Looking back in admiration

Farewell lecture Anne Kox, University of Amsterdam, 12 September 2013

Ladies and gentlemen:

I am very happy to welcome you here for my farewell lecture. A special word of welcome to the Rector of this University and to the Dean of the Faculty of Science, Mathematics and Information Science: I very much appreciate your presence. A special word of welcome as well to Professors Roger Stuewer of the University of Minnesota and Jed Buchwald of the California Institute of Technology, both former Pieter Zeeman Visiting Professors of History of Physics at this university, and to Professor Diana Buchwald, Director and General Editor of the Einstein Papers Project, also at the California Institute of Technology. I am much honored by your presence on this day and I hope that you will forgive me for adhering to tradition and delivering my lecture in my native language. Hopefully, the English translation of my power point presentation, which I have provided for you, will compensate somewhat for this inconvenience.

I was hoping to be able to welcome my successor, Professor Jeroen van Dongen, on this occasion and I am so happy to see that the procedure for his appointment was completed in the nick of time, so that he could be part of the ceremony in full regalia. For me it is very important that I will have a successor, especially one who will be appointed full professor, instead of extraordinary professor. I am very pleased that the Faculty of Science, Mathematics and Information Science has considered my professional pursuits important enough to continue to provide for teaching and research in the history of physics. I take this as recognition of all my efforts in the past decades.

I am leaving the University of Amsterdam because in July I reached the age of 65. Just to reassure you (or perhaps to your horror), I am not leaving science altogether. On the contrary, in my quality of Senior Editor at the Einstein Papers Project I will crank up my activities, and when I am not in California, or in my beloved Italy, I will continue to work on my research projects at home, in my own study.

Before I begin, I would like to show you a photograph. You may have wondered what the images meant on the invitation to this lecture. Here you see the photograph from which they were taken. For some of you this is a well-known picture, but for those of you who see it for the first time, some explanation. These are the participants in a very special meeting, the Solvay Conference of 1911. In the fall of that year, a carefully selected group of scientists met in Brussels in the luxurious Hotel Metropole to discuss the latest developments and problems in physics, in particular with regard to the brand new quantum theory. The meeting took place at the initiative of the German physical chemist Walther Nernst and was funded generously by the industrialist Ernest Solvay—hence the name. The Chairman was our fellow countryman Hendrik Antoon Lorentz. Among the 18 participants were 9 (future) Nobel Prize laureates: Lorentz, Albert Einstein, Walther Nernst, Heike Kamerlingh Onnes, Wilhelm Wien, Ernest

Rutherford, Max Planck, Marie Curie and Jean Perrin. The 1911 conference was the first in a series that continues to the present day.
There is a curious story connected with this picture. The man next to Lorentz is Ernest Solvay, and if you notice that his head is rather large, you are absolutely right. Solvay himself was not present when the photograph was taken, so in order to have him in the picture, his head was later superimposed on that of an anonymous man who had taken his place at the photo session. Unfortunately the head did not have exactly the right size, but the organizers were quite satisfied with the result, according to correspondence by Lorentz that I have recently found.

Now let us get to the point. Many years ago, in the early nineteen nineties, I was giving a lecture at the Institute of Theoretical Physics of this University, the institute that has been my official home base from 1969 to this day, as a student, a PhD candidate, a postdoc, recipient of an NWO stipend, and eventually a Professor. I was lecturing on the history of the origin of Einstein's Theory of General Relativity, a subject which I had recently explored for my work on volumes three and four of Einstein's Collected Papers. Since the audience consisted of physicists, I could go into all sorts of technical details. During my discussion of one of these details, one member of the audience expressed his amazement about a particular choice Einstein had made in the elaboration of his theory: "But that is completely wrong. How can it be that he did not see that"! For him, Einstein fell off his pedestal: a dunce. I tried to explain that you cannot look at early physics with the knowledge of today, as we are wont to say, but that you should try to find out what thoughts and methods were used in the past. Such knowledge often leads to a deeper insight into modern theories of physics. As an example I mentioned how I had begun to understand the General Theory of Relativity better after carefully studying the history of its origin. The commentator was not convinced. He shook his head and continued to be amazed at such stupidity. I let it be and continued my lecture.

When I thought about this incident, later on, I blamed myself for not having been clear enough in my explanation about the importance and the value of the history of physics. I had not pointed out to him that history of physics teaches us how, at one time or another, theories, ideas, and concepts in physics, which are now accepted as obvious, came about and were accepted with difficulty and through much discussion and controversy. I believed especially that I had not emphasized enough how historical knowledge about the achievements of earlier generations leads to admiration, rather than contempt—as in my commentator—about what they did not know or had wrong. They were not at all stupid in the past: on the contrary, they were very smart! But you only see that when you delve into the state of affairs at the time and understand the context in which new ideas were developed.

Moreover, I am convinced—as I said just now—that knowledge of the history of how important theories came about, knowledge about the intellectual struggles that preceded their establishment, can—as a bonus—deepen the content and the meaning of present-day physics.

In later years, in conversations with colleagues and students, and in my teaching as well, I have often spoken in this way about the importance of the history of science in general and the history of physics in particular. However, I have never done so in public. That is why I am using this opportunity, my last official action as a Professor at this University, to discuss this topic. I sincerely hope that, after hearing my arguments, you will understand and appreciate the title I have chosen for this farewell lecture. In addition, I would like to share with you my passion for my subject and make you understand why I have pursued it with so much enthusiasm for so many years.

Before I do so, let me summarize the most important points I would like to illustrate in today's discussion:

In the first place I would like to show that we should judge the past from the standpoint of the *past*, and not from the *present-day* standpoint, and that we should continuously keep in mind that what is self-evident *now* has not forever been so obvious. If we look at history in this way, it almost goes without saying that we begin to feel great admiration for the achievements in the past.
Secondly, I want to show how we discover in textbooks and popular discussions that history is often simplified to a 'streamlined' narrative, a clear succession of logical steps that lead us, without any hitches, to a particular result. While what happened in reality was perhaps more complicated, but also many times more exciting and admirable, and as a bonus it provides us with a deeper insight into present-day science.
Finally, in my lecture I would like to show you how complicated a process the acceptance of new theories and new ideas really is.

I would like to illustrate my argument with three 'case histories', three episodes from the history of modern physics, borrowed from the work of three physicists who have deeply influenced the development of their subject in the twentieth century. Most of you will not be surprised to hear that I will be discussing, among other things, the work of Albert Einstein and Hendrik Antoon Lorentz, the two physicists whose work and life have kept me occupied intensively over the past thirty years. But before getting to them, I would like to discuss the work of a slightly lesser known, but also very important physicist. I am talking about the Danish physicist Niels Bohr and his atom model. It is not by accident that I am selecting this topic: this year it is one hundred years ago that Bohr published his atom model and that has been – and is still being – celebrated in quite a few different ways.

I do not know what it is like now, but in secondary school I learned that Bohr is the creator of the modern atom model, the model in which electrons, small negatively charged particles, orbit around a heavy, positively charged, atom nucleus. The way it was presented in my school physics textbook it was a simple situation from which all sorts of things could be deduced with some simple mathematics. What they did not tell you was that Bohr's model went completely against the current ideas about moving charged particles, which had been confirmed time and again. What was the case? According to the standard theory, a charged particle in such an orbit would rapidly lose its energy, and thus its speed, by emitting radiation and would then land in the nucleus around which it had been circling until then.

Bohr knew this very well, of course, when he published his model. How, then, did he resolve this problem? Very simply and exceedingly unorthodox: in the publication in which he revealed the model, he postulated without much ado that his model was stable, even though this was in flagrant contradiction with the accepted theory. When they move in their orbit, electrons do not emit radiation; only when they 'jump' from one orbit to another do they emit, or absorb, radiation. The reader was expected to just accept this on faith. In the textbooks that our students study today this point is mentioned, of course, but always in the context of current physics and, especially, in the context of quantum theory. The laws of this theory are fundamentally different from those of the old 'classic' physics, which implied the instability of the model. When I read and discussed the Bohr article with my students, during my classes on the history of modern physics, I always tried to make them read the article from the standpoint of physics in 1913, the year in which the article appeared. Of course you cannot just forget everything you have learned about the later developments, but you can try to read a text with an open mind and that is what I tried to get my students to do. Then you do not cease to be amazed: as I said, Bohr adamantly makes his totally implausible assumptions and goes on to try and explore the consequences of his model. In the process he also offers statements or assumptions that are barely motivated, simply because they can barely be motivated. They do lead to a miraculous result, though, a result that Bohr derives in four different ways (and in each of these derivations there are steps that cause amazement), just to show that the result is really inherent in his model. The end result—and I will not go into the more technical details—is that Bohr manages to calculate the value of an important constant that had appeared experimentally during the study of spectral lines: the radiation emitted by atoms. This result was the goal that, so to speak, justified Bohr's rather rickety means.

Reading and analyzing this article, amazement is followed by admiration of Bohr's miraculous intuition, his tenacity in exploring the implications of an idea that seemed absurd at first blush, and his courage to subsequently present his results. In short, we see a totally different Bohr than the one in the textbooks, a Bohr who does not care about established ideas but allows himself to be guided by his intuition. And only now do we see how new and revolutionary Bohr's ideas really were, how much they were a breach with the past.

Especially because of the experimental successes, Bohr's theory was taken seriously and it was quickly accepted. In my next example of an idea that could almost be called absurd, the situation is totally different.

I am referring to Albert Einstein. His name has already come up earlier. I am taking you back to the year 1905, also know as the *Annus Mirabilis*, Einstein's Miracle Year, the year in which he published three articles over the course of a few months, each of which would have been enough for him to win the Nobel Prize. The article I am referring to was dubbed "revolutionary" by Einstein himself—and that is quite a something considering that one of the other 1905 articles presented the theory of relativity, the theory generally associated with Einstein, and which, without exaggeration, could truly be called revolutionary. We will come back to the theory of relativity shortly; now we will discuss the other article, the publication about the so-called light quantum.

Five years earlier, in 1900, the German physicist Max Planck had formulated a daring hypothesis, which stated essentially that energy was somehow exchanged in discrete quantities in the interaction between radiation and matter. I am purposely formulating this somewhat vaguely, because the physical meaning of this so-called quantum hypothesis was anything but clear, even though it is presented in a mathematically clear form in the theory. Background of Planck's work was the formulation of a so-called radiation law, a law that governs the relationship between wavelength and intensity of heat radiation. Based on his hypothesis, Planck could derive a law that agreed perfectly with very accurate experimental results.

Now Einstein. In the introduction to his article, Einstein makes a fundamental statement (as he often does in the introductions to his articles) that will be the starting point for the rest of the article. He philosophizes about the curious phenomenon that in physics such a clear distinction is made between atoms and other particles, on the one hand, which are discrete (you can point to them and count them), and electromagnetic entities, such as fields and waves, on the other, which have a continuous character and cannot be described with a finite number of variables. Is that a fundamental difference, or does it only appear so? Is there perhaps a deeper connection between waves and particles? To investigate this, he starts with the radiation law I just mentioned, which Planck had derived with his quantum hypothesis, and brings to bear a few simple thermodynamic considerations. I will not bore you with the details, but the final result is that he reaches the conclusion that a quantity of radiation (think of light) can sometimes behave as if it consisted of discrete units, some sort of radiation particles, which he calls light quanta. Indeed revolutionary, and an indication that Planck's quantum hypothesis has farther-reaching consequences than just being a device for the derivation of a radiation law and the first step on the way to the wave-particle duality which is a cornerstone of modern physics.

To put his results on a firmer footing, Einstein goes on to discuss a few experiments that could be explained nicely with this light quantum hypothesis. One of them is the so-called photoelectric effect—I will not bother you with details here either. I only mention it because physicists still call Einstein's article the article on the photoelectric effect, though in fact it discusses much more fundamental issues. (Just recently, in the biography of a well-known physicist, I read the claim that Einstein wrote his article in an attempt to explain the photoelectric effect. The fact that this effect was mentioned specifically when he was awarded the Nobel Prize in 1923 has surely contributed to this confusion.)

With Einstein's revolutionary idea, things went differently than with Bohr's atom model. People simply did not believe Einstein. All well and good, they said, but combining particles and waves is not possible, it is contrary to the deepest foundations of physics. Even when Einstein was nominated for the most prestigious position in the natural sciences in his day, the paid membership of the Berlin Academy of Sciences, those who nominated him, amongst whom was Planck, tried to find excuses for the wrong move Einstein had made with this hypothesis. Einstein himself, for that matter, was not fazed. On the contrary, in a groundbreaking article in 1917 he extended his hypothesis and showed that his light quanta did not only have energy, but also momentum. In that article he also published ideas that form the basis for the theory of lasers. It was not until 1920 that strong experimental support appeared for the hypothesis, so that people began to accept it.

What do we learn from this article? Again, the importance of careful reading of the original sources and the power of a seemingly absurd hypothesis. Just like with Bohr, simple arguments lead to astounding conclusions. But while Bohr had the support of established experimental results, Einstein did not. Those who were skeptical about Bohr's ideas were won over quickly, while this took quite a while in the case of Einstein. So we also learn something about the acceptance process of new theories, and about the belief of an individual scientist in his idea, ignoring skepticism and going completely against the grain.

Now I am running the risk of sounding to my colleagues like a hopelessly old-fashioned historian of science who only looks at the heroes of science and at their successes. One who ignores what Thomas Kuhn called 'normal science', the day-to-day craft of doing science and encountering all the things that can go wrong in the process. For that reason I will now present a third and last episode from the history of modern physics, slightly different in character from the first two. They were mostly about miraculous discoveries and leaps of thought, but now we will look at a case in which we will look back, at least initially, in surprise, rather than admiration. The main character is, again, someone with whom I have occupied myself for a long time: Hendrik Antoon Lorentz, the great physicist from Leiden, one of the first Nobel laureates, and for a long time the most prominent theoretical physicist in the world. I want to discuss the relationship of his work with Albert Einstein's special theory of relativity.

First let us return to the nineteenth century. In the theory of electrical and magnetic phenomena a strange substance played a central role. This substance was called the ether and could not be compared in any way with a normal substance, with normal matter. The ether entirely filled all space, even penetrating inside atoms and molecules, but it did not have any mechanical properties, such as mass or kinetic energy, except for one: the ether was considered to be completely at rest. Why was this ether needed? In the first instance, because it was impossible to understand how light propagates without it. As had become clear in the course of the nineteenth century, light was a wave phenomenon. But waves must propagate in something (think of water waves that cannot exist without water) and for that purpose physicists had postulated the ether: as the carrier of light waves. When it appeared, later on in the nineteenth century, that light was a special case of general electromagnetic waves, so that optics became a part of the theory of electromagnetism, the ether became a basic principle in this theory.

Yet the ether also created problems. If light propagates with a fixed velocity through the ether and if the earth also moves through the ether, the speed of light on earth would not have the same value in all directions. If we look at a light beam that runs in the direction in which the earth is moving, the earth will catch up slightly with this light beam, so that the speed of light will appear to be less, and if we look at a beam of light that runs counter to the motion of the earth, the beam will appear to be going faster. It is like the difference between moving with headwind or tailwind: a tailwind does not seem to be as strong as a headwind. Because of this analogy, physicists were used to speaking of the 'ether wind', which they should be able to detect. Except that they could not do so. All experiments that were designed to do just that failed miserably. Apparently, the earth did not move in the ether, which did not seem very likely for an insignificant little planet like ours in the immense universe. Light, as I already said, is an electromagnetic phenomenon, so it is obvious that the ether wind should also be an obstacle for other electromagnetic phenomena. Such phenomena did indeed exist and in these cases experiments did not show any trace of an ether wind either.

Now Lorentz. Early on in his rich career, during which he carried out important work in all different areas of theoretical physics, he had one topic to which he returned time and again: the design of a theory for electromagnetic phenomena for systems in motion. With motion I mean here: motion with respect to the ether. This theory was expected to be able to account for all electromagnetic (including optical) phenomena on earth.

Having started on this task around 1880, Lorentz was able, in 1904, to present in a groundbreaking article a definitive theory in which all experiments were explained. He did require an important artifice: the hypothesis that bodies moving through the ether become slightly shorter. Later this hypothesis has often been criticized as *ad hoc*. Without justification, but here it is not the place to go into this matter further.

All this appears unproblematic. However, a year later, in 1905, a competing theory appeared. You remember the year 1905 as Einstein's Miracle Year. Apart from his work on the light quantum hypothesis, that was also the year of the arrival of the theory of relativity. This theory also dealt with electromagnetic phenomena in systems in motion. But Einstein's approach was completely different. To explain this, I need to tell you something about the so-called relativity principle. This principle goes back, in the first instance, to Galilei and had become one of the foundations of classical physics, in particular of mechanics. The relativity principle states that the same laws of physics hold in systems in motion as in systems at rest. In more concrete terms: assume that I am sitting in a train, in a nice luxurious coach, in which a pool table has been set up for the pleasure of the passengers. All the train's windows are shuttered, so that I cannot look out to see whether or not the train is in motion. The relativity principle states that from the way the pool balls move across the table I will not be able to see either whether or not the train is in motion. The reason for this is that the rules and laws describing the trajectories of the pool balls are independent of the motion of the train. More generally: no mechanical experiment can decide whether or not the train is in motion. A beautiful general principle, and that is what physicists love.

However, we just saw that the existence of the ether would imply that for electromagnetic phenomena we should be able to decide whether or not the earth is moving through the ether. The relativity principle apparently holds for mechanics, but not for electromagnetism. (You can see this from the mathematical form of the fundamental equations of electromagnetics.) So, even though Lorentz had shown that all possible experiments to account for the motion of the earth would lead to nothing, this did not mean that the relativity principle would also hold true for electromagnetic phenomena.

Einstein was not satisfied with this situation. In his famous article he offers a few very general arguments to explain why he believed that the relativity principle should hold for physics as a whole. Then he goes on to claim it as one of the two postulates for a new theory. Based on his postulates, which require a revision of our ideas about space and time, Einstein develops a general theory for electromagnetic phenomena in systems in motion: the theory of relativity. In this theory the laws of physics are independent of the state of motion of the observer, just like in mechanics. So there is no longer anything like absolute rest or absolute motion: only the motion of observers with respect to one another is important. That means the end of the ether as the representation of absolute rest.

The neat thing about Einstein's theory is that its formalism and its experimental elaboration are identical to Lorentz's theory of a year earlier. Except—and I cannot emphasize this enough—that which constituted the result of his theory for Lorentz, namely that no experiment can demonstrate motion through the ether, represented the starting point for Einstein. I cannot express it more beautifully than Lorentz himself has done:

"Einstein simply postulates what I have derived, not always without difficulty".

So we are dealing here with two theories that make the same predictions but on totally different foundations. How did other physicists regard this situation? In the first instance, there were only a few who really understood the fundamental difference—some even referred to the Lorentz-Einstein theory. But gradually they developed more insight into the differences and then, little by little, the theory of relativity gained the upper hand. Yet there were also physicists who stuck to the old ideas. The most important of these was Lorentz himself. He could not let go of his beloved ether and continued, until his death in 1928, to hold on to his ether theory – although he had a great deal of respect for Einstein's work and even lectured to students on the theory of relativity.

In the twenties of the previous century the theory of relativity had been almost universally accepted and by then Lorentz stood practically alone with his ideas. Was he an old fogey who could not be taken all that seriously any more? Was he a striking illustration of Max Planck's adage that the dying off of old physicists with antiquated ideas was an important factor in the acceptance process of new ideas?

If we look at what Lorentz had to say, we need to refine this picture. He had reasonable arguments that he defended time and again. For example: if there is no ether, how should I imagine the electromagnetic waves? Do they propagate in empty space? But what is it that vibrates then? How should I picture this? And how about the energy involved in electromagnetic fields? Where is it? As Lorentz formulated it: the ether functions as a kind of coathanger to hang all this on.

Just as in the two earlier episodes, we can take a look at the acceptance process, in this case of the theory of relativity. It is different again from the situation with Bohr's atom model or Einstein's light quantum. In the theory of relativity, considerations of simplicity, elegance, and the desire to eliminate a superfluous concept played a role. Lorentz's questions, which I just mentioned, were steadily considered more and more irrelevant. Just get used to the fact that these waves propagate in emptiness. There is no need for an ether; an ether-less theory is simpler and more elegant (Occam's razor). Moreover, the theory of relativity is based on a few very fundamental and general assumptions, while Lorentz's theory is more cumbersome and complicated. That was for many physicists an important argument in favor of the theory of relativity.

Earlier, I discussed 'streamlined' versions of history. In the case of the theory of relativity there is also a persistent, oversimplified narrative that crops up time and again. In many textbooks the suggestion is made that Einstein's work resolved a deep crisis in physics, a crisis that resulted from the inability to demonstrate experimentally the motion of the earth through the ether. Yet we just saw that there was no such thing as a crisis, as Lorentz's theory worked just fine. Einstein's relativity article was no more meant to resolve a perceived or imaginary crisis than his article about the light quantum was inspired by his wish to explain the photoelectric effect. Closer consideration of Einstein's original article and the arguments that are put forward there teaches us that Einstein, rather than resolving a crisis, eliminated a fundamental shortcoming of physics which he had detected. This insight throws a very different light on Einstein's work and only serves to increase the admiration for it.

Now I am coming to the conclusion of my lecture. I hope that you have learned something from what I have told you here. More specifically, I hope that I have managed to make clear to you how fascinating, educational, and relevant the study of science's past can be. I also hope that I have been able to show you that in my approach to the history of science, the emphasis lies on the scientists, on the development of their ideas and theories, and on a thorough knowledge of the scientific context in which this happened. Without my thorough training as a theoretical physicist I would not be able to take this approach. In this regard it is a pity and a diminishment of the history of science that there are fewer and fewer historians of science who have had training in one of the hard sciences. My great example in my work as a historian of science, from whom I have learned the craft and whom I consider to be my teacher, was the American historian of science Martin J. Klein, who was also an excellent physicist. Fifteen years ago he was here to hear my inaugural lecture; unfortunately he did not live to participate in this event. I regret this more than I can say.

Having reached the end of this farewell lecture, the end of my professorial and academic career at this university, it is fitting to thank a number of people with whom I have shared many valuable experiences over the years and to whom I owe a debt of gratitude. I will mention a few of them specifically, not necessarily in order of importance:

My colleagues in theoretical physics, who have always continued to consider me a colleague, though I was somewhat of a stranger in their midst, and who have viewed my work as a valuable complement to hard theoretical physics; in particular I would like to mention Leendert Suttorp, Leo van den Horn, Kareljan Schoutens, and Sander Bais.

Also many thanks to Yocklang Chong, Joost van Mameren, and Anne-Marieke Crommentuijn of the Institute of Physics for their indispensable assistance in preparation of this afternoon’s event.

Thanks to my colleagues at the Department of History for the pleasant cooperation: in particular Henk van Nierop, Niek van Sas, and Hans Blom.

The Board of the Stichting Pieter Zeeman Fonds, who have made my Chair possible and who have supported me in so many ways over the past fifteen years, Hans Croes, Jaap Penders, Harrie-Jan Metselaars, and Ben van Linden van den Heuvell: many thanks; a special word of thanks to former Board member Norbert van den Berg, who has played a crucial role in establishing the Pieter Zeeman Chair and in having me appointed, fifteen years ago.

I am also grateful to my current and former colleagues at the Einstein Papers Project for providing the stimulating and congenial surroundings in which I have worked, and from whom I have learned so much, first in Boston and now in Pasadena. Since 1985 I have spent a total of some ten years of my life with the Project, and it has been (and still is) a wonderful experience. I am honored and pleased that the current Director and General Editor of the Project, Professor Diana Buchwald, is here today, not only as a representative of the Project, but also, and more importantly, as a friend.

Finally, I am looking in admiration, this time not backwards, but forward, to the first row of seats in front of me: to the two people who are closest to my heart: my wife Henriette and our daughter Laura. Without their loving, but also critical, support (I sometimes call them my most faithful fans and my sharpest critics) I would not have been able to do what I have done over the years.

Thank you for your attention.

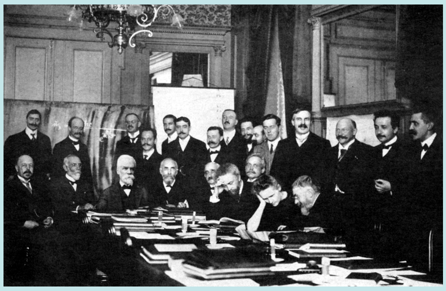
UNIVERSITEIT VAN AMSTERDAM

Faculteit der Natuurwetenschappen, Wiskunde en Informatica

Afscheidscollege *Omzien in bewondering*

Prof. dr. Anne J. Kox

DONDERDAG 12 SEPTEMBER 2013



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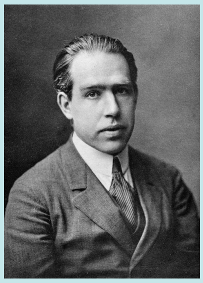
Points:

- Do not judge history from the present, but from the historical context: what is self-evident *now* has not always been so. Knowledge of the origin of theories surprises us, leads us to admiration, and deepens our insight into present-day science
- Do not trust 'streamlined' versions of scientific developments, but read and analyze the primary sources
- Acceptance of new ideas and theories is a complicated process

3

Niels Bohr (1885-1962)

Nobel prize 1922



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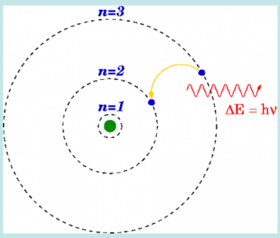
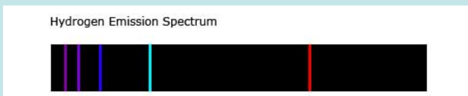


Bohr: electrons move in fixed orbits around an atom's nucleus and only emit radiation when they 'jump' to a different orbit

5



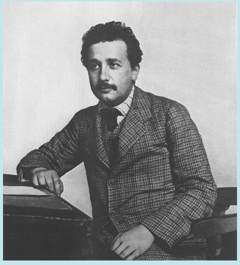
Empirical formula for the hydrogen spectrum:

$$\frac{1}{\lambda} = R\left(\frac{1}{n^2} - \frac{1}{m^2}\right)$$

λ wavelength (color), *m*, *n* integers, *R* constant

6

Albert Einstein (1879-1955)
Nobel prize 1922



7

Einstein in 1905:

Dichotomy in nature:

Matter consists of a finite number of atoms and other particles: matter is ‘discrete’.

Electromagnetic phenomena (waves, fields) have a ‘continuous’ character: you cannot describe them with a finite number of variables

8

Einstein’s *Light quantum hypothesis*:

“Sometimes light behaves as if it consisted of energy units localized in space: light particles or light quanta”

The beginning of the ‘wave-particle duality’ of modern quantum physics

9

Nobody believed Einstein:

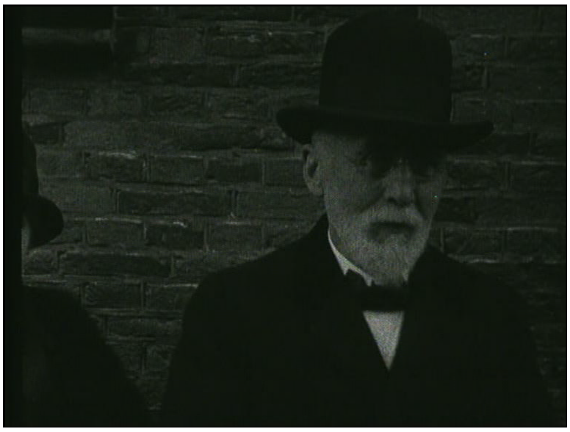
“That he might sometimes have overshot the target in his speculations as, for example, in his light quantum hypothesis, should not be counted against him too much”
(Max Planck et al., June 1913)

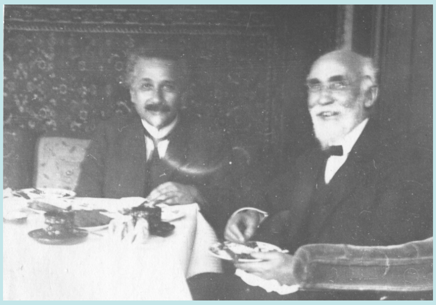
Acceptance only around 1920

10

Hendrik Antoon Lorentz
(1853-1928)
Nobel prize 1902

11





13

Electromagnetic phenomena occur in the 'ether' (e.g. light waves).

The earth moves through the ether: it should be possible to detect that motion through electromagnetic experiments. (observing the 'ether wind').

But: all experiments had negative outcomes

14

Lorentz:

Theory of electrons (1904)

From the theory of electrons it follows that it is impossible to demonstrate the motion of the earth through the ether by means of experiments.

This explains the negative outcomes of the experiments

15

The relativity principle: "The laws of mechanics are the same in a moving train and in a train that is standing still." *So you cannot determine by means of mechanical experiments whether or not the train is moving.*

But: the relativity principle does *not* hold for electromagnetic phenomena. *So you should be able to determine by means of electromagnetic experiments whether or not the train is moving.*

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Replace 'train' by 'earth':

The relativity principle does *not* hold for electromagnetic phenomena. *So you should be able to determine by means of electromagnetic experiments whether or not the earth is moving.*

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So:

'Paradox' in Lorentz' theory of electrons:

- Motion of the earth through the ether cannot be determined experimentally
- Relativity principle does not hold for electromagnetic phenomena

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Einstein (1905), based on some very fundamental considerations:

- *Postulate* the relativity principle for electromagnetism. That will, among other things, explain the experimental results
- Together with a second postulate and a revision of the ideas about space and time, this leads to the *Theory of Relativity*

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Comment by Lorentz:

“Einstein simply postulates what I have deduced, not always without difficulty”

Explanation:

For Lorentz the impossibility to determine the motion of the earth is the *result* of the theory; for Einstein it is the *starting point*

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Important:

- The theory of relativity and the theory of electrons make the same experimental predictions
- The theory of relativity makes the ether superfluous: you cannot demonstrate motion though the ether
- But: Lorentz continued to believe in the existence of the ether. He had good reasons to do so

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Well-known misunderstanding (‘streamlined story’):

“Einstein wanted to resolve a serious crisis in physics by explaining the negative experimental results with his theory”

But:

- There was no crisis: Lorentz’ theory worked well and was accepted
- Einstein was motivated by a much more fundamental problem with electromagnetism

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Lessons?

- Knowledge of the context in which a theory originates offers insight into the content and the foundations of the theory and causes amazement and admiration
- Beware of ‘streamlined’ historical narrative
- Acceptance of a new theory is a complicated process that differs from case to case

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Afscheidscollege *Omzien in bewondering*

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