Rutherford into accepting the full length of the essay. It was then published in *Philosophical Magazine.*

Bohr's essay received a mixed response. Old Lord Rayleigh found it of little interest, and Max von Laue found Bohr's theory so unreasonable that he said he would leave physics if Bohr were right. *Arnold Sommerfeld* (1868-1951) in Munich was impressed, however, and thought that Bohr's essay constituted a milestone within theoretical physics. However, Bohr's theory was based on intuition rather than any theoretical foundations. The energy jumps between the electron orbits in Bohr's atomic model were quantified, but there was still no quantum physics.

In the following year, 1914, Bohr travelled to Germany to speak to physicists in Göttingen and Munich and to go walking with his brother. The war cut short the trip and Bohr returned to Denmark and then to Manchester to assist Rutherford. He returned to Copenhagen in 1916, now realising that physics had become so complicated that it required international cooperation. He dreamed of having his own research institute, open to researchers from all countries. In 1920, that dream became reality when Bohr became director of a newly-established institute for theoretical physics at the University of Copenhagen, an institute that would play a major role in cooperation in international physics. In 1922, he was awarded the Nobel Prize in Physics for his atomic model and thereby became known throughout the world.

The war had prevented many researchers from engaging in the new physics, and the questions raised by Bohr's atomic model had not yet been straightened out. Just before the war in 1913, Einstein had become director of the Kaiser-Wilhelm Institute for Physics, but his success irritated other German physicists, particularly Philipp Lenard. Young *Werner Heisenberg* (1901-1976), who studied physics with Sommerfeld in Munich together with *Wolfgang Pauli* (1900-1958) and who would defend his thesis there in 1923, came to Leipzig with Sommerfeld in 1922 to listen to a lecture by the famous Einstein. However, they found that the lecture had been cancelled owing to demonstrations against Einstein at which students were giving out leaflets attacking Einstein's 'Jewish physics'. Heisenberg has described the situation when he was about to enter the lecture theatre (Powers, 1993):

> …a young man thrust into my hand a red leaflet, reading more or less to the effect that the theory of relativity was a totally unproved Jewish speculation, and that it had been undeservedly played up only through the puffery of Jewish newspapers on behalf of Einstein, a fellow-member of their race. I thought at first that this was the work of one of those lunatics, who do, of course, occasionally frequent such meetings. But when I found that the red leaflet was being distributed by one of the most respected of German experimental physicists (Philipp Lenard), obviously with his approval, one of my dearest hopes disintegrated. So science, too, could be poisoned by political passions.

That same year, Heisenberg got to accompany Sommerfeld to Göttingen to listen to a lecture by Niels Bohr. During the subsequent discussion, Heisenberg criticised one of Bohr's statements, which Bohr welcomed to the extent that he invited the young physicist to take a walk in the hills so he could get to know a bit more about him.

In 1923, Heisenberg defended his thesis at Sommerfeld in Munich and then moved to Göttingen as a lecturer for *Max Born* (1882-1970), who was Professor of theoretical physics there. This led to a cooperation that would yield a Nobel Prize for them both.

In the 1920s, Göttingen was, probably along with Bohr's institute in Copenhagen, the most important research centre in the world for theoretical and mathematical physics. The famous mathematicians *David Hilbert* (1862-1943) and *Richard Courant* (1888-1972) were there, plus at times many of the young European physicists such as Wolfgang Pauli, *Eugene Wigner* (1902-1995), *Robert Oppenheimer* (1905-1967) and *Enrico Fermi* (1901-1954).

In 1924, Heisenberg was invited to visit Bohr in Copenhagen. Bohr had already taken a liking to the young German in Göttingen and now afforded him preferential treatment at his institution, including taking long walks with him and instigating lively scientific discussions with him. While this formed the basis for a friendship of many years in which Bohr, who was sixteen years his senior, became almost a father figure, it also sowed the seed for alienation when discussing the situation in Germany when the First World War broke out.

Bohr viewed German nationalism with animosity, distrusting it. When it came to Denmark in the next world war, he in turn would be a nationalist but, while he saw this as patriotism, he viewed German nationalism as German aggression. He did not comprehend the 'war fever' which he realised had affected all Germans at the start of the war - he found it irrational and barbaric. Heisenberg tried to defend himself: he had been just twelve years old when the war broke out; he loved his country. Those like his cousin (who fell at the start of the war) who had gone out into the fields had done so with a sense of common destiny. What choice had they had? But Bohr was unable to understand - he viewed the action as primitive. 'What you are telling me makes me very sad,' he said.

In 1924, French physicist *Duke Louis de Broglie* (1892-1987) submitted his thesis on matter waves. Einstein had previously pointed out that light waves could also be perceived as wave packets with particle properties, or photons. De Broglie theorised that the opposite could also apply so that particles could sometimes be described using a wave analogy. The wavelength (λ) it concerned was Planck's constant (h) divided by the *momentum* or *impulse* of the particle (the momentum of a particle is the product of its mass and velocity):

$$\lambda = h / (m \cdot v) \qquad (17.1)$$

Bohr and Heisenberg had had in-depth discussions about precisely the fact that the electron orbits of the atom could correspond only to some determined energy states so that the energy levels were *quantised.* Could a good physical model be found to describe this? The theory came from Heisenberg's hay fever. It forced him to look for pollen-free air, so in May 1925 he went to Helgoland for the sea air. The change of environment led him to see the problems from new angles and he suddenly saw a way of processing them, a method using mathematical consistency and logic. He felt exhilarated yet uneasy, and subsequently wrote (according to Richard Rhodes):

> At first, I was deeply alarmed. I had the feeling that, through the surface of atomic phenomena, I was looking at a strangely beautiful interior, and felt almost giddy at the thought that I now had to probe this wealth of mathematical structures nature had so generously spread out before me. I was far too excited to sleep, and so, as a new day dawned, I made for the southern tip of the island, where I had been longing to climb a rock jutting out into the sea. I now did so without too much trouble and waited for the sun to rise.

What Heisenberg had realised was that the numbers that could describe the state of the atom by measuring observable quantities such as the frequency, intensity and polarisation of the radiation emitted could be processed using *matrix calculus*. A matrix is an arrangement of quantities in a given order. A simple example is the results (win or loss) when, for example, four people play against one another in a tennis match:

	Elsa	**Maria**	**Karin**	**Eva**
Elsa	0	win	loss	win
Maria	loss	0	win	loss
Karin	win	loss	0	loss
Eva	loss	win	win	0

This can generally be written in the form of a matrix where for the element a_{ij}, 'i' designates the i^{th} row and 'j' the j^{th} column. In the table corresponding to the above tennis results, a_{32} = 'loss' since Karin lost to Maria. The tennis example gives the following matrix:

$$\begin{matrix} a_{11} & a_{12} & a_{13} & a_{14} \\ a_{21} & a_{22} & a_{23} & a_{24} \\ a_{31} & a_{32} & a_{33} & a_{34} \\ a_{41} & a_{42} & a_{43} & a_{44} \end{matrix} \qquad (17.2)$$

In the simple tennis example, $a_{11} = a_{22} = a_{33} = a_{44} =$ '0' since no-one plays against herself. If all elements that are symmetrically located in relation to the diagonal (i.e., the zeros in the above example) are equal, e.g., $a_{13} = a_{31}$, the matrix is called *symmetrical*. If the symmetrical elements are different yet have some affinity, e.g., are one another's opposites, they are called *conjugated*.

In my tennis example, the symmetrical elements are conjugated since a win for one player automatically means a loss for the other. The one element is then usually designated with an asterisk so that, for example, $a_{31} = a^*_{13}$ One special case is that the numbers are *conjugated complex numbers*, so that for example:

$$\begin{aligned} a_{13} &= a - i\,b \\ a_{31} &= a + i\,b = a^*_{13} \end{aligned} \qquad (17.3)$$

where 'i' is the symbol for the square root of minus one, i.e., a number that multiplied by itself gives minus one.

Matrices containing elements that are conjugated complex numbers are called *Hermitian matrices* after the French mathematician *Charles Hermite* (1822-1901). Heisenberg compiled lists of the different energies ($E^{(1)}$, $E^{(2)}$, $E^{(3)}$ ... etc.) that an oscillator, like the electrons in Bohr's atomic model, was limited to possessing according to the quantisation hypothesis, and the corresponding fundamental frequencies ('the fundamental tones') $\nu^{(1)}$, $\nu^{(2)}$, $\nu^{(3)}$ ... etc. for the oscillation. If the oscillation is not harmonic, i.e., if it includes more frequencies than the fundamental tone, it can be considered to be composed of harmonic oscillations with the 'overtones' of 2ν, 3ν, 4ν ... etc.

Each harmonic oscillation has an *amplitude* (deviation from the mean value) which can be described using a sine function so that the amplitude is proportional to the sine $(2\pi\nu t)$ where t designates the time. Every engineer knows that the sine of an angle x can also be written using exponential terms:

$$\sin x = \tfrac{1}{2}\,(e^{ix} - e^{-ix}) \qquad (17.4)$$

If 'k' designates the n^{th} overtone and q_k a corresponding coefficient, the total amplitude of the oscillation can be written as

$$\begin{aligned} q(t) = {} & q_o + q_1\, e^{2\pi i\nu t} + q_2\, e^{2\pi i(2\nu)t} + q_3\, e^{2\pi i(3\nu)t} + \ldots + q_k\, e^{2\pi i(k\nu)t} + \ldots \\ & + q_{-1} e^{-2\pi i\nu t} + q_{-2} e^{-2\pi i(2\nu)t} + q_{-3} e^{-2\pi i(3\nu)t} + \ldots + q_{-k}\, e^{-2\pi i(k\nu)t} + \ldots \end{aligned} \qquad (17.5)$$

This expression includes terms with the coefficients q_k and q_{-k} in pairs. These terms are complex conjugates, i.e., $q_k = q^*_{-k}$ or $q^*_k = q_{-k}$. The constant term q_o gives the distance of the oscillating particle from a given zero.

Since this expression can be set up for each and every one of the fundamental frequencies $\nu^{(n)}$ corresponding to the permitted energies E_n (n = 1, 2, 3 ...), all terms can be set up so that they jointly form a matrix which permits further calculations.

When Heisenberg returned to Göttingen and to Max Born, Born recognised Heisenberg's mathematical formulation. Heisenberg had used the rules for calculations using matrices. He had formalised *quantum mechanics*.

Göttingen was the right place for the application of matrix calculus. Born's mathematics teacher, David Hilbert, was still Professor there and had developed methods for matrix -calculations.* The young Wolfgang Pauli was also in Göttingen at this time and, at the age of 21, had written an impressive monograph of the theory of relativity and was recognised by everyone, himself included, as being a genius. Pauli would become Professor in Hamburg in 1926 and in Zürich in 1928. He had a habit of saying exactly what he thought without the need for any courtesy, but had encouraged Heisenberg who was one year younger. While the latter was visiting the Cavendish Laboratory, Max Born asked whether or not Pauli would like to help to improve Heisenberg's mathematics. Pauli declined, stating that this would risk ruining Heisenberg's ideas with 'boring and complicated formalism'.

Born considered the formalisation to be important, however, and he worked with Heisenberg and another young physicist, *Pascual Jordan* (1902-1980) for a few months to develop *matrix mechanics*, which was the first example of quantum mechanics; the results were published in November 1925.

Heisenberg was nervous and uncertain of himself, but Pauli allowed himself to be impressed and applied the method to the hydrogen atom and succeeded in deriving the Balmer formula and the Rydberg constant without needing to repeat Bohr's earlier, rather gratuitous assumptions. In 1925, Pauli also established what is usually called the *Pauli exclusion principle*. He had drawn the conclusion that it was 'forbidden' for all the quantum numbers of two electrons to have the same values (see the end of Chapter 9). Pauli's principle became extremely important to the interpretation of the periodical system of the elements since it determines the possible number of electrons at each energy level. If the energy levels are 'full', this reduces the possibility of chemical bondings (as for the noble gases).

Bohr was obviously delighted with Heisenberg's progress and wrote the following to Rutherford:

> Heisenberg is a young German of gifts and achievement. In fact because of his last work prospects have at one stroke been realized which, although only vaguely grasped, have for a long time been the center of our wishes. We now see the possibility of developing a quantitative theory of atomic structure.

However, in 1926, a competing theory entered the scene, introduced by a slightly older physicist, *Erwin Schrödinger* (1887-1961), just two years younger than Bohr. Schrödinger was then Professor of Physics at the technical university in Zürich. He had read about the Broglie theory of matter waves in an essay by Einstein. If this meant that electrons also had wave properties, the condition for a standing wave around the atomic nucleus at a distance r from the centre would be that the 'orbit' $2\pi r$ must be a whole number (n) of wavelengths, i.e.

$$2\pi r = n\lambda = nh/(mv) \qquad (17.6)$$

This expression is identical to Bohr's condition for the electron orbits (9.12). Matter waves with other wavelengths would be obliterated through interference. The mysterious quantisation of the energy levels could thus be explained using the wave concept of classical physics.

Schrödinger added to his idea and examined the way in which the *wave equation* could be applied. The wave equation is a partial differential equation, which means that it includes *partial derivatives*, i.e., derivatives in respect of one of the variables of the function while the others are seen as constants. The wave equation generally describes the propagation of waves. While common derivatives, e.g., of the function F(x), are designated as dF/dx, the partial derivatives are written in respect of x, y and z of a function F(x,y,z) as $\partial F/\partial x$, $\partial F/\partial y$ and $\partial F/\partial z$. Correspondingly, the second derivatives are written as $\partial^2 F/\partial x^2$, etc.

Schrödinger introduced a function $\Psi(x,y,z,t)$, which he called the *wave function*, and which can be seen as a compilation of a spatial function $\psi(x,y,z)$ and a time function f(t). The latter can, as is customary for periodical functions (see the discussion on matrix mechanics), be stated as a function of a complex variable. For the spatial function, Schrödinger established conditions that are described by the famous *Schrödinger equation:*

$$\partial^2\psi/\partial x^2 + \partial^2\psi/\partial y^2 + \partial^2\psi/\partial z^2 + \frac{8\pi^2 m}{h^2}(E - U)_\psi = 0 \qquad (17.7)$$

In that expression, m is the mass of the electron, E its total energy and U its potential energy under the influence of coulomb forces from the atomic nucleus. You usually introduce an *operator* designated as Δ which, if applied to a function, means the sum of the partial second derivatives in respect of x, y and x. It is easier to write the Schrödinger equation using this operator:

$$\Delta\psi + \frac{8\pi^2 m}{h^2}(E - U)_\psi = 0 \qquad (17.8)$$

The Schrödinger equation can be solved only for specific values of E, which are called the *eigenvalues*. These eigenvalues turn out to be identical to the energy levels calculated by Bohr for his atomic model (equation 9.13). The problem's *eigenfunctions* are the equivalent of the eigenvalues, consisting of the functions ψ(x,y,z) which constitute solutions to the Schrödinger equation. These functions thus have the given values in space. The way in which these values are geometrically distributed can best be illustrated by dividing the wave function into two components, one f(r) which indicates the way in which the values vary with the distance r from a centre, and one g(φ,ϑ), which states the direction dependency of the values as a function of the angles φ and ϑ.

The function f(r) turns out to be determined by two parameters that prove to be identical to the *principal quantum number* n and the *second quantum number* l, just as these are included in the description of the atomic model that I have given at the end of Chapter 9. So, the principal quantum number states the different energy levels as 'shells' at different distances from the atomic nucleus and the second quantum number can be interpreted as a measure of the electron's element of *angular momentum* (= *rotational momentum*). The direction dependency section of the wave function does not include the principal quantum number n as a parameter, but the second quantum number and the *magnetic quantum number* m, which is mentioned in Chapter 9.

The quantisation of the energy levels (E_n) which Bohr intuitively introduced to explain the properties of the atom and which constitute a basis for quantum mechanics thus does not constitute a basis for, but rather a result of, Schrödinger's *wave mechanical* model and the mathematical formulation thereof. This made Schrödinger's theory unbelievably acceptable to physicists who had difficulty 'understanding' quantum mechanics and Bohr's hypothesis. Not even Max Planck had seen a physical 'content' in his constant h, but said he had introduced it simply to get the radiation formulae to tally with that which had been observed in experiments. Heisenberg saw Schrödinger's wave mechanics as an attack on his pet quantum mechanics. At a lecture by Schrödinger in Munich in 1926, the latter was criticised by Heisenberg, who thought that Schrödinger was demolishing that which had been built up with great cerebration, and that Schrödinger's wave mechanics could not explain Planck's radiation law. Swedish Professor of Physics *Gudmund Borelius* (1889-1985), my old teacher at KTH, was present at Schrödinger's lecture during a visit to Sommerfeld who had told Borelius about the promising young Heisenberg. Sommerfeld had the impression that Heisenberg and Schrödinger were talking about the same thing but using different mathematical tools. Heisenberg denied this and an hour-long discussion ensued. After that, remembers Borelius, our going out for a beer led to the birth of quantum mechanics.

Heisenberg did not get much support, not even from Sommerfeld who was impressed by Schrödinger's mathematics. Other physicists had a dig at Bohr and Heisenberg for mysticism and thought it was time to leave the heresies that had grown up through Planck's radiation-quanta hypothesis. One of these was the respected classical physicist Wilhelm Wien, who was Professor of Physics at Munich. Only Bohr defended Heisenberg. He did so by inviting Schrödinger to Copenhagen in September 1926. Heisenberg, who was also present, has described the heated debate that started as soon as Bohr met Schrödinger at Hovedbanegaarden and which continued more or less all day (according to Rhodes):

> For though Bohr was an unusually considerate and obliging person, he was able in such a discussion, which concerned epistemological problems which he considered to be of vital importance, to insist fanatically and with almost terrifying relentlessness on complete clarity in all arguments. He would not give up, even after hours of

> struggling, [until] Schrödinger had admitted that [his] interpretation was insufficient and could not even explain Planck's law. Every attempt from Schrödinger´s side to get round this bitter result was slowly refuted point by point in infinitely laborious discussions.

Not even when Schrödinger came down with a cold and had to retire to bed did he escape Bohr since he unfortunately was staying in Bohr's home. Mrs. Bohr took care of him while Bohr sat on the edge of the bed and repeated: 'Schrödinger, you must understand...' or 'But, Schrödinger, you must still admit that...' In the end, Schrödinger left Copenhagen, tired and downhearted, when Bohr's parting words to him were: 'But we are all so grateful for what you have done because you have thus brought physics a decisive step forward.'

However, the contrast between wave mechanics and quantum mechanics created problems for Bohr and Heisenberg, who spent a great deal of time in Copenhagen. Schrödinger saw the wave theory as a description of reality and Heisenberg saw the mathematical formulation of quantum mechanics as an abstract model. Bohr was pondering how two such different concepts as particles and waves could be united.

A hint of a solution came through Max Born's interpretation of Schrödinger's wave function ψ. This has no directly physical content, but the square thereof - or rather the product of ψ and its complex conjugate function ψ^* - can, following suitable standardisation, be interpreted as the probability per volume unit (*the probability density*) of an electron being in a given place. With this probability interpretation of the wave function, for which Born later won the Nobel Prize, Heisenberg's quantum mechanics and Schrödinger's wave mechanics could be seen as different mathematical methods of describing the same phenomenon. And, what is more, Schrödinger actually succeeded in deriving Planck's radiation law.

In February 1927, the idea was born that is usually linked with Heisenberg's name. He says that it developed on a walk he took in the park after midnight past the football fields behind Bohr's institute. He then had the idea which was later formulated into *Heisenberg's uncertainty principle.* The name is slightly misleading since the principle does not actually refer to any uncertainty but to *indeterminacy*. According to the uncertainty principle, it is not possible to simultaneously determine with great accuracy two quantities whose product has the dimension of energy × time. This does of course directly concern the quantities of energy and time, but also things such as location and momentum. Such quantities are called *canonic variables*. The uncertainty principle is usually written as follows:

$$\Delta x \times \Delta p \geq h/2\pi \qquad (17.9)$$

Here, h designates Planck's constant but Δ is no longer the operator that is used in the expression for the Schrödinger equation; Δx and Δp instead designate the indeterminacy of the position of a particle after the x-coordinate or its momentum component $p = mv_x$ in the same direction. For example, Δx for an electron around the nucleus of an atom is very large since we cannot say exactly where the electron is; we have to use a probability measurement instead. However, this means that the momentum of the electron can be very accurately determined. If we see particles as packets of matter waves with the wavelength of $\lambda = h/(mv) = h/p$ and can determine the momentum p and thereby the wavelength λ with great accuracy, we are dealing with a single sine wave and thereby a wave packet of infinite extent, i.e., great uncertainty as to the position of the particle. If we can determine the position well, i.e., a very short-wave packet, this is possible only through interference between many waves with different wavelengths, which makes the momentum indeterminate.

Heisenberg wrote to Pauli about his indeterminacy relation and received a positive response: 'The day for the quantum theory is now dawning'. Bohr was not quite as positive, which led to heated discussions, this time between Bohr and Heisenberg. Bohr had been on a skiing holiday in Norway and had found his own principle which he was now taking care of. It was called the 'complimentarity' principle. Bohr wanted to see it based on wave-particle dualism. In short, it means that it is not possible for a quantum mechanical system to determine all physically measurable quantities at the same time. The measurement of one of the two canonic quantities has an indeterminate influence on the other quantity. In classical physics, measurements also influence the observed system, but in a manner that can be calculated. In quantum physics, the relationship between cause and effect is lost at atomic level. The only things that remain are probabilities. And the reality that lies behind our observations 'consists' of neither particles nor waves. These are but models that we use to describe reality.

In September 1927, the 100th anniversary of Allesandro Volta's death was celebrated in Italy with a major congress on physics in Como. The majority of important physicists went there since quantum physics was now the subject of debate. However, a few were missing, including Albert Einstein who did not want to do anything that might be seen as supporting the fascist Italian government under Benito Mussolini. Just in time for the congress, Bohr and Heisenberg, following a long exchange of views which had caused Heisenberg to burst into tears on occasions, had agreed that the indeterminacy principle was a special case of Bohr's more wide-ranging complementarity principle.

In Como, as a young student, *Emilio Segrè* (1905-1989) listened to Bohr's lecture and summarised much later the way that the concept of complementarity could be explained:

> Two magnitudes are complementary when the measurement of one of them prevents the accurate simultaneous measurement of the other. Similarly, two concepts are complementary when one imposes limitations of the other.

Immediately after the congress in Como, the annual Solvay Conference was held in Brussels*, and Einstein was now able to attend. He did not approve of quantum mechanics in the manner that Bohr and Heisenberg presented them. It was the repudiation of determinism which tormented Einstein. He thought that everything was predetermined, even at atomic level, and if only one knew how, it would be possible to predict exactly what would happen. 'God does not throw dice' was Einstein's objection.

The discussions between Bohr and Einstein in Brussels in 1927 are historical. They took place not just during the conference, but continuously - they began at the breakfast table and continued long into the night. Einstein was 48 years old and Bohr 42. Heisenberg, who supported Bohr, was just 26. Einstein had found new arguments every morning and Bohr had crushed them every evening. Both were incredibly stubborn, and the discussions would continue until the war broke out in 1939. Bohr considered Einstein to be far too constrained by old causality concepts and far too lacking in imagination. When Einstein repeated his objection that God does not throw dice and that it was unreasonable to assume that events were completely random, Bohr's irritated response in the end was that 'it is not for us to prescribe how God should run the world'.

In the 1930s, the persecution of the Jews gained official support in Germany. It had already become increasingly common before Hitler seized power. In 1931, the twenty-five-year-old *Hans Bethe* (1906-2005) bore witness to an affront of Jews during a Sommerfeld seminar. When the latter was about to raise the outer blackboard, which was now full of writing, this revealed the text 'DAMNED JEWS' in capital letters on the board behind.

A number of the European Jews who would later come to play a significant role in the continued research came from Hungary: Georg de Hevesy, Leo Szilard, Eugene Wigner, *John von Neumann* (1903-1957) and Edward Teller. Of these, de Hevesy had already left Hungary in 1904 to work with Fritz Haber in Berlin and then with Rutherford. He returned to Budapest to accede to a professorship in 1918, but left Hungary again in 1919.

After the First World War, Hungary was not exactly a salubrious place for intellectual Jews from affluent families, which was the case for all of the five mentioned above. The Hungarian Republic had been declared on 16 November 1918 after the fall of the dual monarchy, but had already been transformed into the Hungarian Soviet Republic by 21 March 1919, which led to a 133-day reign of terror under *Béla Kun* (1886-1939). The red terror was followed by a white terror under the ultra-

* The Solvay conferences were arranged every three years at the Solvay Institute for Physics and Chemistry in Brussels. The Institute was founded in the 1890s by the Belgian industrialist *Ernest Solvay* (1838-1922), who is known as the inventor of the Solvay process for the production of soda.

conservative Admiral *Miklós Horthy* (1868-1957) until 1944. In 1920, Horthy introduced the first racial law, which limited the options of education for the Jews. The racial persecution increased drastically through the formation of a Nazi party, the *Arrow Cross Movement*, in 1940. When Hitler removed Horthy in 1944, the persecution of the Jews culminated until the Germans were driven out of the country in 1945. It was during this final persecution that *Georg Klein* (1925-2016), now a well-known tumour biologist and author in Stockholm, succeeded in escaping and then moved to Sweden.

It is easy to understand why de Hevesy left Hungary again in 1919 and why physicists like Szilard, Wigner and Teller were exiled and viewed the parallel development in Germany with great unease. Ironically enough, these physicists became driving forces behind the development of the American nuclear weapon intended for use against Germany but which was instead used in Japan.

From 1924-1927, German diplomat *Ernst von Weizsäcker* (1882-1951) was counsellor of legation in Copenhagen. During the Second World War, Weizsäcker became Secretary of State of the German Ministry of Foreign Affairs and von Ribbentrop's second-in-command. He was conservative but did not approve of Nazism and took great risks to prevent the war. After the war, he had close connections with the German resistance movement. In spite of this, he was sentenced to prison for participating in the attack on The Czech Republic but was released after 18 months.

Weizsäcker's sons, Richard and Carl Friedrich, both became very well-known. *Richard von Weizsäcker* (1920-2015) became President of West Germany and Carl Friedrich von Weizsäcker (1912-2007) would become a nuclear physicist and philosopher, an early colleague of Lise Meitner and a close colleague of Heisenberg. Heisenberg and Carl Friedrich met for the first time in Copenhagen in 1927 when Heisenberg visited von Weizsäcker's home. Thirty years later, Carl Friedrich would describe the event as follows (according to Thomas Powers):

> One day my mother told me that she had met a very young German scientist, who was working with the famous Danish physicist Niels Bohr (whose name was just known to her) - he had played the piano so well and he had been a nice man. I asked, 'What is his name?' and she said, 'His name is Heisenberg'. And then I said, well, I have just read his name in one of the periodicals I then had as a boy in which new things on science were reported and I said, 'I must see the man – you must invite him.' And so she invited Werner Heisenberg and I met him when he was 25 and I was 14. We had long discussions and I was very much struck by the fact that here was a man who knew everything better than I (because I had been a little bit proud of my achievements) but he was better in physics anyway, and in mathematics, that was clear, but also in speaking Danish and English and in music (which I was not able to do) and in skiing at which I wasn´t good and even in chess where I was a little bit better.

They met by chance in Berlin a few weeks later, just as Heisenberg was elated at having completed although not yet having published his essay on the uncertainty principle. Heisenberg said: 'I think I've disproved the law of cause and effect!' von Weizsäcker then decided to study physics in order to be able to continue talking to Heisenberg. A friendship that would last for the whole of Heisenberg's life was formed.

In 1932, Heisenberg introduced Carl Friedrich to Bohr, and the young von Weizsäcker came to listen to the discussion between Bohr and Heisenberg, which was difficult to follow and during which Bohr appeared to be utterly straining to make himself clear. Afterwards, Weizsäcker wrote in his diary that that was the first time he had seen a physicist: 'He struggles when he's thinking'.

On 30 January 1933, a 43-year-old Adolf Hitler became Chancellor of Germany, which signalled the onset of the serious persecution of the Jews. Pauli, safe in Zürich, had calmed those who were worried by saying that a dictator could be possible in Russia but never in Germany. He was quick to change his view. Antisemitic racial laws in April 1933 precluded Jews from State institutions and universities. One quarter of Germany's physicists became unemployed. Einstein, having more foresight than most, had already made off and others now followed in haste. Among the few who stayed behind was Lise Meitner. She was an Austrian citizen and the Kaiser-Wilhelm Institute was not

State-owned, so she was safe for the moment. She had never considered herself to be Jewish - she had a Protestant upbringing and had been christened, but was soon forced to wear a Star of David.

Niels Bohr turned up in Germany to find out about the situation for himself. In Hamburg, he met Lise Meitner's nephew *Otto Robert Frisch* (1904-1979), a young experimental physicist who had returned from a year with Fermi in Rome but who now saw no possibility of staying in Germany. Frisch had recently verified the quantum mechanics conclusion whereby an atom that emits a photon must be exposed to a recoil effect, a bit like a gun firing a shot. The belief was that it would not be possible to show the recoil of an atom, which had to be pretty insignificant, but the skilful Frisch had succeeded. Frisch has described Bohr's visit (according to Rhodes):

> To me it was a great experience to be suddenly confronted with Niels Bohr – an almost legendary name for me – and to see him smile at me like a kindly father; he took me by my waistcoat button and said:
>
> 'I hope you will come and work with us sometime; we like people who can carry out "thought experiments"!' That night I wrote home to my mother and told her not to worry: The Good Lord himself had taken me by my waistcoat and smiled at me. That was exactly how I felt.

Several of the non-Jewish physicists were indignant about what had happened. Max Born had silently left Göttingen, but mathematician Richard Courant refused to go away. Along with some colleagues, he wrote a letter of protest which was sent to 65 leading German researchers asking for their signature. 16 of the addressees never responded and 21 refused to sign. Among those who had the courage of their convictions to sign the letter were Max Planck, Max von Laue, Sommerfeld and Heisenberg, but the letter proved to be fruitless. Courant finally realised that it would be insane to remain in Nazi Germany. He moved to the USA and was Professor at New York University from 1934-1958.

Heisenberg, who became Professor in Leipzig in 1927 and gained a worldwide reputation in 1933 by winning the Nobel Prize in Physics for his work with the uncertainty principle, considered leaving in protest, but remained in his job in the hope of being useful. He thought that the ongoing madness would not last long.

In his capacity as president of the Kaiser-Wilhelm Society, Max Planck went to see the new statesman Adolf Hitler as etiquette demanded. He took the opportunity to put in a good word for Fritz Haber, bearing in mind the latter's achievements during the war, but was met by a fit of rage from Hitler. All Jews are the same, yelled Hitler, they stick together like limpets. This was what Planck said to Heisenberg later (according to Powers):

> I am afraid I can no longer advise you. I see no hope of stopping the catastrophe that is about to engulf all our universities, indeed our whole country … You simply cannot stop a landslide once it has started … Hence I can only say this to you: No matter what you do, there is little hope that you can prevent minor disasters until this major disaster is over. But please think of the time that will follow the end.

Otto Hahn also visited Planck to try and organise a mass protest from German scientists, but Planck realised that it was already too late. He said:

> If today thirty professors get up and protest against the government´s actions, by tomorrow there will be 150 individuals declaring their solidarity with Hitler, simply because they´re after the jobs.

Heisenberg's resentment against what was going on lost him friends. The fact that he refused to join the national socialist party did not improve matters. His personal problems increased. His hope was to be able to succeed Arnold Sommerfeld in Munich when the latter retired. The faculty had him down as one of three possible candidates but the Ministry of Education removed his name - his close contact with Bohr and Einstein had made him suspicious and, let's face it, he was not a member of the party.

Heisenberg was exposed to ever increasing bitter attacks, mainly from his older physicist colleagues Philipp Lenard and *Johannes Stark* (1874-1957), who viewed the new physics with abhorrence. In 1935, Lenard published his masterpiece, *Deutsche Physik*, in four volumes in which he uses a preface to pit 'German physics' against 'Jewish physics' as incompatible. He wrote (quoted from *I Demokritos fotspår*):

> German physics you ask yourself! I could also have written Aryan physics or Nordic physics, the physics of those researching reality, the physics of those seeking the truth, or the physics of those who have founded natural science. Science is and will remain international is the objection I hear being raised. But that is a mistake! Science is actually like everything else accomplished by people, determined by race and kin.
>
> [...]
>
> People who are of other mixed race are predestined for another type of science. Nothing is yet known about Negro physics. On the other hand, a strange Jewish physics has developed far and wide.

Lenard then bitterly attacks Einstein as the primary representative of 'Jewish physics', which is incompatible with 'the equally unaccommodating and scrupulous will of Aryan research to seek the truth'.

In December 1935, Stark made an address at a ceremony at which the physics institute in Heidelberg was being named after Lenard, and at which he attacked Heisenberg. The latter responded with a well-balanced defence of theoretical physics and the theory of relativity in an article in February 1936 in the Nazi Party organ *Völkischer Beobachter*. However, at the same time, the editors introduced a new attack from Stark who dismissed Heisenberg's article as Jewish delusions.

Stark's persecution of Heisenberg culminated in July 1937 with an article in the SS magazine *Das Schwarze Korps*, in which he called Heisenberg a 'white Jew'. At the same time, there was an editorial notice containing a recommendation that 'white Jews' like Heisenberg ought to be made to 'disappear'. This was a threat which Heisenberg took very seriously.

Now, it just so happened that Heisenberg's maternal grandfather had been a teacher at the same school as the father of the head of the SS, *Heinrich Himmler*, and that the two had been good friends. Heisenberg therefore asked his mother to contact Himmler's mother, a widow in her seventies. Mrs. Himmler reputedly said:

> My heavens, if my Heinrich only knew of this, then he would immediately do something about it. There are some slightly unpleasant people around Heinrich, but this is of course quite disgusting. But I will tell my Heinrich about it. He is such a nice boy – always congratulates me on my birthday and sends me flowers and such. So if I say just a single word to him, he will set the matter back in order.

On Mrs. Himmler's recommendation, Heisenberg wrote to Himmler on 21 July 1937 and protested against the persecution from Stark, declaring that he would feel obliged to leave his professorship in Leipzig unless Himmler could put a stop to the attacks in the SS magazine. Himmler did not get in touch until November and encouraged Heisenberg to defend himself against the attacks. Others joined in his defence, including Ernst von Weizsäcker. An SS investigation was carried out under the supervision of *Reinhard Heydrich*, Himmler's second-in-command and head of the Gestapo, and Heisenberg was repeatedly called to hearings at the Gestapo's headquarters in Berlin. The investigation was not finished until 21 July 1938, and Himmler wrote to Heisenberg to say that he had now been declared free and that he would not be the victim of any more attacks. The investigation had been carried out in such detail just because Heisenberg had been recommended by Himmler's mother. On the other hand, Heisenberg could not succeed Sommerfeld since Rudolf Hess opposed this 'for political reasons'.

Heisenberg had escaped by the skin of his teeth. He had been closer to death than he realised. On the same day that Himmler wrote to Heisenberg, he also wrote to Heydrich declaring 'that Heisenberg is a respectable person and we can't afford to lose or destroy this man who is still young and can still create an advanced generation within science'. Heisenberg escaped being killed, but what would he be able to do?

18. THE REVELATION OF THE ATOMIC NUCLEUS

Following our insight into theoretical physics, let us link it to experimental physics and go back to Professor Rutherford and his colleagues at the University of Manchester. We'll renew our acquaintanceship with him in March 1911, just after Rutherford had published his explanation of Geiger's and Marsden's demonstration that alpha particles can also ricochet off very thin gold leaf. The atoms had been shown to have nuclei. The next task was now to reveal the nature of the nuclei.

There was an almost hundred year-old hypothesis that could be of assistance. In 1815, an English doctor and chemist, *William Prout* (1785-1850), had guessed that all atoms were made of hydrogen atoms. He found that the atomic weights of the elements appeared to be integral multiples of the atomic weight of hydrogen.* This was well suited to many elements such as helium, carbon, oxygen, phosphorous, sulphur, calcium, iron, cobalt and arsenic, but poorly suited to others such as silicon, chlorine and copper.

The idea was appealing, however. What could be simpler than if the hydrogen atom or, to be more accurate, its nucleus, the proton (from the Greek *protos* = first) were the constituent of all matter? If that were the case, the atom of all atoms had been found.

But the hydrogen atom has only one electron. Its nucleus can therefore have only *one* positive electric charge. Bohr's atomic model and Moseley's x-ray spectrometer results required heavier atoms to have nuclei with Z times the nuclear charge of the hydrogen atom if Z were the ordinal number in the periodic system, i.e., the atomic number. If Prout's hypothesis were correct, the atomic number would therefore be equal to the atomic weight (which is a dimensionless number if calculated in relation to the weight of the hydrogen atom). But for elements other than hydrogen, the atomic weight is approximately twice the size of the atomic number, and it is even greater for heavy atoms.

The atomic nucleus could therefore not consist purely of protons. It was not yet known whether there were any protons at all in atomic nuclei other than that of hydrogen, but if there were, the positive charge in approximately half of them had to be neutralised, e.g., by electrons also being in the nuclei. This was no far-fetched idea since some of the radioactive substances emitted electrons in the form of β radiation. For example, in beryllium, with the atomic weight 9 and the atomic number Z = 4, if the nucleus consisted of protons, 5 of them would need to be neutralised with electrons inside the nucleus because on the outside it has 4 positive charges.

It looked as though Doctor Prout's hypothesis from 1815 might not be a silly one after all (maybe the protons should actually have been called 'proutons'?). There was one objection though. How would the atomic weights that were not integral multiples of the hydrogen atom be explained, such as the atomic weight of chlorine at 35.5?

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* The *atomic weight* is an older designation of the relative atomic mass, i.e., a number that states the mass of an atom in relation to a reference atom. Prout used the hydrogen atom as a reference. Later on, oxygen-16 was used as a reference and its atomic weight was called exactly 16. Carbon-12 is now used as a reference, so its atomic weight is exactly 12. Within chemistry, the atomic weight of an element is stated as the average atomic weight of its natural isotope mixture. Carbon that occurs in nature (98.9% carbon-12 and 1.1% carbon-13) therefore has the atomic weight 12.011.

Explanations were proposed. In 1909, the two Swedes *The Svedberg* (1884-1971) and *Daniel Strömholm* (1871-1961) had already demonstrated the possibility of there being several different atomic species of each element. There was now a whole lot of experience of the radioactive substances which had shown that there were different physical properties (half-life, type of radiation) in atomic species which were shown to have the same chemical properties. Soddy had called such atomic species with same position in the periodic system *isotopes*. Since they have the same atomic number (Z), they have the same nuclear charge. What could be expected to be different was the number of electrically neutral particles of the nucleus, i.e., the number of 'neutrons'. But these were hypotheses for the moment; no one had yet demonstrated any such particle. Nor had anyone actually shown that the atomic nuclei contained protons, i.e., hydrogen atomic nuclei.

To begin with, it was not known whether the presence of isotopes was limited to radioactive substances or whether there could also be stable isotopes. However, in 1912, i.e., before Soddy had even proposed the name 'isotope', J.J. Thomson had shown that there were two isotopes of neon. He had ionised the gas and accelerated the neon ions in a discharge tube in which he could allow a current of ions to penetrate through the cathode as 'channel rays' (see chapter 6) and then attempt to deflect the rays in electric and magnetic fields. The deflections were different owing to the different masses of the isotopes, and Thomson was able to calculate the mass of the ions. He then found that neon consisted of two atomic species with the atomic weights 20 and 22. It looked as though Dr. Prout may have been right.

We now know that the atomic weight 35.5 for chlorine is due to the fact that chlorine occurs in the form of two stable isotopes with the atomic weights 35 and 37 and the relative abundances of 75% and 25%. This makes the atomic weight for the mixture equal to $0.75\times35 + 0.25\times37 = 35.5$ (I have rounded off the abundance numbers for the sake of simplicity). Most of the elements show stable isotopes, even if some of the isotopes have a fairly small abundance.

After the war, in 1919 when Rutherford left Manchester to become Professor at Cambridge and succeeded J.J. Thomson as director of the Cavendish Laboratory, it was time to evaluate the next big discovery from the Rutherford group. Like many big discoveries, it began as an inexplicable observation, an irritating disturbance to an experiment. Many researchers have found that it is not worthwhile sighing over an experiment that has gone wrong – it is better to turn the error to your advantage and argue that 'If this strange thing has happened now, I might as well try and make use of it. It's a type of research jujitsu, i.e., 'utilising the force and movement of the attacker to defeat him'.

It all started in 1915 when Ernest Marsden made an inexplicable observation in a routine experiment. He used a α-radiating preparation in an enclosed chamber for the experiment. At one end of the chamber there was a screen with zinc sulphide that fluoresced if the screen was hit by an energy-rich particle. He first pumped the air out of the chamber and then filled it with hydrogen gas. When the α particles collided with the lighter hydrogen atoms, these were knocked against the screen where gleams of light could be observed. The screen could be protected against the α particles by covering it with a thin film so that only the hydrogen atoms reached their target.

What surprised Marsden was that he already found scintillations on the screen while he was evacuating the chamber and therefore had not filled it with any hydrogen gas. It seemed as though in addition to its α radiation, the α-radiant preparation also emitted energy-rich hydrogen atomic nuclei ('H particles' was the term used at the time). Rutherford had already discovered α radiation and β radiation. Could there also be a natural H radiation?

Marsden left Rutherford in 1915 to return to New Zealand and Rutherford was overloaded with all sorts of assignments during the war so he had no time to immediately follow up Marsden's observation. He still occasionally stole some time for himself to investigate whether Marsden's results, which he was able to repeat himself, could possibly be due to the fact that hydrogen after all was present. He found that scintillations decreased if he filled the chamber with oxygen gas or carbon dioxide but, to his surprise, they were at their greatest when he had air in the chamber. They were equal in intensity to those caused by the knocked hydrogen atoms when he filled the chamber with hydrogen gas.

In the end, Rutherford concluded that it was hydrogen atoms that collided with the zinc sulphide screen and that they had originated when the α particles collided with the nitrogen in the air. In 'firing' α particles at nitrogen atoms, i.e., the nuclei of helium atoms, he had split the nitrogen nuclei into oxygen and hydrogen. Rutherford had produced the first nuclear reaction to be created by any human being.

The atomic nucleus of helium has the atomic weight 4 and the atomic weight of nitrogen is 14. If the collision created a hydrogen atom with the atomic weight 1, there must also have been a residue with the atomic weight 17. The atomic nucleus of helium had had two positive electric unit charges and nitrogen with the atomic number 7 had had 7 charges. The hydrogen nucleus had been repelled with a charge with 2+7−1 = 8 charges remaining. The residual product must therefore have the atomic number 8 and be oxygen, an oxygen isotope with the atomic weight 17. It had not previously been observed in nature, but it is there, albeit in very small quantities.

It was 1919 before Rutherford dared to interpret his observations. 'We must draw the conclusion that the nitrogen atoms have disintegrated....' His report led to newspaper headlines about 'nuclear fission', which was misleading. On the other hand, he had succeeded in knocking a hydrogen atom out of the atomic nucleus of nitrogen.

What was missing now was the conformation that the atomic nucleus also contained the neutral particles required to explain the relationship between the mass and the electric charge of the atoms. Rutherford had noted the possibility that there were also electrons inside the atomic nucleus, sufficient in number to neutralise the positive charge of just that number of hydrogen atomic nuclei which would be necessary for the remaining positive charge to be as expected.

Rutherford saw such a neutralised proton, i.e., a system consisting of a proton and an electron, as being a special, neutral particle which he called the *neutron* in 1920. About this, at the time still hypothetical, particle he had the following to say on 3 June 1920 in a lecture ('the Bakerian Lecture') before the Royal Society:

> Under some conditions, it may be possible for an electron to combine much more closely with the H nucleus, forming a kind of neutral doublet. Such an atom would have very novel properties. Its external field would be practically zero, except very close to the nucleus, and in consequence it should be able to move freely through matter. Its presence would probably be difficult to detect by the spectroscope, and it may be impossible to contain it in a sealed vessel. On the other hand, it should enter readily the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field.
>
> The existence of such atoms seems almost necessary to explain the building up of the nuclei of heavy elements; for unless we suppose the production of charged particles of very high velocities it is difficult to see how any positively charged particle can reach the nucleus of a heavy atom against its intense repulsive field.[10]

A neutron of the type imagined by Rutherford would have the approximate mass of the proton and be the 'neutral proton' that was required to get the connection between atomic number and atomic weight to correspond.

When Rutherford came to Cambridge and the Cavendish Laboratory to succeed J.J. Thomson in 1919, he was accompanied by a young Manchester physicist, *James Chadwick* (1891-1974). Rutherford's former colleague Hans Geiger had returned to Berlin just before the war and Chadwick, who had been awarded a grant after having done his M.Sc. in Manchester in 1913, had travelled there to visit him and to meet famous researchers like Einstein and Hahn. When the war broke out in 1914, Chadwick did not manage to get home, but was interned in a primitive prisoner of war camp in Ruhleben near Spandau. He had recently been released when he accompanied Rutherford to the Cavendish Laboratory.

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[10] E Rutherford 1920: Bakerian Lecture. Nuclear Constitution of Atoms. Proc.R.Soc.Lond. A, 97 (686): 374-400

A colleague whom Rutherford took over from Thomson was *Francis William Aston* (1877-1945), who won the Nobel Prize in Chemistry in 1922. In 1921, another future Nobel Prizet winner came to Cambridge: *Pjotr Kapitsa* (1894-1984), Russia's most prominent physicist.

Aston won the Nobel Prize for his 'discovery by means of the mass spectrograph of a large number of isotopes of the radioactive elements', discoveries which created a good basis for understanding the structure of the atomic nucleus. The mass spectrograph was based on J.J. Thomson's discovery in 1913 that a bundle of channel rays could be broken down by a combined electric and magnetic field so that ions with different masses were deflected by different amounts. Aston designed the first actual mass spectrograph according to this principle.

Chadwick had accidentally become a physicist. When applying to the University of Manchester and joining a long queue for an interview for admission to mathematics, it just so happened that he had selected the wrong queue. When he came to the lecturer and was given his questions, he realised that he had been interviewed for physics rather than mathematics. He did not correct the mistake because he found the lecturer interesting and thought it would be far too much of a nuisance to explain the mistake.

Rutherford, who had initially predicted the existence of neutrons with great enthusiasm, became downhearted when the atomic nuclei eluded the researchers. In 1927, he wrote the following in a paper: 'We still can do no more than make conjectures as to the structure of the lighest and probably least complicated atoms'. But things started to happen one year later.

In Germany, physicist *Walther Bothe* (1891-1957) built on Rutherford's method of breaking away protons from the atomic nuclei of light elements by bombarding them with α particles. Together with his colleague *Heinrich Becker* (1911-1942), he registered the γ radiation that occurred when irradiating elements such as boron, magnesium and aluminium following nuclear reactions corresponding to the one that Rutherford discovered when irradiating nitrogen. This was no surprise although, when irradiating beryllium, they did find what which they believed was γ radiation had an unexpectedly high penetration capacity. Nor were they able to detect any protons after irradiating the beryllium. Bothe and Becker wrote about their unexpected finds in August and December 1930.

Marie Curie's daughter *Irène* (1897-1956) had married physicist and chemist *Frédéric Joliot* (1900-1958) in 1927. From 1929, they began to work together and repeated the German experiments with beryllium. On 28 December 1931, Irène Joliot-Curie was able to report the first results to the French Academy of Sciences. The mysterious radiation from beryllium was even more energy-rich than the Germans had found.

The Joliot-Curie couple continued their studies by investigating the properties of the new radiation. Could it in turn generate nuclear reactions? They used an ionisation chamber with a thin window so that particles could also penetrate the chamber and generate measurable ionisation. In front of the window they placed different materials that they exposed to the radiation from the beryllium preparation, which was irradiated with α particles. They found that the irradiated materials released protons, but only if they contained hydrogen atoms, like in paraffin. It seemed as though the radiation from the beryllium knocked hydrogen atoms out of the paraffin. The two French researchers still did not understand the context - they believed that the radiation from the beryllium was very energy-rich γ radiation. On 18 January 1932, they reported to the Academy of Sciences that paraffin released energy-rich protons if it was exposed to 'very penetrative γ radiation'. In doing so, they, like Bothe and Becker, missed discovering the neutron. They looked without seeing.

When the report in *Comptes Rendus* arrived at the Cavendish Laboratory in early February 1932, Chadwick and Rutherford could not believe their eyes. One of their colleagues, *Norman Feather* (1904-1978), equally surprised came to see Chadwick, who later wrote:

> Not many minutes afterward Feather came to my room to tell me about this report, as astonished as I was. A little later that morning I told Rutherford. It was a custom of long standing that I should visit him about 11 a.m. to tell him any news of interest and to discuss the work in progress in the laboratory. As I told him about the Curie-Joliot observation and their views on it, I saw his growing amazement; and finally he burst

> out 'I don´t believe it.' Such an impatient remark was utterly out of character, and in all my long association with him I recall no similar occasion. I mention it to emphasize the electrifying effect of the Curie-Joliot report. Of course, Rutherford agreed that one must believe the observations; the explanation was quite another matter.[11]

Chadwick, who had performed similar experiments while the research had been taking place in Germany and France, could now cheerfully continue - he knew what he was looking for. His previous experiments had been made difficult by the lack of an appropriate source of α radiation. The radiation sources he used (radium, radon and early daughter products) emitted not just α rays but also γ rays, which disrupted the measurements. The Joliot-Curie couple had access to large quantities of polonium. Glass ampoules containing radon had been very useful in medicine as short-lived sources of γ radiation. When all other daughter products had decayed, what remained in the ampoules was the long-lived lead-210 ('radium D') which has no energy-rich γ radiation, and which in turn through relatively short-lived bismuth-210 ('radium E') builds up an equilibrium quantity of polonium-210 emittting α particles. The used glass ampoules were often sent back to Marie Curie's radium institute which thereby had access to very large quantities of polonium.

However, in 1930, the Cavendish Laboratory had gained access to a hundred used radon ampoules from the Kelly Hospital in Baltimore (where Norman Feather had spent a sabbatical year). Chadwick separated the polonium in autumn 1930 and had thereby gained a useful source of α radiation.

From the 7th up to and including 17 February 1932, Chadwick worked hectically and almost continuously with his search for the neutron and begrudged himself just a few hours' sleep each night. He started by repeating the Joliot-Curie experiment to ensure that the particles that the neutron radiation supposedly knocked out of paraffin were actually protons. He then continued by allowing the radiation from the beryllium to hit substances other than paraffin and was able to show that it could not be γ radiation as had been believed in Paris.

On 17 February, he sent a notification to the *Nature* magazine with the heading 'The possible occurrence of a neutron', in which he drew the conclusion that the mysterious beryllium radiation consisted of the 'neutron' as discussed by Rutherford in his Bakerian Lecture in 1920. The neutron had been discovered.

Chadwick's discovery was celebrated on the evening of that same day by the 'Kapitsa Club'. Follwing a period at Cambridge, Pjotr Kapitsa had thought that the physics students behaved too obsequiously to their teachers as did the teachers to older Professors. He therefore formed a closed discussion club, membership of which was very sought after. He often introduced the discussions himself, including deliberate, obvious blunders so that nobody would hesitate to oppose him. On the evening of 17 February, Kapitsa ate dinner with Chadwick and then took him to the club where Chadwick gave an account of the discovery, expressed his appreciation of the German and French researchers who had been so close to the discovery, and finally shouted: 'I now want to be chloroformed and put in bed for fourteen days!'

The fact that all atomic nuclei consist of protons and neutrons was now quickly accepted. It was proposed by Heisenberg immediately after Chadwick's discovery, but was also exactly what Rutherford had long suspected. It was now possible to introduce a new concept, the mass number (A), which states the number of *nucleons* (i.e., protons and neutrons) that form the nucleus. It was now understood that all matter was made up of three *elementary particles* (there would be many more): electrons, protons and neutrons. The number of protons and thereby the number of positive electric unit charges in the nucleus was designated by Z, a number which thereby also determines the number of electrons that can normally be found in the atom outside the nucleus and influence the chemical properties of the atom. A nuclide of the substance X with A nucleons, of which Z are protons, could

[11] Original English text by Richard Rhodes (1986), quoting Chadwick.

thereby be designated as ${}^{A}_{Z}X$. Rutherford's first nuclear reaction where he released a hydrogen nucleus (proton) from nitrogen using a nucleus of the helium atom (α particle) could thereby be written as

$$ {}^{14}_{7}\mathrm{N} + {}^{4}_{2}\mathrm{He} \rightarrow {}^{17}_{8}\mathrm{O} + {}^{1}_{1}\mathrm{H} \qquad (18.1)$$

In a similar way, the emission of a neutron from a beryllium preparation irradiated with α particles can be written as

$$ {}^{9}_{4}\mathrm{Be} + {}^{4}_{2}\mathrm{He} \rightarrow {}^{12}_{6}\mathrm{C} + {}^{1}_{0}\mathrm{n} \qquad (18.2)$$

It was actually unnecessary to write out the atomic number Z since the symbols of the chemical elements (N, He, O and H) also unambiguously determine which elements are involved. In a formula like the one above, however, it can help you do a quick check that not just the number of nucleons but also the overall nuclear charges are the same after the reaction as they were before it.

Another reminder about the terms nuclide and isotope might be appropriate here. All of the atomic species ${}^{1}_{1}H$, ${}^{2}_{1}H$, ${}^{35}_{17}Cl$, ${}^{37}_{17}Cl$, ${}^{9}_{4}Be$ are *nuclides*, while hydrogen-1 and hydrogen-2 (which you can also write) are *isotopes* of hydrogen (the same value of Z and the same chemical properties), as chlorine-35 and chlorine-37 are isotopes of chlorine. In nature, stable beryllium occurs only in the form of the nuclide beryllium-9, which thus has no stable isotope. On the other hand, short-lived radioactive ${}^{7}_{4}Be$ is constantly formed in the atmosphere through nuclear reactions with the cosmic radiation. The nuclide thus consists of 4 protons and 7 - 4 = 3 neutrons.

In layperson's terms, many people incorrectly refer to a nuclide as an isotope. People can correctly say, for example, that 'beryllium-7 is a radioactive isotope of beryllium' which, to be more precise, means that 'the nuclide beryllium-7 is a radioactive isotope of beryllium'. If you simply say that 'beryllium-7 is a radioactive isotope', it is implied that it is from beryllium. If, on the other hand, you say that 'beryllium-9 is an isotope', that is incorrect. Beryllium-9 is a nuclide.

Nuclear research took off following the discovery of the neutron. Hans Bethe, not to be confused with Bothe, has said that the period before 1932 was 'the prehistory of nuclear physics and from 1932 onwards the history of nuclear physics'. However, it was not only Chadwick's discovery of the neutron that was sensational in 1932 – a further noteworthy discovery was made at the Cavendish Laboratory in the same year.

People had been looking for more suitable projectiles than the heavy nucleus of the helium atom with its double electric charge. The lighter and singly-charged proton would be suitable, but how would we be able to provide it with sufficiently high energy? A 'proton cannon' was needed.

Once again, it was researchers at the Cavendish Laboratory who were the first to come forward, i.e., *John Cockcroft* (1897-1967) and *Ernest Walton* (1903-1995). They designed a suitable proton source where hydrogen was ionised in a gas discharge, plus a high voltage generator that could generate electric voltages of up to one million volts (I actually worked with such a generator myself in around 1950).

Using protons accelerated by a voltage of 0.6 million volts, Cockcroft and Walton first bombarded lithium in 1932 and immediately succeeded in obtaining a nuclear reaction:

$$ {}^{7}_{3}\mathrm{Li} + {}^{1}_{1}\mathrm{H} \rightarrow {}^{4}_{2}\mathrm{He} + {}^{4}_{2}\mathrm{He} \qquad (18.3)$$

This was actually the world's first experimental nuclear fission. The shot lithium atom had become two helium atoms. But the reaction was not actually possible! A calculation of the energy that would be required for the proton to penetrate the lithium nucleus against the repulsive coulomb forces showed that it was approximately 1.24 MeV while the energy of the protons was only 0.6 MeV.* However, this barrier is effective only in the world of classical physics. If you instead set up a wave mechanical

* An electron-volt (eV) is the kinetic energy that a particle with an electric unit charge receives when it (in vacuum) has been accelerated by an electric potential difference of 1 volt. So, 1 MeV (one megaelectron-volt) is the kinetic energy that such a particle receives when it has been accelerated by one million volts. The energy 1 eV is equal to $0.160217733 \cdot 10^{-18}$ joules.

approach, you find that there is some probability of a particle passing through the barrier, the so-called *tunnel effect*.

Inversely, some of the nucleons that form an atomic nucleus have a certain probability of escaping from the nucleus through the tunnel effect, in spite of the forces that normally keep them together. However, we now know that this does not usually apply to single protons or neutrons but to α particles, i.e., a configuration of two protons and two neutrons. It is the small probability of α particles escaping which makes heavier atoms radioactive and α emitting. It has now been discovered that heavier configurations of nucleons can also escape through the tunnel effect, although with a very small probability (one example is carbon-14). The particular ease with which α particles could escape had already been theoretically demonstrated in the 1920s by the then Soviet physicist *George Gamow* (1904-1968). Gamow had a doctorate in physics from the University in what was then Leningrad, but was studying in Western Europe at the end of the 20s, including with Niels Bohr in Copenhagen. He returned to Leningrad in 1931 for a job at the Academy of Sciences.

Early on, it was also shown that the particles, e.g., protons or α particles, that were emitted during nuclear reactions could have very high kinetic energies. When Cockcroft and Walton split the lithium atom into two helium nuclei (i.e., α particles), they found that each of these had a kinetic energy of approximately 9 MeV. The known Edinburgh Professor and popular science author *Ritchie Calder** noted Rutherford's response when Calder commented that staking energy of 0.6 MeV to get 18 MeV in return amounted to a good exchange. Rutherford responded: 'Nonsense. Only one proton in ten million has a chance of hitting the nucleus. It's like shooting a mosquito in the middle of a dark night and using ten million shots to do it.'

Rutherford seemed convinced that overall, more energy would always be needed to generate nuclear fission than that which could be obtained in return. But where did the energy come from on the occasions when fission did actually occur? The energy invested in unsuccessful shots had been lost completely.

The explanation lay in Einstein's theory of relativity, which says that mass and energy are equivalent and that the mass of a body is therefore also a measure of the energy it contains. According to the theory of relativity, the energy masquerading as mass in a body is equal to the rest mass of the body multiplied by the square of the speed of light, $E = m_o c^2$. When this was first said in 1905, it was an abstract speculation, but was now starting to take on practical significance.

However, Einstein had already received crucial support for his theories in 1919. In the early stages of the First World War in 1915 and with reference to his general theory of relativity, Einstein had predicted that the light from the stars would be deflected when it passed close to a very large mass like our sun. The prediction could be tested during a solar eclipse. The war came in the meantime, but after the war there was a total solar eclipse on 29 May 1919. Several observatories sent expeditions to suitable places and it was possible to observe the predicted deflection. J.J. Thomson ascertained: 'It's not a discovery of an isolated island but of a whole continent of new scientific ideas.' Einstein was thus a recognised and esteemed scientist in the early 1930s (except by the racist Nazis who hated him because he was a Jew).

Einstein's formula says that one kilogramme of any type of matter is the equivalent of approximately 25 billion kilowatt hours (25 terawatt hours, or TWh) - an enormous source of energy to draw from if only one knew how! If we instead state the energy in electron-volts, we can calculate that the mass unit of the atomic weight system (now 1/12 of the mass of carbon-12) is the equivalent of an energy of 931.16 MeV.** During the fission of the lithium atom, the two newly-formed helium nuclei

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* Later Lord Ritchie-Calder and also known as chairman of the committee that prepared the United Kingdom's change over to the decimal system.

** The *atomic mass unit* (u), i.e., 1/12 of the mass of carbon-12 is $1.66041 \cdot 10^{-27}$ kg. If we multiply it by the square of the speed of light, we get the equivalent energy in joules. If it is expressed in electron-volts instead, this is 931.16 MeV.

together had a kinetic energy of 18 MeV. According to Einstein's theory, this would mean that a mass of 18/931.16 = 0.019 atomic mass units had been converted into energy.

It was possible to verify this. The lithium nucleus (lithium-7) has a mass of 7.017 mass units and the proton 1.008 mass units, together 8.025 mass units. The helium nucleus has a mass of 4.003 mass units, so two nuclei together have 8.006 mass units. The difference is thus exactly 0.019 mass units. Einstein's theory was confirmed once more. The old law that energy cannot be destroyed now applied only if energy and mass are considered equivalent.

Comparisons could now be made between the total mass of the nucleons that form a given atomic nucleus and the measured mass of this nucleus, and a difference was found. The atomic nuclei were proven to have masses of less than the sum of the masses of the 'building blocks'. The difference is usually called the *mass defect* (M_B) and corresponds to a quantity of energy known as the *nuclear binding energy* (B). The binding energy is the energy that is released when a number of free protons and neutrons are joined together to form one atomic nucleus, but also the energy that is required to split the nucleus into completely free nucleons.

The crucial factor for the stability of an atomic nucleus is not its total binding energy but the binding energy per nucleon, i.e., B/A. With the exception of some of the nuclei of the lightest atoms - deuterium (hydrogen-2), helium-4, lithium-6, the binding energy per nucleon is around 8 MeV. The normal hydrogen atom lacks binding energy, consisting solely of the nucleus.

At first glance, these conditions do not appear to be particularly promising if you are looking to obtain energy from mass – it is by putting together nucleons, not by splitting or fissioning atoms into nucleons, that you can obtain energy. But Cockcroft's and Walton's fission of the lithium atom does give a lead. The lithium atom was not split up into nucleons but into two helium-4 atoms that have a lower binding energy per nucleon than the lithium atom and the proton together.

The maximum binding energy per nucleon occurs in nuclides with a mass number between 30 and 90. This means that splitting ('fission') of heavy atomic nuclei into fission products with lower mass numbers releases energy. But it is also possible to come close to the maximum binding energies and the mass numbers between 30 and 90 by bringing together or 'fusing' the atomic nuclei of light nuclides.

The mass of an atomic nucleus can be written as

$$M = Z \cdot m_p + (A - Z) \cdot m_n - M_B \qquad (18.4)$$

The first two terms after the equals sign sum up the masses of the Z protons and (A – Z) neutrons that are part of the atomic nucleus and whose masses are m_p and m_n, respectively. The third term is the mass defect, which corresponds to the binding energy $B = M_B c^2$. In an article in *Zeitschrift für Physik* in 1935, Carl Friedrich von Weizsäcker described the way in which the binding energy could be calculated using assumptions for the forces that influence the nucleons but with corrections for experimental results. Hans Bethe and *R.F. Bacher* simplified Weizsäcker's formula in 1936 to an expression that is usually known as the 'Bethe-Weizsäcker semi-empirical mass formula'. It gives the binding energy as the sum of a number of terms, primarily a 'volume term' that is proportional to the mass number A. Thereafter, there are a few negative terms, the most important of which constituted corrections because nucleons in the outer region of the nucleus cannot interact with an equal number of other nucleons like those that are deeper in the nucleus, and because the electrically-charged protons try to expand the nucleus through their repulsive forces on each other.

The first of these two correction terms can also be seen as the result of a surface tension corresponding to that of a drop of liquid. Bohr would publish an article on this along with *F. Kalckar* in 1937 in *Kgl. Danske Videnskab. Selskab's* mathematical physics communications, stating that the atomic nucleus can in some respects be compared to a drop of liquid. This 'liquid drop model' would become important when Lise Meitner and Otto Frisch were to interpret German chemist Otto Hahn's inexplicable results the following year, thereby discovering nuclear fission.

In 1939 in *Physical Review*, Hans Bethe published a proposal for a fairly complicated series of nuclear reactions whose net results involved a fusion of protons to form helium. To facilitate an electric charge balance, positive unit charges (*positrons*, i.e., electrons with a positive charge) also had to be released. The process also included carbon-12 as catalyst. Bethe proposed that this proton fusion could be responsible for the development of the energy from the sun. The nuclear reactions were:

${}^{12}_{6}C + {}^{1}_{1}H \rightarrow {}^{13}_{7}N + 1.95\,\text{MeV}$	(1 million years)	(18.5)
${}^{13}_{7}N \rightarrow {}^{13}_{6}C + {}^{0}_{1}e + 2.22\,\text{MeV}$	(10 minutes)	
${}^{13}_{6}C + {}^{1}_{1}H \rightarrow {}^{14}_{7}N + 7.54\,\text{MeV}$	(200 000 years)	
${}^{14}_{7}N + {}^{1}_{1}H \rightarrow {}^{15}_{8}O + 7.35\,\text{MeV}$	(30 million years)	
${}^{15}_{8}O \rightarrow {}^{15}_{7}N + {}^{0}_{1}e + 2.71\,\text{MeV}$	(2 minutes)	
${}^{15}_{7}N + {}^{1}_{1}H \rightarrow {}^{12}_{6}C + {}^{4}_{2}He + 4.96\,\text{MeV}$	(10 000 years)	

Since many terms occur in both sections, the net yield is quite simply:

$$4\,{}^{1}_{1}H \rightarrow {}^{4}_{2}He + 2\,{}^{0}_{1}e + 25\,\text{MeV} \qquad (18.6)$$

Positrons, which are similar to electrons in all respects except that their elementary charge is positive rather than negative, were demonstrated for the first time in 1932 by *Carl D. Anderson* (1905-1991) at the California Institute of Technology in Pasadena when he studied the cloud chamber photo of nuclear reactions generated by cosmic radiation. This brought Anderson the Nobel Prize for Physics in 1936. The positron has sometimes also been called the *antielectron*.

Hans Bethe's solar energy reactions are no longer considered to be those that cause the development of energy in the interior of the sun, but rather that within hotter stars. In the interior of the sun, it is presumed that protons, i.e., hydrogen nuclei, undergo fusion processes. In the first stage, two hydrogen nuclei form heavy hydrogen (*deuterium*) and an energy-rich positron:

$${}^{1}_{1}H + {}^{1}_{1}H \rightarrow {}^{2}_{1}H + {}^{0}_{1}e \qquad (18.7)$$

In the next stage, deuterium and hydrogen fuse to form a helium isotope with only one neutron (i.e., helium-3):

$${}^{2}_{1}H + {}^{1}_{1}H \rightarrow {}^{3}_{2}He + \gamma\ \text{radiation} \qquad (18.8)$$

Finally, two such helium-3 nuclei fuse to form a nucleus of normal helium-4 and two protons:

$${}^{3}_{2}He + {}^{3}_{2}He \rightarrow {}^{4}_{2}He + 2\,{}^{1}_{1}H \qquad (18.9)$$

The net result of these reactions is that hydrogen has been transformed into helium while releasing energy, including positrons:

$$4\,{}^{1}_{1}H \rightarrow {}^{4}_{2}He + 2\,{}^{0}_{1}e + \text{energy} \qquad (18.10)$$

The reaction takes place in the interior of the sun. The original energy-rich γ radiation and positron emission are transformed through repeated absorbtion and emission processes to more long-waved electromagnetic radiation, i.e., the heat radiation, UV radiation and visible light that reach the Earth.

In Paris, the Joliot-Curie couple continued their research, encouraged by Marie Curie whose health was starting to deteriorate. They had missed the discovery of the neutron but the startling progress in Cambridge spurred them on to renew their efforts. Irène had succeeded Marie Curie as head of the Radium Institute in 1932.

As might have been anticipated, the 1933 Solvay conference in Brussels would concentrate on nuclear physics. Many of the most prominent physicists were expected there: Bohr, Chadwick, Fermi, Heisenberg, Irène and Frédéric Joliot-Curie, Pauli and Rutherford to name but a few in alphabetical order.

In the Soviet Union, George Gamow found out that he had been appointed as a Soviet delegate. Gamow was enthusiastic. He had had eough of the country and had already attempted to flee to the west. Together with his wife *Rho*, he had attempted to paddle over the Black Sea from Crimea to Turkey where he intended to persuade the authorities to believe he was Danish and send him to Bohr. From the time he had spent with Bohr, he had obtained a Danish motorcycle licence as a form of ID. But the sea was far too rough and the couple were unsuccessful. He now had another chance, but this time a passport was also needed for Rho.

After having taken great pains to successfully obtain the necessary passports, the Gamow couple left the Soviet Union for good and travelled to Brussels and the Solvay Conference. They then continued to the USA where Gamow became Professor at the George Washington Uuniversity in Washington, D.C.

One of the matters discussed at the conference was Carl Anderson's discovery of the positron. Many physicists started to re-examine their cloud chamber photos, even the Joliot-Curie couple. The found to their annoyance that not only had they missed the neutron, they could also have discovered the positron.

At that time, the thought was that the proton might actually be a particle composed of a neutron and a positron. The Joliot-Curie couple reported to the conference that when they had bombarded aluminium or boron with α particles, they had firstly observed a neutron followed by a positron rather than the expected proton. This could indicate that the proton was a composite particle.

The other physicists were sceptical. Bohr and Pauli were certainly encouraging, but Lise Meitner was critical. Irène and Frédéric returned downheartedly to Paris to repeat their experiment. Frédéric then found beyond doubt that aluminium which had been exposed to α particles released both neutrons and positrons, but that the emission of positrons also continued after the preparation emitting α particles had been removed. The positrons thus did not come from the nuclear reaction – they had to come from the irradiated preparation exactly as though the latter had become radioactive!

The Joliot-Curie couple reflected on what had happened. One possibility was that the α particle that supplied the aluminium atom with two neutrons and two protons generated an unstable atomic nucleus which immediately lost a neutron. If this were the case, the final product ought to be phosphorus. If this phosphorus were radioactive and threw out a positron, it would in turn be transformed into silicon. The process could be written as follows:

$$\begin{aligned} {}^{27}_{13}\mathrm{Al} + {}^{4}_{2}\mathrm{He} &\rightarrow {}^{30}_{15}\mathrm{P}^{*} + {}^{1}_{0}\mathrm{n} \\ {}^{30}_{15}\mathrm{P}^{*} &\rightarrow {}^{30}_{14}\mathrm{Si} + {}^{0}_{1}\mathrm{e} \end{aligned} \qquad (18.11)$$

where the asterisk denotes that the nuclide is unstable and radioactive. Had the production of an artificial radioactive substance been successfully produced for the first time?

In his novel *The World Set Free*, H.G. Wells had predicted in 1914 that someone would succeed in producing artificial radioactive substances by 1932. His guess was only two years out.

It was not possible to prove that the hypotheses were correct by chemically demonstrating silicon (Si) since the quantities were incredibly small - less than 10^{-15} grammes as estimated by Frédéric. On the other hand, it was possible to chemically separate phosphorus and see whether the radioactivity followed the phosphorus fraction. The lifetime of the nuclide that was guessed to be phosphorus-30 was only around three minutes, however. So, it was a matter of performing the chemical analysis very quickly.

The chemist whom they asked for help had never been asked to work so quickly, but succeeded. The radioactive substance was phosphorus. In 1934, the achievement was put beyond all doubt. The now very ill Marie Curie had the pleasure of experiencing the discovery of her daughter and son-in-law. Frédéric described her reaction (according to Richard Rhodes):

> Marie Curie saw our research work and I will never forget the expression of intense joy which came over her when Irène and I showed her the first artificially radioactive element in a little glass tube. I can still see her taking in her fingers (which were already burnt with radium) this little tube containing the radioactive compound – as yet one in which the activity was very weak. To verify what we had told her she held it

> near a Geiger-Müller counter and she could hear the rate meter giving off a great many 'clicks'. This was doubtless the last great satisfaction of her life.

The Joliot-Curies reported their work in *Comptes Rendus* on 15 January 1934 and in a letter to *Nature* four days later. Rutherford wrote to them within a few weeks to congratulate them. He had done similar experiments himself without success, he wrote. Irène and Frédéric had finally obtained redress.

However, Marie Curie was dying of aplastic anaemia, a disease which means that the blood-forming stem cells in the bone marrow have been destroyed. She had exposed them to a great deal of radiation, firstly with her radium work and then with her x-ray work in the fields during the war. Her daughter Eve writes the following in the book about her mother:

> She was working with singular haste - and also with the singular imprudence which was usual with her. She had always scorned the precautions which she so severely imposed on her pupils: to manipulate tubes of radioactive bodies with pincers, never to touch unguarded tubes, to use leaden "bucklers" to ward off the harmful radiations. She barely consented to submit to the blood tests which were the rule at the Institute of Radium. Her blood content was abnormal. What of it? […] For thirty-five years Mme Curie had handled radium and breathed the emanation of radium. During the four years of the war she had been exposed to the even more dangerous radiation of the Röntgen apparatus. A slight deterioration in the blood, annoying and painful burns on the hands, which sometimes dried up and sometimes suppurated, were not, after all, such very severe punishments for the number of risks she had run!

On 4 July 1934, Marie Curie passed away at the Sancellemoz sanatorium in Saint-Gervais up in the foothills of Mont Blanc. The doctors had mover her there as a last desperate measure, thinking there was a risk that her declining strength would reawaken old tuberculosis. Her doctor wrote in her record:

> Mme Pierre Curie died at Sancellemoz on July 4th, 1934. The disease was an aplastic pernicious anaemia of rapid, feverish development. The bone marrow did not react, probably because it had been injured by a long accumulation of radiations.

The Joliot-Curie couple's discovery of the possibility of producing artificial radioactive substances was of substantial practical importance. Such substances proved to be capable of replacing radium within healthcare and medical treatment and would become extremely important in technical terms.

Someone who would do early research into the usage options was Bohr's good friend, George de Hevesy, who left Copenhagen in 1926 to become Professor in Freiburg but returned to Copenhagen in 1935 when it became far too dangerous for him to live in Germany. In 1913, de Hevesy, togetherx§ with *Friedrich Paneth* (1887-1958), had already shown that it was possible to use radioactive tracking substances to follow chemical reactions involving far smaller contents than had been possible using conventional methods. He had written the book called *Lehrbuch der Radioaktivität* with Paneth in 1923, so he was well prepared.

19.ATOMIC EXPLOSIONS BY MISTAKE

In the early 1930s, it had been established beyond doubt that unprecedented amounts of energy were bound in atomic nuclei, like the genie in the bottle. But how was the bottle to be opened? And how would the genie behave? In the old saga from *The Arabian Nights*, it showed no mercy to the poor fisherman who freed him from the bottle.

> 'I bring you good news, Mr Fisherman.'
> 'Oh, what news is that?'
> 'Well, you're about to die the most appalling death!'
> 'Bah, you should be ashamed of yourself, genie if you call that good news! Why do you want to kill me when I have just released you from the bottle and saved you from permanent captivity at the bottom of the sea?'
> 'Well, I will grant you one wish,' said the genie. 'You may choose for yourself the way in which you will die!'

But the fisherman did not want to die. He tricked the genie back into the bottle and then persuaded himself: 'A genie he may be, but I am a person and God has given me superior powers of comprehension; let me see if I cannot use cunning to prepare for his destruction and get the better of his evil treachery!'

His trick succeeded so well that the genie was repressed, and his services to the fisherman meant that the latter 'became the richest man of his time and his daughters lived as princess consorts until their death'. I do not wish to imply that the saga has any value in terms of evidence, but the moral expressed may be of some consolation for those who are superstitious.

When Irène Curie succeeded in producing a radioactive isotope of phosphorus in 1934, her innovative feat attracted a great deal of attention all over the world. She had created phosphorus-30 by bombarding aluminium with α particles. Among the many who were inspired by this discovery was the young Italian physicist Enrico Fermi, who had already defended his doctoral thesis at the University of Pisa who at the age of just 21 and then studied in Göttingen and Leiden. He had been given the first Italian Professorship in Theoretical Physics at the University of Rome in 1927.

Fermi realised that the recently-discovered neutron had to be a better projectile than α particles if you wanted to influence atomic nuclei. Since there was no braking electric charge, it would be easy to penetrate the nuclei. Theorist Fermi therefore decided to involve himself in experimental physics in the coming year.

In Rome in the 1930s, it was not easy to obtain resources for experiments in nuclear physics. The established experimental physicists, led by *Professor F. Rasetti*, were occupied with spectroscopy, which was of little help. Not even the most elementary tools, such as Geiger counters to measure the radiation, were available to borrow or to buy. It was therefore up to Fermi to make his own instruments. To crown it all, Rasetti, who was also a skilled instrument maker, was on a long holiday to Morocco and was therefore not available to give much-needed advice.

Nor were measurement instruments sufficient - Fermi also needed a neutron source. Here, he relied on 'God's providence', which was not a theological concept in this case but a being of flesh and blood in the form of *Professor Giulio Cesare Trabacchi* at the Board of Healthcare's physics laboratory. The latter was better equipped than the university's laboratories in some respects. Since Trabacchi had his affairs in order, the younger university physicists often appealed to him for loans and, as a mark of their gratitude, never called him anything other than 'God's providence'.

Trabacchi had a radon laboratory in the basement. There, he was able to collect in a glass ampoule the radon that emanated from a radium preparation. After a few hours and for a few weeks, the radon ampoule had largely the same radiation properties that the radium preparation would have had if the radon had remained inside it. Professor Trabacchi's radium preparation weighed 1 gramme, which meant that the radon ampoules were very strong sources of radiation.

When Chadwick discovered the neutron, he had irradiated beryllium with α particles from polonium. It was therefore natural for Fermi to scrounge radon from Trabacchi and mix the daughter products that were radiating α particles with beryllium powder. In doing so, he created a neutron source.

Fermi then began to systematically bombard element after element with neutrons. He started with the lightest, i.e., hydrogen, and continued with lithium, beryllium, boron, carbon and nitrogen, but failed to obtain results that were of any interest. He had almost given up when he tried with fluorine and succeeded in creating a new radioactive substance. He was now starting to reap the rewards of his troubles; the heavier elements were finally producing results. Fermi sent Rasetti a telegraph to Morocco and asked him to come home since the combined strength of everyone was now needed for the experiments to continue.

Every week, the radon apparatuses in Professor Trabacchi's basement were milked of the newly-formed radon, which was transferred to a glass tube containing beryllium powder. Fermi had the assistance of a young colleague, *Emilio Segré* (1905-1989), Professor in Palermo at the time. Segré's father was a businessman and Emilio had inherited some of his father's business sense. He had an excellent flair for finding stocks of elements that had not yet been examined and was able to return with valuable finds without incurring hefty expenses for the laboratory. Some metals such as caesium and rubidium were given free of charge, since they were so rare that the tradesman had given up hope of anyone ever asking for them. But the tradesman was not completely lacking in Latin skills, and handed over the dusty stocks showing the words *Rubidium caesiumque tibi donabo gratis et amore dei.*

In spring 1934, Fermi and his assistant had already managed to go through all of the available elements and were able to conclude the experiments by irradiating the heaviest known element, uranium, with neutrons.

When an element is bombarded with neutrons, any nuclear reactions can be expected to transform some of the atoms into either an isotope of the same element (by supplying a neutron but not changing the nuclear charge) or another element that is close to the original as regards atomic number (if the isotope formed is unstable and rapidly disintegrates).

When Fermi had irradiated an element with neutrons (e.g., iron), the sample was dissolved in an acid and small quantities of related substances were added (e.g., in the case of iron: chrome, manganese and cobalt). These elements were then separated from one another using conventional chemical methods and examined to see whether any of the fractions were radioactive. If the irradiation of iron had given rise to radioactive manganese, which did actually happen, the newly-formed radioactive manganese atoms accompanied the added manganese. Following the separation, it was possible to prove that the manganese, as opposed to the chrome and the cobalt, retained the radioactive substance, which was therefore presumably manganese.

When uranium had finally been irradiated, it was again found that the radioactive substances had formed and that more than one element was involved. There was an attractive possibility that a new element had now been created for the first time, one that was heavier than uranium, with the mass number 93. The following nuclear reaction was conceivable:

$$ {}^{238}_{92}U + {}^{1}_{0}n \rightarrow {}^{239}_{92}U^{\ unstable} \rightarrow {}^{239}_{93}X + {}^{0}_{-1}e \qquad (19.1)$$

Several years later, in 1940, this alleged element 'X' with atomic number 93 would actually be discovered and produced by Americans *Edwin McMillan* (1907-1991) and *Philip Abelson* (it is called neptunium), but in 1934 the element was purely hypothetical and referred to only as 'element 93'.

Did the activity that Fermi was able to demonstrate originate from element 93? He was not sure. The uranium became more radioactive than he had expected following irradiation. Fermi and his colleagues sent an initial report to the *Ricerca Scientifica* journal in May 1934. They did not claim to have found element 93, but gave an account of the signs indicating that this could be what it was.

Unfortunately, the discovery was mentioned in a ceremonial speech at an academic meeting on 4 June in the presence of King Victor Emmanuel and news reporters were therefore present in their droves. The Italian press hyped up the event and splashed big headings all over the papers proclaiming that the discovery was proof of ‘the triumphs of fascist culture’.

Fermi, who was far from certain about his claim, became distracted and saw the headlines as a threat to his scientific credibility. Many researchers tried to advise him. German chemist *Ida Noddack* (1896-1978), who was working at the University of Freiburg alongside her husband *Walter*, wrote to Otto Hahn saying that ‘when heavy nuclides are bombarded with neutrons, the nuclide in question may break down into larger fragments that would definitely be isotopes of known elements’. Ida Noddack seemed to be the only person who understood that Fermi may well have executed the world’s first nuclear fission. Hahn, who was afraid that Noddack would destroy his good reputation as a researcher by making excessively outlandish statements, refused to allow her statement to be published. Hahn thought the idea that the uranium nucleus could be split into large fragments was completely absurd.

However, Fermi did not overlook the opportunity. He made theoretical calculations. He estimated the energy that could be extracted from the mass defect according to Einstein’s law, and realised that it was insufficient to overcome the electric forces that could be expected to hold together fractions of the nucleus. Nuclear fission was theoretically impossible, he thought.

Unfortunately, the information about the mass of the atomic nuclei to which Fermi had access was not reliable. There was nothing at all wrong with his calculations, and, had he had more reliable values for the mass deficit, he would have found that nuclear fission was actually possible in his experiment. The unexpectedly high activity that he had found in the uranium came about because he, as Ida Noddack had thought, had split the uranium atom more or less in half. If, rather than stubbornly searching for the new elements that were heavier than uranium, he had performed chemical analyses for the lighter substances such as barium, which can be an expected product of nuclear fission, he would have discovered the phenomenon of fission as early as 1934. Could this have given the Axis Powers a lead towards the atomic bomb?

Fermi had taken great pains to systematically research the atomic nucleus and finally to discover element 93. It was disappointing that he stumbled on a much more important discovery yet failed to see it.

The hard work was not just scientific; it was often quite literally physical. The researchers in Rome were obliged to do almost the equivalent of sporting achievements when it came to rushing through the laboratory’s corridors in the shortest possible time from the irradiation room to the chemistry lab and the measurement room, carrying samples containing radioactive substances that rapidly disappeared by disintegrating. Fermi’s wife *Laura* mentions in her book about Enrico the way this rapid decay could surprise visitors. A respectable Spanish visitor on the ground floor asked for ‘His Excellency Fermi’ and received the response ‘The Pope is one floor up!’ The Spaniard, who did not know that ‘the Pope’ was nickname that Fermi’s colleagues had for him, climbed the stairs perplexed and was almost immediately run into by a man in a dirty laboratory gown. A new enquiry after ‘His Excellency Fermi’s’ work room led to the visitor being dragged into the corridor race and, to his dismay, seeing that it was actually the harassed laboratory worker who was ‘His Excellency’. The Spaniard had to make do with a confused conversation in front of a measurement apparatus with Fermi eagerly noting down numbers.

The experiments where nuclear reactions were generated through irradiation continued. The Joliot-Curies were obviously very energetic following their initial achievement in producing a new radioactive substance. The researchers at the Kaiser-Wilhelm Institute for Chemistry in Berlin-Dahlem were also eager. In 1934, Otto Hahn and Lise Meitner, who were almost the same age, had a new colleague 23 years their junior in the shape of chemist *Fritz Strassmann* (1902-1980). Hahn, Meitner, and Strassman started collaborating on research to produce the elements that could conceivably be heavier than uranium ('transuraniums') and for which Fermi had searched so eagerly. The group worked industriously over the space of four years until 1938 and believed they had found a number of new heavy elements. They designated them with the prefix 'eka' (from Sanskrit meaning 'the first') followed by the name of the previously-known element that lay immediately above them in the periodic system. Therefore, when they thought they had found eka-rhenium and eka-osmium, these were elements with atomic numbers 93 and 94, i.e., the elements that we now call neptunium and plutonium.

These discoveries would have verified Fermi's dubious discovery of 'element 93' had it not been for a few strange and inexplicable contradictions in the explanation as to how the new elements could have arisen. In spite of everything, neptunium and plutonium had still not definitely been confirmed.

In 1938, competing information came from Paris. The Joliot-Curie couple reported that, after having irradiated uranium with neutrons, they had found a radioactive substance with a half-life of three and a half hours. They believed that it was an isotope of thorium. This could be explained only if the uranium atom had become unstable and disintegrated while emitting an α particle after having been hit by a neutron.

The finding irritated Lise Meitner. She had already in 1934 thought of the possibility that thorium could be formed in such a reaction, but, following repeated experiments, Strassmann had assured her that the irradiated uranium contained no thorium. Had Strassmann made a mistake? At Meitner's request, he repeated the experiments and was able to provide clear information. The French researchers must have been mistaken.

Rather than publishing these results and thereby putting the French research group to shame, Meitner and the chivalrous Hahn wrote a letter to Paris, warning them against continuing to claim to have found thorium. Irène Curie then withdrew her hypothesis and described the new 3.5-hour substance with the following words: 'The analysis results say that the 3.5-hour substance has the same properties as lanthanum, from which it currently seems to be impossible to separate it using any means other than fractionation'.

The idea that the new substance could actually be lanthanum was so unreasonable that it was not even discussed. Such a light atomic nucleus as lanthanum, only half the weight of uranium, could not possibly arise through radioactive decay following neutron bombardment. The new substance must be a transuranium element with the same chemical properties as lanthanum, they thought, and thereby fell into the same trap as Fermi had earlier in his endeavour to find 'element 93'. Ida Noddack, who had pointed out the truth, had been firmly dismissed as allowing her imagination to get the better of her.

The researchers in Europe were now starting to be affected by Hitler's onslaught. In March 1938, the Germans crossed the border into Austria and on 16 April the 'Anschluss' to the German Reich was adopted through 99.75% of the stated votes. This meant that Lise Meitner's Austrian passport ceased to offer any protection and, as a Jew, she ran a very great risk of remaining in the Nazi empire. She therefore fled to Stockholm in July 1938 where she was first met by Manne Siegbahn at the Nobel Institute for Physics. This is how the Kaiser-Wilhelm Institute for Chemistry lost its leading physicist.

When Hahn and Strassmann got to see Irène de Joliot-Curie's report on a transuranium element with the same properties as lanthanum, they quickly performed a series of experiments. They then found, following the addition of barium to the acid solution of the neutron-irradiated uranium, that at least three new radioactive substances with the same properties as barium could precipitate out. They were also able to show that these substances decayed to daughter products that precipitated together with lanthanum. So, the Paris researchers were right in that the precipitate would contain substances similar to lanthanum. It was possible that the barium-like substances were isotopes of radium and the lanthanum-like substances isotopes of actinium. It occurred to neither Hahn nor Strassmann that the substances were quite simply isotopes of barium and lanthanum. Like Irène Joliot-Curie, they thought that this was far too unreasonable.

However, the idea that the uranium could disintegrate into radium and actinium was also very much of a long shot for the physicists who re-read Hahn's and Strassmann's results in autumn 1938. On a visit to Copenhagen, Hahn discussed their theory with Niels Bohr, who immediately said that the idea was unreasonable and that the new substances must be transuraniums. Lise Meitner stated in a letter that her former colleagues might be proficient chemists, but that their physical speculations were nonsense.

In summer 1938, Emilio Segré came to New York with the intention of continuing on to California to spend a few months at Berkeley as a guest researcher. When he read about Mussolini's anti-Semitic campaign, he decided to remain in the USA. Fermi had spent the summer of 1935 at the University of Michigan in Ann Arbor and had been impressed by the technical resources in the USA and the freer political climate. He had nothing against staying, but his wife Laura did not want to leave Italy. The next year, Fermi lectured at a summer school at the University of Columbia in New York. That summer began with the Spanish Civil War, where Franco would be supported by both Hitler and Mussolini. At home in Rome, fascist researchers started to gain more and more power over the research. The Fermi family began to worry but nonetheless Fermi returned home.

The situation in Italy became even more dangerous, however, and the anti-Semitic manifesto of which Segré had read came in 1938. Italians are Aryans, was the proclamation, but Jews 'do not belong to the Aryan race'. Fermi's wife was Jewish. Fermi started secretly keeping a lookout for any possibility of being employed at an American university, and succeeded in obtaining the promise of a Professorship at the University of Columbia.

On a visit to Denmark to meet with Bohr, Fermi heard that he was nominated for the Nobel Prize for Physics, and Bohr said that Nobel Prize winners were expected to come to Stockholm for the prize ceremony along with their family, and the very opportunity needed to flee from Italy had presented itself.

The political situation deteriorated rapidly. On 7 November 1938, German diplomat *Ernst vom Rath* was murdered in Paris by a Jewish student who wanted to avenge wrongs against his parents in Poland. The event was taken as a pretext for one of the acts of vengeance against Jews in Germany staged by the Minister of Propaganda Goebbels on the night between the 9th and 10th November, 'Kristallnacht'. A good forty people were killed and tens of thousands were captured to be sent to concentration camps.

On the morning of 10 November, Fermis heard about the response in Italy on the radio news. Jewish teachers were dismissed and Jewish children could no longer go to normal schools. From then on, Jewish doctors and lawyers could assist Jews and no-one else. 'Aryans' could no longer work for Jews. Jews were deprived of their civil rights and their passports would be withdrawn. It was then that Fermi received the official information from Stockholm that he had won the 1938 Nobel Prize for Physics for the demonstration 'of new radioactive elements, produced through neutron irradiation and, in association with this, his discovery of nuclear reactions created through slow neutrons'. So, the Fermi family left Italy at the last minute to travel to Stockholm and the Nobel ceremonies. They left behind all their possessions; they had not dared to sell anything and thus arouse suspicions. The Nobel Prize would become their travel fund to New York and freedom.

At the Kaiser-Wilhelm Institute for Chemistry in Dahlem, Hahn and Strassmann attempted once more to identify the mysterious 3.5-hour substance. Following the precipitation of barium, they now chemically separated radium to demonstrate that the elusive substance was a radium isotope. To his surprise, they found no activity in the fraction that ought to have contained all of the radium. They repeated the separation after first having added a known quantity of radium (^{224}Ra). The method proved to be immaculate; the added radium was found in the right fraction.

The physicists were obviously right – the 3.5-hour substance was not a radium isotope. Hahn and Strassmann did more and more complicated analyses. In the end they were convinced. However hard they had tried to separate the new substance from barium and its daughter product from lanthanum, it had proven to be impossible. Only one conclusion was possible, however unreasonable it might seem: the mysterious substances must quite simply be barium and lanthanum.

On 19 December at eleven o'clock in the evening, Otto Hahn finally wrote a letter to his old colleague Lise Meitner. What could be the best explanation for the fact that the alleged 'radium' isotope appeared to be barium? 'The fact is, there is something so strange about the "radium isotopes" that for the time being we are mentioning it only to you [...] Our radium isotopes act like *barium*'. He continued to cajole:

> Perhaps you can suggest some fantastic explanation. We understand that [uranium] really *can't* break up into barium. […] So try to think of some other possibility. Barium isotopes with much higher atomic weights than 137? If you can think of anything that might be publishable, then the three of us would be together in this work after all. We don't believe this is foolishness or that contaminations are playing tricks on us.[12]

The letter had already reached Lise Meitner in Stockholm by Wednesday 21 December. Meitner, who felt lonely and isolated in Sweden, was preparing to travel to Kungälv to celebrate Christmas with a female friend, *Eva von Bahr-Bergius*, together with her nephew Otto Frisch. Frisch, who was also a nuclear physicist, would come to visit from Niels Bohr's laboratory in Copenhagen where he was working temporarily. Lise Meitner responded immediately to Hahn's letter:

> Your radium results are very amazing. A process that works with slow neutrons and leads to barium! […] To me for the time being the hypothesis of such an extensive burst seems very difficult to accept, but we have experienced so many surprises in nuclear physics that one cannot say without hesitation about anything: 'It's impossible.'

In her letter to Hahn, Meitner also said that she was on the way to Kungälv and asked him to address his next letter there. She would be travelling there on Friday 23 December, the day before Christmas Eve. On the Tuesday, the day after Hahn had written to Lise Meitner, Hahn and Strassmann attempted to write a report of their observations, and they had asked *Paul Rosbaud*, editor at Springer Verlag, to prepare a place in *Naturwissenschaften*. Rosbaud realised that it concerned something important and promised to remove a less urgent paper. But he must have the manuscript no later than Friday.

Hahn and Strassmann continued their analyses and were able to show that the substance that behaved like lanthanum was a daughter product of the disintegration of the substance that behaved like barium. However incredible it might seem, barium must have been formed following the irradiation of uranium with neutrons. But the chemists were worried about claiming something which nuclear physicists could show to be impossible. They hoped to receive a new letter from Meitner which would add something to their paper by means of a physical explanation, but no letter arrived. Lise Meitner was saying nothing until she had discussed the problem with her nephew.

Hahn wrote to Lise Meitner again on Wednesday 21 December when the lanthanum results were available, but by then he had not received her letter asking him to address it to Kungälv. He therefore addressed the letter to Stockholm.

> We cannot hush up the results, even though they may be absurd in physical terms. You can see that you will be performing a good deed if you find an alternative [explanation]. When we finish tomorrow or the day after, I will send you a copy of the manuscript.

The day before Christmas Eve in 1938, Meitner travelled to Kungälv at the same time as the respite period for quick publication was due to run out, but Meitner did not know this because she had not yet received Hahn's second letter. In the meantime, Hahn and Strassmann had written a joint report. However, they had had problems with the wording. Should they continue to talk about 'radium', or should they change 'radium' to 'barium'? They got around the matter by instead talking about '-alkaline earth metals' - a joker which could mean absolutely any of the substances. They expressed themselves very carefully since they had received no advice from Meitner:

[12] Quoted by Richard Rhodes, "The Making of the Atomic Bomb, 1986, as are most of the later quotes in this chapter.

> We come to the conclusion that our ‘radium isotopes’ have the properties of barium. As chemists, we should actually state that the new products are not radium, but rather barium itself. Other elements besides radium or barium are out of the question.

They continued, with equal caution:

> As chemists, we really ought to revise the decay scheme given above and insert the symbols Ba, La, and Ce in place of Ra, Ac, Th. However as ‘nuclear chemists’ working very close to the field of physics, we cannot bring ourselves yet to take such a drastic step which goes against all previous laws of nuclear physics*. There could perhaps be a series of unusual coincidences which has given us false indications.

Hahn posted the manuscript on Thursday 22 December and sent a copy to Lise Meitner at the same time, this time also to the Stockholm address. But Meitner arrived in Kungälv on the Friday and moved into a guest house on Västra gatan. Her nephew Otto Frisch came late in the evening by train from Copenhagen, concerned that his father was in the Dachau concentration camp following Kristallnacht. Otto had a pair of skis with him and was resolutely determined to use them.

As soon as they had the chance to talk over breakfast the following day, Christmas Eve, Meitner was eager to discuss the observations of the German chemists and their meaning. Frisch, on the other hand, was equally eager to tell the story of a large electromagnet that he planned to build for his experiments in Copenhagen. His aunt then showed him Hahn’s letter of 19 December.

‘Barium,’ said Frisch, ‘I don’t believe it. There must be some mistake.’

But Lise Meitner insisted. ‘If Hahn, with all his experience as a radiochemist, says it is so, there must be something in it.’

She eventually persuaded her nephew to take an interest and they began to discuss the problem. Nuclear fission? At first it seemed impossible. There would be insufficient energy. Otto Frisch later described their first reaction:

> But how could barium be formed from uranium? No larger fragments than protons or helium nuclei (alpha particles) had ever been chipped away from nuclei, and the thought that a large number of them should be chipped off at once could be dismissed; not enough energy was available to do that. Nor was it possible that the uranium nucleus could have been cleaved right across. Indeed a nucleus was not like a brittle solid that could be cleaved or broken; Bohr had stressed that a nucleus was much more like a liquid drop.

The latter gave them an idea. They made a note of ‘the liquid drop model’ for the atomic nucleus as proposed by Bohr in 1937 on the basis of Bethe-Weizsäcker binding energy calculations, and the observation that the correction terms for the superficial nucleons led them to think of the surface tension of a drop of liquid while another term corrected the repulsive effect of the coulomb forces.

They argued that if energy were supplied to an atomic nucleus and the energy distributed itself between the nucleons and led to disturbances, it was conceivable that the nucleons and the whole nucleus would vibrate so that the nucleus would be stretched at some stage, as happens to a drop of liquid. The ‘strong force’ that held the nucleons together appeared to work only over a very short distance. The depletion of the nucleus therefore led to the mutual repulsion of the proton being given temporary precedence by means of the electric coulomb forces The ‘surface tension’ that arose through the uneven influence on the most superficial of the nucleons would return ‘the drop’ to a spherical shape, but if the drop were far too depleted, this would mean that the outermost halves would each be cut off in approximately the same way as a drop of liquid splitting into two.

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* The last five words of this sentence were changed by Hahn in his corrections to ‘all previous experience’.

The liquid drop model, although very primitive for the nuclear physicist of today, made Meitner and Frisch accept that nuclear fission was possible. Frisch has said: 'I remember at that particular time immediately thinking that the electric charges reduced the surface tension.' He has also said in his memoirs:

> And so I promptly started to work out by how much the surface tension of a nucleus would be reduced. I don't know where we got all our numbers from, but I think I must have had a certain feeling for the binding energies and could make an estimate of the surface tension. Of course we knew the charge and the size reasonably well. And so, as an order of magnitude, the result was that at a charge [i.e., an atomic number] of approximately 100 the surface tension of the nucleus disappears; and therefore uranium at 92 must be pretty close to that instability.

In other words, what Frisch meant was that the electric coulomb forces that would repulse the equally charged protons from one another would get the better of the uniting forces if there were more than 100 protons in the nucleus, i.e., at an atomic number greater than 100.

There are many accounts of the way in which Lise Meitner and her nephew came to be convinced that Hahn and Strassmann had produced nuclear fission. Otto Frisch wanted to use the skis he had with him and Lise Meitner, who liked taking long walks, followed him on foot. They sat on the trunks of felled trees and drew and calculated. Using what they knew about the magnitude of the mass deficit, they were also able to estimate the energy that would be released at the time of such nuclear fission, and arrived at approximately 200 million electron-volts. It does not seem as though they initially saw the information about this enormous amount of energy (not in absolute measurements but relative to a single nuclear fission) as anything other than a piece of the puzzle in a physical problem. They probably did not realise the possibility of a chain reaction and the extraction of nuclear energy on a large scale at the time.

Neither Hahn's letter of 21 December containing the information mentioning the find of what looked like lanthanum as a daughter product of barium, nor the copy of the manuscript that he had posted to Stockholm on 22 December, had reached Kungälv before the Christmas weekend, although Hahn had now received Lise Meitner's first letter saying that she would be celebrating Christmas there. He was very anxious to obtain her views and support. So, on Wednesday 28 December he wrote again, this time to the correct address. He cajoled: 'I am of course very interested in hearing your honest opinion. Maybe you can work something out and publish it.' The letter arrived on the Thursday, again with impressive speed, and Meitner answered on the same day that she thought the barium find was 'very exciting. Otto R. and I have already cogitated over it', but she gave Hahn no explanation.

A few days later, the forwarded post arrived with the earlier letters and the manuscript from Stockholm and, on 1 January 1939, Meitner sent Hahn New Year's greetings. She expressed her view of nuclear fission very carefully: 'We have read your work very thoroughly and consider it *perhaps* possible energetically after all that such a heavy nucleus bursts'. She continued to lament that the earlier misunderstanding was not exactly a good recommendation for her new start in exile. Frisch added: 'If your new findings are really true, it would certainly be of the greatest interest, and I am very curious about further results'.

The failure of the two physicists to give Hahn the clear information he was looking for, of which they must have been well aware, is not easy to understand. Was the response so incredible that they did not dare to believe in their conclusions, or did they want to be unique in their conclusions? Did they want Bohr's support before daring to express themselves definitively?

On New Year's Day in 1939, Frisch and Meitner left Kungälv, one going to Copenhagen and the other to Stockholm. On 3 January, Frisch met Professor Bohr and began to tell him the story. Before he had finished, Bohr struck his forehead, shouting: 'Oh, what idiots we've all been! Ah, but this is wonderful! We could've predicted the whole lot! This is just as it must be!'

Back in Stockholm, Lise Meitner found Hahn's adjusted corrections. Her uncertainty had now evaporated, and she wrote to Hahn: 'I am fairly *certain* now that you really have a splitting towards barium, and I consider it a wonderful result for which I congratulate you and Strassmann very warmly.

[...] You now have a wide, beautiful field of work ahead of you. And believe me, even though I stand here very empty-handed at the moment, I am still happy about the marvelousness of these findings'.

Frisch discussed a joint report with his aunt in a telephone conversation with Stockholm. He also wanted to discuss the report with Bohr but there was not enough time because Bohr was about to travel to the USA for a three-month visit, which included visiting Einstein at Princeton. Frisch barely managed to write out a few pages to rush to Hovedbanegaarden where Bohr was going to take a train to Gothenburg and thereafter a ship for America. They decided that Frisch should immediately send the article to *Nature*, and Bohr promised not to say anything to his American colleagues until it had been printed.

The report from Hahn and Strassmann was published in *Naturwissenschaften* on 6 January 1939 and it was in Frisch's hands the following day. He then discussed the whole problem with *George Placzek*, a sharp-tongued Czech theoretical physicist who worked with Bohr. Placzek was sceptical, realising that Frisch needed more proof - could he not get himself some cloud chamber pictures of the energy-rich fission products? Frisch did not believe that a cloud chamber was suitable for that purpose. A measurement of the current pulses in an ionisation chamber would be easier. The heavy fission products with mass numbers between 80 and 150 could not be expected to have a range of more than a few millimetres in air, but they would generate very powerful ionisation. Frisch reckoned that they would split off approximately 3 million electrons from the air molecules and thereby cause very strong pulses.

Frisch set up an ion chamber consisting of two metal plates separated from one another by an insulating glass ring of 1 cm in height. One plate was charged and connected to an amplifier and an oscilloscope so that the current pulses could be observed on a monitor. He then placed a neutron source and uranium in the ion chamber and began his experiment in the afternoon of Friday 13 January. He continued throughout the night and found what he wanted to find: sufficiently powerful current pulses.

Tired and happy, the following day he stated that Friday the 13th was a lucky day in his case. He was even happier later in the day when he received a telegram stating that his father had been released from the concentration camp and that both of his parents were on the way to Stockholm to share an apartment with his aunt.

Working with de Hevesy was a young American biologist, *William Arnold*, who followed Frisch's experiment with interest when he later proudly repeated it to all those who were interested. Arnold has described how the word 'fission' came to be used for nuclear fission:

> Later that day Frisch looked me up and said: 'You work in a microbiology lab. What do you call the process in which one bacterium divides into two?' And I answered, 'binary fission'. He wanted to know if you could call it "fission" alone, and I said you could.

The word 'fission' was used to designate nuclear fission for the first time in the two papers that Frisch posted to *Nature* on the evening of Monday 16 January 1939. The first paper, which had Aunt Lise down as co-author, was entitled 'Disintegration of uranium by neutrons: a new type of nuclear reaction'. The second paper described his own physical proof of nuclear fission.

Bohr travelled to the USA with Swedish American Line's *Drottningholm* and was accompanied by his son Erik and colleague *Léon Rosenfeld* (1904-1974). Sea sickness aside, they had lively discussions about the meaning of the unexpected discovery in Berlin and the physical explanation given by Lise Meitner and Otto Frisch. *Drottningholm* arrived in New York on 16 January, the same day that Frisch had posted the two papers. The Fermi couple, who had recently come to the USA from the Nobel ceremonies in Stockholm, were waiting on the quay at Street 57, along with *John Wheeler*, a young physicist who worked with Bohr during the 1934 to 1935 winter period and was now able to work with him in Princeton.* Bohr stayed overnight in New York and left with the Fermis while Rosenfeld

* Princeton is in New Jersey, half way between New York and Philadelphia, the distance from Manhattan being about 75 km.

accompanied Wheeler to Princeton. Bohr had not thought of asking Rosenfeld to promise to keep things quiet, and Rosenfeld was burning with eagerness to tell the story of the fantastic discovery. Wheeler was therefore given a full report on the train to Princeton.

In Princeton, a meeting of the 'journal club' was arranged regularly every Monday evening, a meeting at which physicists discussed new announcements and reports among themselves. Wheeler was in charge of this 'club' at the time, which was always well attended. Wheeler asked Rosenfeld to tell Hahn, Strassmann, Frisch and Meitner. So many people attended that the late arrivals had to remain standing.

This was the first time that the other side of the Atlantic got to hear the news of 'uranium splitting' (the word 'fission' had not been adopted by Frisch until after Bohr and Rosenfeld had left Europe). Rosenfeld's memory of the occasion was that 'the effect of my talk on the American physicists was more spectacular than the nuclear fission phenomenon itself. They rushed about spreading the news in all directions'.

However, Bohr had kept his promise and had told Fermi nothing (he did not get to hear of the innovation until *Isidor Isaac Rabi* (1898-1988), Professor of Physics at the University of Columbia in New York, returned there from Princeton. Only now did Fermi begin to understand what he had actually observed when he thought he had observed 'element 93' in Rome in 1934.

When Bohr arrived in Princeton on the Tuesday, he was met by a great commotion; the news that he had kept to himself had become publicly known. Bohr became very ill at ease. That same evening, he wrote home to his wife in Copenhagen: 'I was immediately frightened as I had promised Frisch I would wait until Hahn's note appeared and his own was sent off'.

On the ship journey, Bohr and Rosenfeld had prepared voluminous notes. Bohr now spent three days writing a letter to *Nature* based on that material, a letter in which he would clearly credit Meitner and Frisch for the physical explanation. He realised this was something they would need in order to be able to re-establish themselves in exile.

On 25 January, Bohr travelled to Washington DC to participate in a conference the following day. The conference was the fifth in a series initiated by George Gamow after he and his wife Rho had arrived from Belgium in 1933. For the 1939 Washington conference he was assisted by Edward Teller, the dynamic, Hungarian-born physicist who would play a major role in the further developments.

As soon as Bohr had arrived in Washington, he rang Gamow and told him the exciting story about nuclear fission. Gamow in turn called Teller and said: 'Bohr has just arrived in town. He's out of his mind. He's saying that a neutron can split uranium.'

The conference was opened the following day, 26 January 1939. A photograph of 51 participants shows known physicists like Bethe, Bohr, Fermi, Gamow, Rabi and Teller. Bohr now felt released from his promise: the report from Hahn and Strassmann had been published and Bohr now knew that the articles from Frisch and Meitner were also on the way. The conference was opened by Gamow, who introduced Bohr. One of the participants has said: 'Bohr proceeded to reveal his news concerning Hahn's and Strassmann's experiment. [...] He also told of Meitner's interpretation that the uranium had split. As usual he mumbled and rambled so there was little in his talk beyond the bare facts. Fermi then took over and gave his usual elegant presentation, including all the implications.'

The innovation was definitely out of the bag.

In 1939, the newspapers wrote freely about the discovery made by Hahn and Strassmann. It was soon appreciated that this constituted the first step towards the extraction of 'atomic energy' and maybe also towards 'atomic weapons'. There was no talk of 'nuclear power' or 'nuclear weapons' as yet, and the word 'atom' was used in slightly misleading ways, even though normal chemical reactions could also be said to transform atomic energy (but not nuclear energy).

On 6 February 1939, *Time* magazine wrote:

> **Great accident:** Some weeks ago Dr. Otto Hahn of Berlin's Kaiser Wilhelm Institute donned his work clothes, walked into his laboratory to perform a physical experiment. With a stream of neutrons (obtainable by subjecting a pinch of beryllium to the emanations of the radioactive gas radon) he bombarded a bit of uranium. While the routine little experiment proceeded all was peace and quiet in the laboratory. [---]

But when Dr. Hahn examined his end-products and sat down with pencil and paper to figure out what had happened, he concluded that he had created the most violent atomic explosion ever effected by human agency. Moreover, he had not intended to do it. It was a great accident. [---]

There was no audible or visible violence, for the reason that the total quantity of energy released was not enough to knock a fly off the wall. [---] Though the discovery does not raise an immediate prospect of driving ocean liners for thousands of miles with the atomic energy locked in a cupful of water, it does help justify the statement of popularizing physicists that such things would be possible if atomic energy could be efficiently released.

On 13 March 1939, *Time* wrote that the news of Dr. Otto Hahn's unexpected atomic explosion streaked over the physical world like a meteor, because with it came the news that in the products of bombardment was found something which seemed to be atoms of barium:

This barium was the clue to something terrific. For the huge uranium atom, heaviest of the 92 standard elements, weighs 238 units. The barium atom weighs 137 units. Since the barium could have originated only as a fragment of the big uranium atom, it was logical to suppose that the latter had cracked asunder, in two nearly equal parts. Heavy atoms had been 'chipped' before [---] but this was the first time they had been cracked in two.

This discovery has already brought into play new words [---]. The splitting of the uranium nucleus is described as a 'fission', which, in biology, means division of an organism into two or more parts. [---] Last week the 'fission' of the uranium atom definitely looked like a find of Nobel Prize calibre. But present German law forbids Germans to accept Nobel Prizes. Meanwhile, physicists have unofficially distributed some of the credit to Lise Meitner in Stockholm (a woman physicist) and R. Frisch of Copenhagen [---]. Some credit also went to Nobel Laureate Irene Curie-Joliot (daughter of Marie Curie) and P. Savitch of Paris, who had done work which helped Hahn identify the all-important barium in his bombardment products.

It may also be interesting to see what Swedish newspapers wrote in early 1939. On 24 February, *Folkets Dagblad* wrote:

EPOCHAL ATOMIC EXPLOSION IN COPENHAGEN
Radium irradiation transforms uranium into barium
200 million volts of energy measured at time of split

According to *Berlingske Tidende*, Austrian researcher Dr. Robert Frisch at the university's Institute for Theoretical Physics, who was invited to work at the Institute by Professor Niels Bohr, has confirmed the epochal discovery by German scientists Hahn and Strassmann through new experiments showing that the irradiation of uranium results in the appearance of barium.

The results of the German scientists were communicated to Dr. Frisch through Mrs. *[sic]* Professor Meitner in Stockholm, with whom he was in constant cooperation until as late as the start of last month, and he immediately started to investigate the physical aspect of the matter. In doing so, Dr. Frisch used the 600 milligrammes of radium that Bohr was given as a gift on his 50th birthday, and ascertained not only the unusual transformation of uranium into barium but also that the splitting of the atomic nucleus led to the development of an energy that was measured to be no less than 200 million volts, the greatest energy ever measured on Earth and that has been surpassed only by the cosmic forces.*

* The author of the article forgets to say that it is a question of a very small amount of energy that is big only in relative quantities such as kinetic energy for atoms, and that the electron-volt rather than the volt is the correct energy measurement.

> The discovery has been the subject of lively discussions in America over the past few days where Professor Bohr is currently residing and, according to a telegram from over there, the Professor has apparently said that he ought actually to have predicted the discovery himself since it was in some way a natural consequence of his nuclear theory.

On 17 February, the known oceanographer and physicist Professor *Hans Pettersson* (1888-1966) in Gothenburg had already drawn the following conclusions in an article with the heading 'Transformation of the Uranium Nucleus' in *Göteborgs Handelstidning*:

> It is easy to understand why Hahn and Strassmann are taking the greatest of care in portraying these unexpected results. The fact that they were confident enough to publish them is also reason enough to believe that they are correct. For even though great credit is given for this type of conquest in nuclear research, mistakes, however obvious they may appear to have been, will be judged very harshly.

And so, how would that credit be shared? The war interrupted the proceedings and no Nobel Prizes were awarded for the three years of 1940-1942. In 1943, the Nobel Prize for Chemistry was awarded to George de Hevesy for his trace element methods with radioactive isotopes of studied elements. It was not until after the fall of Germany that a German could be awarded the Nobel Prize. In 1945, Otto Hahn won the Nobel Prize for Chemistry, reserved from 1944 for the discovery of nuclear fission. No equivalent Nobel Prize was awarded in physics, which rather grieved Lise Meitner who had chosen to share the Nobel Prize with Hahn. But nor did Strassmann get to share the prize with Hahn - after all, he was a chemist. Maybe Meitner and Frisch ought to have shared a Nobel Prize for Physics, but this never happened. The discovery had 'been in the offing' and many others claimed they had participated.

On 1 September 1939, the German army crossed the border into Poland and the Second World War had started. In the USA, exiled physicists Szilard and Wigner had by then already begun to contact Albert Einstein because they were concerned that the Nazi German government would arrive upon the idea of using the nuclear energy for weapons of mass destruction. This concern was not unfounded. In Germany, two young physicists in Hamburg, *Paul Harteck* and *Wilhelm Groth*, had already written to the Ministry of War on 24 April 1939 to bring this possibility to their attention. At the same time, well-known Professor of Physics *Georg Joos*, with no knowledge of his colleagues' initiative, had written to the National Ministry for Education and pointed out the consequences of an article in *Nature* magazine on 22 April. In that article, Joliot-Curie's colleagues *Hans von Halban* and *Lew Kowarski* had drawn attention to the possibility of a chain reaction in the event that nuclear fission were to prove capable of generating a surplus of neutrons that could cause the splitting of additional atoms. And the seal of Pandora's box was thus broken.

REFERENCES AND BIBLIOGRAPHY

The Table below lists the major sources used in each chapter. Details of references are provided after the table. Much information is available in well-known encyclopaedias but searching can be cumbersome. Some reference books were particularly useful, viz.: Berg 1992, Cauf 1990, Grig 1965, Lilj 1935, Lind 1972, Moul 1995, Powe 1993, Rhod 1986, the 2,000 page Tayl 1979, and Wein 1990. Together, these ten books provide a good and reasonably complete picture of the major elements of the early history. The other references supplement the picture, at times somewhat anecdotally. The selection reflects availability and does not claim to be complete.

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CHAPTER 2. LIGHT

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During the 19th Century, a common perception was that mankind basically knew the laws of nature, and little remained to be discovered. That view was bumped when, in 1895, Röntgen discovered x rays. Prominent scientists such as Becquerel, the Curies, Planck, Einstein, Rutherford, and Bohr soon added further findings.

Early on, it became clear that radiation was both good and bad. Medical uses of radiation permitted vast progress, but careless use of the new tools entailed a risk of serious harm. This led to international collaboration on radiological protection. In the late 1930s, nuclear fission was demonstrated, corroborating Einstein's thesis that matter can be turned into energy.

Pandora's Box, the first part of four in this popular science history, is an overview from the Ancient Greeks and their ideas about light, until just before World War II when scientists in several countries began to consider nuclear weapons. It is aimed at persons with a general interest in radiation and requires no previous knowledge.

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 the International Commission on Radiological Protection (ICRP) and the Swedish delegate to, and for a time Chairman of, the United Nations Scientific Committee on the Effects of Atomic Radiation (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.