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The History of Quantum Mechanics

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Quantum mechanics is one of the cornerstones of modern physics and a scientifically informed philosophy of nature needs to integrate it. Not surprisingly, its genesis and its further historical development figures prominently in many debates, given that the integration of history and philosophy of science has grown in popularity. Unfortunately, many of these historical narratives suffer from distortions and misrepresentations – a phenomenon known as "Whig-history" among historians. This entry provides a brief summary of the history of quantum mechanics between 1900 and 1930 and puts special emphasis on issues which are relevant to the philosophy of nature and particularly affected by whiggish tendencies.

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1. Introduction

There is a widespread tendency, in particular in physics textbooks, to tell a so-called "Whig history", a term introduced by Herbert Butterfield in 1931 to characterise a historiography which judges the past by present standards, or, in his own words: "The study of the past with one eye, so to speak, upon the present is the source of all sins and sophistries in history, starting with the simplest of them, the anachronism" (Butterfield [1931] 1965: 21). To call a historical narrative "wiggish" has become a synonym for the tendency to streamline the presentation of historical events such that the current state of knowledge gets a sense of inevitability. This involves for example the neglect of the original motivation of the historical actors, ignoring blind alleys of research, the influence of specific social and economic contexts and the like (Russell 1984). The effect can be twofold: For concepts still in use, such a narrative tends to exaggerate the continuity and falsely portrays the history of physics as a cumulative sequence which finally and necessarily led to the acceptance of the current theories. If, however, some conceptual change occurred, like the advent of quantum theory, such a narrative typically overemphasises the rift between the "old" and the "new", i.e. ignores the elements of continuity which are still present. Again, such a judgement – namely that some problem was intractable in the old framework and forced a new framework to emerge – can only be made from hindsight, i.e. is anachronistic likewise. These two errors, i.e. the respective overemphasis of continuity or discontinuity, though seemingly opposite in character, turn out to be just two sides of the same coin (cf. Wilson/Ashplant 1988: 15).

The present entry places special emphasis on the above-mentioned distorted presentations of the history of quantum mechanics in physics textbooks – sometime also referred to as "quasi-history" (Whitaker 1979). The reader may think that textbook-critique is rather a topic for physics education and not relevant to the philosophy of nature or science.[1] However, textbooks reflect the attitude of the practitioners and have an impact on future scientists that is easily underestimated. It should be kept in mind that physics – in contrast, e.g. to the humanities – is a typical textbook-science and that no physics student is supposed to read, say, Newton, Maxwell or Einstein in the original, let alone to study the history of his subject. Eventually, these distorted views may be carried into philosophical debates. Indeed, some eminent physicists were rather outspoken in their

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[1] It certainly is also a topic for physics education; see Passon (2022) were some of the current material is discussed in this context.

disregard for detailed and scholarly acceptable history. For example Richard Feynman, Nobel Prize in Physics 1965, confessed in a popular book on quantum electrodynamics: “By the way, what I have just outlined is what I call a ’physicist’s history of physics,’ which is never correct. What I am telling you is a sort of conventionalized myth-story that the physicists tell to their students, and those students tell to their students, and is not necessarily related to the actual historical development, which I do not really know!” (Feynman 2006: 6)

A similar remark can be found in the popular science book “The God particle”, authored by the American physicist Leon M. Lederman, Nobel Prize in Physics 1988. Therein Lederman confessed openly with respect to the common “myth-history”: “However; from the point of view of storytelling, myth-history has the great virtue of filtering out the noise of real life […]. There may, in fact, be no source for some of the best stories in science, but they have become such a part of the collective consciousness of scientists that they are ‘true’, whether or not they ever happened” (Lederman 2006: 412).

We will try to correct some of these problematic “myth-historical” accounts, but not just for the sake of historical accuracy alone. As indicated above we believe that they promote a naïve view of the history and philosophy of science by applying the standards of current knowledge to past events and actors (cf. Perillán 2021). The strategic role of distorted “textbook-history” was already discussed in the 1960s by Thomas S. Kuhn in his “Structure of Scientific Revolutions” who noted that these accounts of the history produce among students and researchers a sense of participation in a certain methodological and social tradition which never existed (Kuhn [1962] 1996: 137 f.).

To give a comprehensive report of this development which spans roughly the period from 1900, i.e. Planck’s law of blackbody radiation, to 1930, when the formalism was settled, on the level of a lexicon entry is virtually impossible. Thus, in this entry we have to make choices, motivated by personal interest and especially the relevance to the *philosophy of nature*. Here, we focus on the issue of discreteness versus continuity, the so-called wave-particle duality, and, somewhat related, the question of proper parthood relations. In addition and as mentioned above, a specific focus is put on events whose history is typically distorted by whiggish narratives.[2]

Parts of the following presentation assume some background in quantum mechanics already (for a short introduction see Passon/van Strien 2022) and in few places we even could not avoid some technical details. The less skilled reader may skip those passages without harm.

2. Some remarks on pre-quantum and quantum physics

The origin of quantum physics in 1900 is usually portrayed as a dramatic rift with the former “classical” or “mechanical” conception of nature. We are usually told that the main features (and chief novelties) of quantum theory are discreteness and indeterminism while classical physics assumes continuous motion guided by strict and causal laws. While the origin of this narrative is understandable it does not bear close scrutiny. A more balanced view needs to include some issues we can only mention briefly.

For one thing, in 1900 the “mechanical” worldview was compromised already by the advent of James Clerk Maxwell’s electrodynamic (published in 1865). It is certainly true that, e.g. by constructing mechanical ether models, some researchers tried to reconcile electrodynamics with more traditional mechanical notions. However, there were also the opposite attempts and at the turn of the century the so-called “electromagnetic world view” (“elektromagnetische Weltsicht”) was

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[2] There are comprehensive accounts on the history of quantum physics like Max Jammer’s classic from 1966, Darrigol (1992), the monumental six volumes published by Jagdish Mehra and Helmut Rechenberg between 1982 and 2001 on “The Historical Development of Quantum Theory” or the recent piece by Duncan and Janssen (2019). A knowledgeable account which addresses a wider audience can be found in Kragh (1999) and more specific issues are dealt with in, e.g. Kuhn (1978), Kragh (2012) or Badino (2015). Seth (2013) provides an excellent handbook article. It goes without saying that these accounts from the history of science are essentially free of the quasi-historical distortions mentioned above in connection with typical textbook narratives. This holds at least with one restriction: We suggest that any potential reader of Mehra and Rechenberg should consult the harsh criticism of this work by Forman (1983) before.

rather popular which tried to ground all of physics onto this new framework instead of in mechanics. The historian Russell McCormmach has noted that this was also supported by the particular zeitgeist. He summarised the situation as follows: "The whole cultural configuration at the turn of the century was implicated in the change from mechanical to electromagnetic thinking. The immaterial electromagnetic concepts were attractive in the same measure that the inert, material imagery of mechanics was displeasing" (McCormmach 1970: 495).

Another research direction was the so-called "energetic world view" or simply "energetics", pioneered by Georg Helm and picked up, e.g. by the chemist Wilhelm Ostwald. Also here, as with the electromagnetic world view, a non-materialistic (and non-mechanical) monism was aimed at, i.e. explaining every physical event not on the basis of matter and mechanical forces but on the basis of energy and its transformations (Kragh 1999: 7 ff.). This line of research aroused some interest in the late 19th century and was, e.g. subject of a lively debate at the "Naturforscherversammlung" (i.e. a large gathering of German-speaking physicists) in Lübeck in 1895 (Deltete 1999).

Furthermore, thermodynamics and its possible foundation through statistical mechanics were at the forefront of the research at that time. These developments emphasised the importance of probability considerations and Franz S. Exner famously suggested in 1909 that all of physics could possibly be grounded on randomness. Exner was one of Schrödinger's academic teachers in Vienna and his former student defended this indeterministic view in his inaugural lecture, delivered 1922 but published later (see Schrödinger 1929).

Already these brief remarks suggest a complexity and diversity of pre-quantum physics which is hardly captured by the common label of "classical physics". As pointed out by Staley (2005) this label overemphasises both, the rift between the so called "classical physics" and the so called "quantum physics" as well as their respective uniformity. In fact, Staley argues that the very notion of "classical physics" was co-created together with the notion of "modern physics" in 1911 and also served a strategic function. As pointed out by Gooday and Mitchell (2013), the use of the term "classical" allowed physicists to "establishing broad cultural connections between physics and other knowledge-making enterprises" (ibid.: 729). And perhaps even more important, the use the term "classical" allowed physicists to bypass thorny epistemological questions surrounding the partially discredited theories of the 19th century. They were turned into a "classical" canon rather than a falsified precursor. Thus, to put it pointedly: Quantum mechanics did not overturn "classical physics" but established the very notion. Let us now turn to our main concern, namely the developments which led into quantum physics eventually.

3. Planck and the quantum

Famously, this development started with the investigation of "blackbody radiation". The underlying phenomenon is easy to explain: As is well known heated bodies eventually start to glow. As is reflected in the common names "red heat" or "white heat" the "colour" or, more precisely, the "frequency" of the radiation is directly related to the temperature. Already in the mid-1850s it was argued that for an idealised physical body that absorbs all incident radiation ("blackbody") the radiation-spectrum should be independent of the shape or material of the body. This universality made the "blackbody radiation" problem a perfect testing ground for theoretical models. On the experimental side the leading experts were gathered at the "Imperial Institute of Physics and Technology" ("Physikalisch-Technische Reichsanstalt", PTR) in Berlin-Charlottenburg. However, the experimental investigations at this institute were largely driven by economical interest. The German lighting and heating industry – one of the largest consumers of the institute – was interested in the results (Kragh 1999: 59). In 1900 Max Planck could provide the still valid description of this spectral energy density of a blackbody in thermal equilibrium in the frequency interval $[\nu, \nu + d\nu]$:

$$u(T,\nu) = \frac{8\pi\nu^2}{c^3} \cdot \frac{h\nu}{\exp\left(\frac{h\nu}{kT}\right) - 1}.$$

Here, T denotes the temperature, ν the frequency, c the velocity of light, k is Boltzmann's and h Planck's constant. In its derivation, Planck had to assume that the energy of the hypothetical oscillator within the blackbody emits only the *discrete* energy $\varepsilon = h\nu$.

One key question is of course if this result established already the "quantisation" – from Latin *quantum* designating a sudden or discrete change without intermediate stages –, i.e. fundamental discreteness of matter and/or energy. Textbooks typically take this view (see, e.g. Tipler/Llewellyn 2009: 122 ff.). In fact, many accounts note that Planck was a "revolutionary against one's will" who took this bold step only reluctantly and in an act of desperation. However, even a reluctant and desperate revolutionary is a revolutionary. This standard narrative was challenged by Kuhn (1978). He questioned whether Planck in 1900 really assumed a quantisation of energy. There is compelling evidence for the claim that at that time Planck kept strict continuity notions and Kuhn argues that Planck proposed a "physically structured phase space rather than discontinuous energy levels" (Kuhn 1980: 187). This interpretation seems to be supported by Planck's (1900) following remark: "If the ratio [of the total energy E to the energy element ε] thus calculated is not an integer, we take for P an integer in the neighborhood." However, an isolated quote is certainly not enough to prove any point. Hence, whether Planck intended a quantisation at that point remains debateable and some have argued that he might have been undecided at that time (see Passon/Grebe-Ellis 2017 for further details and references on this debate).

The discovery of Planck's radiation law in 1900 is surrounded by another common myth, namely that it was the "ultraviolet catastrophe" of the "classical" Rayleigh-Jeans radiation law which prompted Planck's effort. While the Rayleigh-Jeans law provides an excellent description of the measured spectral energy density in the short frequency regime, it diverges in the high frequency, i.e. ultraviolet regime and thus generated a so-called anomaly. Now, Planck is typically interpreted to have found his law in response to this anomaly. This "anomaly-response" narrative cannot be true for a number of reasons. The most obvious one is that the Rayleigh-Jeans law was not published until 1905. We should mention that Rayleigh (1900) suggested this law qualitatively in a brief note but introduced an exponential damping factor immediately. Only in 1905 he returned to this work and provided the numerical factors which were missing in his earlier note. Here, however, he committed a small mistake and his result was too big by a factor of eight. James Jeans corrected this mistake immediately – thus the Rayleigh-Jeans law got its double name. The role of Jeans is discussed by McCaughan (1980) who suggests even an important origin of the quasi-historical anomaly-response narrative: It was Jeans himself who reported on the history of the radiation problem in 1914. With respect to the Rayleigh-Jeans law he stated: "This formula was given by Lord Rayleigh and the present author in 1900" (Jeans 1914). This wrong date also entered the 2nd edition of the report and made a textbook career thereafter.

Note, that these two misconceptions are mutually supportive: If there had been any awareness of an anomaly ("ultraviolet catastrophe") in 1900 this would have lent support to a revolutionary reaction ("quantisation"). Apparently, both elements were missing (see also Seth 2007).

Instead, until 1900 the heuristically derived Wien radiation law from 1896 was the generally accepted blackbody radiation law. This law (which differs in modern notation from Planck's law only by a "-1" in the denominator; the Planck constant h was so to say "hidden" in a coefficient called β) could describe the existing data – including the ultraviolet region – very well. However, its theoretical justification was unsatisfactory. Planck's ambition was to fill this gap and in 1899 he provided a theoretical derivation of Wien's law that seemed definite to him at that time. Subsequently some authors referred to this law even as "Wien-Planck law". It was in that paper from 1899 that Planck's constant was introduced although at that time it went by the name "b". But Planck clearly recognised its fundamental character and based the by now famous "Planck units" on it (together with c, G and k). Thus, technically, these Planck units have nothing to do with quantum physics. It was only when new experiments at the PTR explored the short frequency regime more thoroughly the Wien (or Wien-Planck) law failed, that Planck revised his derivation and came up with his modified law.

Summing up, according to current historiography Planck's work was not a reaction to an anomaly, and it is doubtful whether Planck intended any quantisation at all. Furthermore, for many years nobody in the community picked up on "quantisation" anyway. Kragh (1999: 63) notes pointedly: "If a revolution occurred in physics in December 1900, nobody seemed to notice it, least of all Planck. During the first five years of the century, there was almost complete silence about the quantum hypothesis, which somewhat obscurely was involved in Planck's derivation of the blackbody radiation law. The law itself, on the other hand, was quickly adopted because of its convincing agreement with experiment."

This is not the reaction one would expect if the very foundation of "classical physics" had just been shattered.

Part of the explanation lies in Planck's rather obscure derivation of his law. Another explanation is how claims of validity are negotiated within the scientific community: The problem of blackbody radiation was a rather specific one and the finer details of this work were of little concern to many; therefore Planck's law did not become the basis for questioning the validity of the present paradigm (to use Kuhn's terminology). Kragh (1999: 69 f.) points out that, in contrast, Albert Einstein's later application of Planck's distribution law to the problem of specific heat in 1907 played an important role in negotiations of validity claims since this was a more traditional field of physics. Given the applications of specific heat in physical chemistry, Walther Nernst became interested in quantum physics and initiated the first Solvay conference, which was held in Brussels in 1911. This event played a major role in broadening the interest in quantum physics within the physical community. Before that, however, Einstein extended the meaning of quantum theory to the radiation field.

4. Einstein and the light quantum

According to Kuhn and others, Einstein was among the first to take seriously the possibility that energy needs to be quantised. One of his many memorable papers in 1905 was called "On a Heuristic Point of View about the Creation and Conversion of Light" and in its introduction Einstein notes the following tension: While the electromagnetic theory of light assumes a continuum, the then current accounts of matter postulate discrete atoms. By applying thermodynamics to the radiation field, he argued that also radiation, i.e. light, behaves under certain conditions as if it consisted of "light quanta" whose energy is given by, in modern notation: $\varepsilon = h\nu$.

Many textbooks call this an application of Planck's earlier insight (see, e.g. Auletta et al. 2009: 12 f.; Tipler/ Llewellyn 2009: 129). Here we have another typical element of quasi- or Whig-history at work. As compelling as the "anomaly-response" narrative is the notion that scientific developments build on each other directly and form a conclusive and coherent sequence. However, Einstein did not apply Planck's law and did not even use the Planck constant h in his 1905 paper. Einstein's "light quantum paper" cites Planck's law in the first chapter only to show that it reduces to the Rayleigh-Jeans law in the long wave-length regime; actually, Einstein derived the Rayleigh-Jeans law independently for this purpose so that one might speak of the "Rayleigh-Einstein-Jeans law" instead. Given that another independent discovery is due to Lorentz (Kragh 1999: 66), it might even be called the "Rayleigh-Lorentz-Einstein-Jeans law". However, the so-called "first law of the history of science" states half-seriously that scientific results are never named after the first discoverer anyway.

Einstein's argument in 1905 is based solely on Wien's radiation law from 1896 and his "quantisation condition" reads $E = \frac{R\beta\nu}{N}$, with the gas constant R, the Avogadro number N, and β a constant from Wien's radiation law, i.e. put together this equation yields $E = h\nu$. All this indicates that Wien's law from 1896 is actually a "quantum law" already. Not only that Planck could introduce his constant already in 1899 when deriving Wien's law (as noted above), also Einstein's light quantum hypothesis derives from it. From today's perspective, this is of course trivial: Wien's law receives its current justification as the high-frequency limit ($h\nu \gg kT$) of Planck's law where some quantum effects dominate and the -1 in the denominator can be neglected.

Another misrepresentation of Einstein's 1905 light quantum paper is the scope of the work: It is usually presented as concerned mainly with the explanation of the photoelectric effect, i.e. the emission of electrons when electromagnetic radiation, such as UV light, hits a material. While Einstein discusses this possible application of his idea, this issue is rather mentioned in passing. However, his law of the photoelectric effect was a novel prediction and it earned him the Nobel Prize for 1921 (received in 1922). In a way the misleading name "photoelectric effect paper" for the work expresses the reception history accurately but fails to capture Einstein's own intend.

But the reception of the light quantum hypothesis was rather hostile in general. Even when Robert Andrews Millikan confirmed Einstein's law for the photoelectric effect in 1916, it was still not recognised by many, or, as Kragh (1999: 68) puts it: "What Millikan had confirmed was Einstein's equation, not his theory".

5. Bohr's atomic model

We have seen that historically the problem of blackbody radiation prompted what eventually became quantum physics. The specific property of this type of radiation is its universality: The functional form of the radiation spectrum does not depend on the material or shape of the body. However, if not a solid body is heated but a vapour of, say, some specific element, the resulting spectrum is all but universal. In fact, heated vapours emit specific “spectral lines”, i.e. radiation with specific frequencies, that characterize the corresponding substance uniquely. In 1913 Niels Bohr developed his atomic model which incorporated quantum concepts and could account for the spectrum of the simplest element, namely hydrogen. Only thereby “quantum physics” became “atomic physics”.

Bohr’s key inside was that electrons should only be allowed to occupy specific “stationary orbits”. In this sense their movement became “quantised” since only some *discrete* orbits were allowed. Additionally, the frequency of the spectral lines was related to the energy *differences* between these stationary states – and not to the orbital frequency of the electrons. This last claim was completely at odds with Maxwell’s electrodynamics which ties the frequency of radiation to the frequency of its source. A common textbook claim is that in Bohr’s model light quanta, so-called “photons”, are emitted or absorbed when electrons jump between their stationary states (see, e.g. Giancoli 2004: 789; Weinberg 2013: 7; Griffiths 2012: 76). However, Bohr was among the most vocal critics of the light quantum hypothesis and rejected it until 1924. Instead, Bohr assumed “classical” radiation with a frequency that obeyed his quantum condition. To view the Bohr model falsely as an application of the light quantum hypothesis is again a striking example for the whiggish tendency to read the present into the past. How then did the story of the light quantum continue? This brings us to the Compton effect.

6. The Compton effect

The almost complete acceptance of the light quantum hypothesis, so we are usually told (see, e.g. Auletta et al. 2009: 35 f.; Tipler/ Llewellyn 2009: 561), came with the discovery of the Compton effect, i.e. the wave-length shift in the scattering of X-rays from quasi-free electrons and its explanation as elastic scattering between photons and electrons in 1922/23. Brush (2015: 202) has pointed out that it is difficult to provide evidence for such acceptance claims and Kojevnikov (2002: 199) notes that there was already a “post-war wave of publications on light quanta” prior to Arthur Holly Compton’s work.

But even if most physicists had been persuaded by Compton’s landmark result in 1923 and had accepted the light quantum hypothesis, the advent of quantum mechanics in 1925/1926 (compare section 8) gave rise to a re-evaluation. When Compton was awarded the Nobel Prize in 1927 it was suspiciously not for the discovery *and explanation* of the effect named after him but only for its discovery. On the reception of the prize, Karl Manne Siegbahn – a Swedish physicist who had received the Nobel Prize for his work on X-ray scattering in 1924 – gave the presentation speech on behalf of the Nobel committee and remarked (quoted from Ekspong 1994: 101): “[T]he Compton Effect has, through the latest evolutions of the atomic theory, got rid of the original explanation based upon a corpuscular theory. The new wave mechanics, in fact, lead as a logical consequence to the mathematical basis of Compton's theory. Thus the effect has gained an acceptable connection with other observations in the sphere of radiation.”

Here, Siegbahn was referring to a series of works which explained the Compton Effect by basing all quantum effects on the electron, i.e. without introducing light quanta. These so-called “semi-classical” approaches were championed, e.g. by Erwin Schrödinger, Guido Beck, Gregor Wentzel, Oskar Klein and Walter Gordon (see Ekspong 1994: 103). Likewise, the photoelectric effect, i.e. the other example for the alleged “particle-like” properties of light, could be explained semi-classically, and this calculation also explains the angular distribution of the photoelectrons correctly. Interestingly, even when Oskar Klein and Yoshio Nishina (1929) calculated the differential cross-section for the Compton scattering (including the effects of spin and relativity) they applied a semi-classical approximation, i.e. treated the radiation classically.

It is rarely acknowledged that the “photon”, a term popularised by Compton after 1926, got its proper place only within quantum electrodynamic and remains a foreign body within non-relativistic quantum mechanics. Genuine quantum electrodynamic effects are, e.g. “spontaneous emission”, the “Lamb shift” or the “Casimir effect”. This is not the place to explain what these fancy names mean. It is sufficient to mention that these effects occur in the absence of any “classical” radiation field so that the semi-classical approach is blocked. Instead, these effects assume so-called “vacuum fluctuations” which are typical for quantum field theories.

All this vividly demonstrates that Einstein's light quantum differs decisively from today's photon because his ingenious speculation from 1905 was in important respects still too classical. In 1905 Einstein conceptualised "light quanta" as *localised* and *distinguishable* entities. Already in 1914 Paul Ehrenfest and Heike Kamerlingh Onnes could show that the last assumption is at odds with Planck's law. The first assumption, i.e. photons being localised, is at odds with quantum theory either. Note that a "wave function of the photon" with a probability interpretation in three-dimensional space does not exist (Peierls 1979: 10 f.) and that a position operator for the photon cannot be constructed (Newton/Wigner 1948). Today's photon, i.e. the photon of quantum electrodynamic, results from the quantisation of the electromagnetic field and its only "particle-property" is the discreteness of the eigenvalues of the occupation number operator (see, e.g. Passon et al. 2019). To call the photon a "particle" is rather jargon; hence, the talk of the "wave-particle duality" of light should be viewed as metaphorical or rather outmoded (cf. Mairhofer/Passon 2022).

All this bears important implications for philosophy of nature, especially by the issue of proper parthood relations: To some extent based on a distorted history, the common narrative draws a direct connection between Einstein's light quantum and the current photon. All too often, photons are presented as "particles of light", i.e. light is viewed as an aggregation of photons. However, the brief remarks above indicate that un-localised and indistinguishable objects do not qualify as "part-icles" – the hyphen indicates that this name supports implicit assumptions about the "part-hood" relation already, i.e. the "part-icle" as the "part" of some "whole". Instead, the parthood relation of modern physics is not aggregation but superposition: In quantum physics the different states of a system are described by vectors in some abstract state space, say ψ_1 and ψ_2. Given that the underlying equations are linear, we have the property of superposition, i.e. the expression $c_1\psi_1 + c_2\psi_2$, with c_i being arbitrary complex numbers, is a possible state likewise. This introduces features that are unknown in pre-quantum physics; especially the issue of "entanglement" is related to this (see Healey 2013; Passon/van Strien 2021).

Let us return to the historical thread of the discussion. As noted above, Ehrenfest and Kamerlingh Onnes (1915) are involved in the exciting pre-history of "indistinguishability" ("Fermi-Dirac" versus "Bose-Einstein statistics", as we would say today) which is rarely told in textbooks (see Passon/Grebe-Ellis 2017 and the references therein). One of the reasons why this line of research was continued only 10 years later – and this time prompted by a complete outsider like Satyendra Nath Bose – is surely related to the outbreak of the First World War. This disruptive event destroyed the scientific connections among the then hostile countries and shifted research interests understandably. It is curious to note that the impact of the First World War is so often neglected when dealing with the history of quantum physics, which leads us to a brief interlude.

7. Interlude: On political, economic and social context

After the First World War, Germany as well as Austria, Hungary and Bulgaria were excluded from the newly founded International Research Council (IRC) and German scientists were banned from all international conferences; this ban lasted until 1928 but became less effective over the years (Kragh 1999: 144 f.). In the beginning even the neutral countries were not admitted to the IRC. Kragh (1999: 144) argues that this reflects the fear on side of the Allied powers that neutral countries might vote for the admission of the former Central powers. And indeed, in 1925 the Dutch physicist Hendrik Antoon Lorentz – albeit unsuccessfully – suggested that this exclusion policy be annulled. When in 1919 the Swedish Academy of Science awarded the Nobel Prize to Max Planck, Johannes Stark and Fritz Haber, it was considered by many as an offensive act to rehabilitate German science.

The rise of Copenhagen to one of the centres of theoretical physics in the 1920s was also related to the fact that Denmark had remained neutral during the First World War and had provided a place where physicists from formerly hostile countries could meet again (Kojevnikov 2020: 39 ff.). This is not to deny that also a charismatic figure like Niels Bohr was needed to attract a group of gifted students and co-workers. In addition, Bohr kept friendly relations to his German and Austrian colleagues during and after the war, continued to publish in German journals and joined unauthorised conferences in Germany. All this led to Bohr being viewed as pro-German but apparently his scientific status prevented direct criticism (Kragh 1999: 147).

However, the boycott of German physics did probably more harm to the boycotting nations. For example, the Solvay conferences from 1921 and 1923 suffered severely from the absence of participants from the former Central powers. It is curious to note that during this time of political, social and economic crisis in Germany, its scientific research could maintain its leading position. Kragh (1999: 140) points out that exactly at this time of crisis, science was viewed by many as a "surrogate for the political and military power that, alas, no longer existed". Technically, it was the "Notgemeinschaft der deutschen Wissenschaft" (Emergency Society for German Science and Scholarship) that provided many of the necessary resources, funded by the federal government but also by donations from abroad, including *General Electric* and the *Rockefeller Foundation*.

8. The advent of quantum mechanics and the aftermath

In the early quantum theory, i.e. until 1925, one was typically applying "classical" descriptions or rather "pre-quantum" descriptions (compare section 2) which were supplemented by specific "quantum conditions". Many valid results were achieved only by an artistic application of Bohr's "correspondence principle", i.e. the demand of asymptotic agreement between the classical and the quantum description. Hence, any conceptual autonomy and consistency were missing – this is at least the harsh judgement made, e.g. by Max Jammer (1966: 196 ff.). Especially the failure to deal with complex many-electron systems made it apparent that this approach was incomplete.

In 1925 Werner Heisenberg managed to formulate the first unifying formalism of quantum physics, the so called "matrix mechanics" (Jammer 1966: 208 ff.). Soon after, Erwin Schrödinger found his "wave-mechanical" formulation (ibid.: 242 ff.; Mehra/Rechenberg 1982: 367 ff.). However, these formulations did not drop out of the blue and were firmly grounded in the former works which are typically, and somehow disrespectfully, labelled as "old quantum theory". As argued by Suman Seth, it is even an oversimplification to portray the mid-1920s simply as a "time of crisis" for the old quantum theory which called for a revolutionary and fresh start. It is true that some contemporaries used this expression, but for example Arnold Sommerfeld in 1929, i.e. after these events took place, could write: "The new development does not signify a revolution, but a joyful advancement of what was already in existence" (as quoted by Seth 2007: 47).

How then did "old" and "new" quantum theory relate to each other? In the case of Heisenberg, it was in particular his previous work on dispersion theory which shifted his focus from orbits to frequencies and amplitudes (Seth 2013: 838 ff.). His famous "Umdeutung"/ re-interpretation paper from 1925 renounced with such "orbits" as unobservable quantities. Schrödinger's wave mechanics from 1926, on the other side, applied ideas which had already been developed by Louis de Broglie in the early 1920s and had proven their usefulness in Einstein's quantum theory of the monatomic ideal gas from 1925 (Seth 2013: 843 ff.). However, there also is a sense in which matrix and wave mechanics offered something unexpected. It is curious to note that from the beginning the development of quantum physics was inspired by problems of radiation theory (black-body radiation, photo-electricity, atomic spectra, dispersion etc.) while the emerging theories of matrix and wave mechanics turned out to be theories of *matter*, i.e. not theories of *light* and *radiation*. The recent work of Blum and Jähnert (2022) is dealing with this tension and traces the "spirit of radiation theory" in these works.

Interestingly, the common view that the advent of matrix and wave mechanics was the reaction to a crisis of the "old quantum theory" *downplays* the continuity of the events, while our discussion so far was concerned with the *overemphasis* on continuity and coherence. In fact, our remarks in section 2 on the alleged crisis of "classical physics" indicate a similar tension. If Whig history is narrowly understood as the inclination to streamline the past development such that the current state of knowledge gets a sense of inevitability, this seems to imply that the crisis-narrative runs counter to it. This, however, would be a very restricted understanding of the term. More appropriate is the reading that Whig history is about applying present categories to evaluate past events. On this understanding, the overemphasis on crises is in the same spirit because viewed from hindsight, and only from hindsight, it is easy to spot the problems which turned out to be intractable in the framework of, e.g. "old quantum theory" – just like the very name "old quantum theory" is obviously a post hoc ascription (Seth 2007: 46).

According to the usual understanding both formulations, i.e. matrix mechanics and wave mechanics, were soon (i.e. 1926/27) recognised as mathematically equivalent, and they are now jointly referred to as "quantum mechanics". However, Muller (1997) has questioned this claim and calls it the "equivalence myth". He argues convincingly that at that time no equivalence was shown. According to his analysis, the proofs were not only missing rigour but in 1926 even no empirical and mathematical equivalence *existed* between these two still evolving frameworks. A true equivalence could only be established in the early 1930s by John von Neumann. (Note that Muller's claim has been challenged by Perovic 2008.)

However, Schrödinger's wave mechanics applied more familiar mathematical tools like linear partial differential equations and a continuous wave function usually denoted as ψ. It raised hopes that an intuitive ("anschaulich") understanding of quantum phenomena could be eventually regained. Now, what exactly is the meaning of the German "anschaulich" and "Anschaulichkeit" which figured prominently at that time? Muller (1997: 38) notes unsurpassably that "Anschaulichkeit" is untranslatable and means "a proper mixture of 'visualizability', 'intuitiveness', 'pictoriability', 'comprehensibility', 'intelligibility' and 'understandability'". To the German reader, this term also is immediately and closely associated with Kant's "Anschauungsformen" ("forms of intuition"), i.e. space and time. Thus, the demand of an "anschaulich" description of quantum phenomena is often supposed to mean "a description in space and time". But the complex valued wave function could not be interpreted as a physical wave in the three-dimensional space, and instead the probability interpretation of Born (1926) prevailed. Here, the expression $|\psi|^2$ is identified with the *probability* density to measure the system at a certain position.

While operationally well understood, interpretational issues have remained debated actually until today (Passon/van Strien 2021). An important early step was taken by Heisenberg in 1927 by introducing his uncertainty principle. According to his analysis, for example the position and the momentum of, say, an electron can only be defined within limits set by Planck's constant *h*. Heisenberg thus restored the meaning of such classical notions if one refrains from the joint use with unlimited precision. As the position can be identified in an obvious manner with a particle property and the momentum (via de Broglie's relation $\lambda = \frac{h}{p}$, suggested already in 1923) with a wave property, Heisenberg's uncertainty bears directly on the infamous wave-particle duality issue. However, also here this duality should be viewed at most as a vague metaphor. Given that for many-particle systems the wave function is defined on the high-dimensional "configuration space", it does not resemble any "wave" in the ordinary sense.

In the same year, Niels Bohr held his by now famous Como lecture (Bohr 1928) in which the notion of complementarity was introduced, i.e. the somewhat paradoxical relation between descriptions or properties which are jointly necessary although mutually exclusive. His key example for complementary is the relation between a causal and a space-time description of quantum phenomena, but in the Como lecture he also described wave- and particle-like properties as complementary. This example is still very popular among those who mention complementarity at all. This is unfortunate as it is rather easy to come up with situations in which particle- and wave-like properties display jointly, say electron diffraction with such a feeble source that the detection of discrete events is possible. This problem was apparently also noticed by Bohr who famously discussed the double-slit experiment along similar lines in his later writings. Presumably for this reason Bohr did not use wave-particle duality as an example for complementarity since 1935 (Held 1994).

The Como lecture was delivered in September 1927, and in October of the same year the experts – this time also including Einstein – convened at the 5th Solvay conference in Brussels.[3] This conference has gone down in history as the place where Bohr and Einstein had fierce disputes over the interpretation of quantum mechanics. According to the common narrative, Bohr and his "Copenhagen Interpretation" prevailed, and the interpretational issues were settled, if not definitively, then at least for a long time. For example, Jammer (1966: 361) explains: "Even Einstein, defeated but not convinced, had to admit that from the logical point of view

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[3] The proceedings of this conference have been translated into English (and annotated) only quite recently by Guido Bacciagaluppi and Antony Valentini (2009).

the theory and its complementarity interpretation form a consistent system of thought. For the next two and a half decades, the Copenhagen interpretation was the only accepted interpretation of quantum mechanics – and for the majority of physicists it is so even today. It may therefore be said that the search for a general consistent theory of the mechanics of atoms, a search which, as we have seen, was ushered in at the first Solvay Congress of 1911, found its successful completion and finale in the fifth Solvay Congress of 1927."

However, already more than 30 years ago John Heilbron (1988) has pointed out that the reception of the so-called "Copenhagen Interpretation" was more complicated. Don Howard (2004) calls it even a myth that there is any coherent and unified "Copenhagen Interpretation" which developed in the late 1920s, given that the alleged representatives of this view, say Bohr, Pauli, Heisenberg, Born and von Neumann, had dissenting opinions on many important issues (cf. Camilleri 2009). Howard argues that the Copenhagen Interpretation is actually an invention of the mid-1950s, for which Heisenberg is chiefly responsible. In addition, various other physicists and philosophers, e.g. Karl Popper, David Bohm and Paul Feyerabend, promoted such a view in the service of their own agendas. Until today, the "Copenhagen Interpretation" is used as an excellent straw man in philosophical debates.

There are a couple of debates on "external factors" which we can mention only briefly. The first concerns the issue whether the milieu of the Weimar republic had any influence on the reception and development of quantum physics. Already in 1932, Erwin Schrödinger raised the question whether science is milieu-dependent ("Ist die Naturwissenschaft milieubedingt?"). Schrödinger (1932) made a compelling case for subjective factors even in science. Briefly, he argued that while science results meet the most stringent demands on objectivity, at least the very question of research interest introduces subjective factors. He observes further, that for example the ancient Greek science is firmly rooted in the general cultural context and establishes a similar connection between modern physics and the then current cultural tendencies. On this general level this statement is rather uncontroversial. However, this claim was much sharpened in the early 1970s by the historian Paul Forman who, by the way, did his PhD under Kuhn. He noted that in the German post-war period holism, intuition, relativism and existentialism were very much on the agenda. Forman argued that German physicists, because of the influence of the "Weimar Zeitgeist", were predisposed toward an acausal and anti-materialistic physics like the new quantum mechanics (Kragh 1999: chapter 10). However, while there are good reasons to question any strong influence of "Weimar Zeitgeist" on the genesis of quantum theory (Kragh 2002: 153 f.), the "Forman-thesis" and the resulting debate were enormously influential and shaped the following *science studies* (Carsen et al. 2011).

There are other examples for the influence of "external factors" on physics research. For example, Peter Galison (1998) investigated the impact of doing war related work with its specific pressure to get quick results (rather than a deep understanding). Galison has coined the term "modular culture" for this development, and he traces the influence of this style even after the war was over. As another example for a "cultural influence" one may mention David Kaiser's award-winning book "How the Hippies saved physics" (Kaiser 2011). Although rather a popular science book, the eminent historian of physics Kaiser makes a compelling case for the influence of the New Age and hippie movement in the 1970s on fundamental quantum physics. More specifically, Kaiser traces the origin of some of quantum information science (e.g. the no-cloning theorem) on the engagement of a rather obscure "Fundamental Fysiks group" in Berkeley.

Coming back to the earlier history: While in the late 1920s important philosophical questions remained open, quantum mechanics reached a phase of operational and mathematical consolidation – marked, e.g. by the advent of canonical textbooks like Paul Dirac's "The Principles of Quantum Mechanics" (1930) or John von Neumann's "Mathematische Grundlagen der Quantenmechanik" (1932). The next stage was devoted to application, interpretation and generalisation. In certain respect, we are still in this phase.

9. Summary

The danger of judging a past event by present standards has been acknowledged by historians for a long time and it is meanwhile typically avoided in scholarly texts. However, whether Butterfield's analysis went deep enough is open to debate and, as argued by Wilson and

Ashplant (1988), the danger of present-centredness in the writing of history is still present (no pun intended). In any event, whiggish narratives, which easily turn into a hagiography or a celebration of the present, are still very common in the history of science on textbook level, as I have illustrated in this entry. The reasons are manifold and some of them are certainly open to speculation. As noted already by Whitaker (1979) textbooks treat the history not as an end in itself but in order to foster the understanding of current theories. This implies the danger to provide rather a rational reconstruction of the historical events and to emphasise concepts which are still important today. Accordingly, a naïve view of science as cumulative process follows. This holds at least if the corresponding concepts are still in use. If, however, a conceptual change has occurred, these narratives tend to overemphasise discontinuity.

But there are also more interesting reasons for these narratives – more interesting especially from the philosophical and sociological point of view. As indicated already by the quotations from Feynman and Lederman (section 1), scientists tend to view the present state of scientific knowledge not as an *arbitrary* but a *superior* vantage point (Wilson/Ashplant 1988: footnote 6). They typically subscribe to the view of science as self-correcting and its development as a linear history of progress. In that case the allegation of whiggishness seems less severe; one may even argue that under this assumption a present-centred history of science provides a rather accurate account of the development since it simply avoids contingent factors which turned out to be irrelevant anyway. Leon Lederman's remark about "filtering out the noise of real life" (see section 1) is a perfect illustration of this attitude.

One way to look at this is as follows: The history of a research field is a means to give meaning and identity to a community. Historical narratives of science – especially when authored by scientists themselves – are doing "boundary work" in the sense introduced by the sociologist of science Thomas F. Gieryn (1983; 1999). In brief, "boundary work" describes the efforts of scientists to create and shape the image of science to contrast it favourably to non-scientific activities. Part of this is the common self-perception that scientists are "neutral fact finders", and that science is a purely objective enterprise, driven by experimental evidence and logical rigor only. Brush (1974) remarked that the closer examination of the history of science compromises this image severely. While scientific results meet the most stringent demands on objectivity, science is at the same time a human enterprise that displays institutional set-ups, personal interests, specific cognitive faculties and other subjective and contingent factors. Moreover, scientific results are eventually open to discussion and revision.

In brief: The question what counts as an appropriate historiography of science relates to core issues in the philosophy of science, namely the question "what science is", i.e. the demarcation problem, and whether science is self-correcting and converges towards the truth. A historiography which assumes the objectivity of science from the outset will lose the ability to review this assumption and turn into a "self-serving and self-confirming" enterprise (Cunningham 1988: 369). Nevertheless, as noted above, this anachronistic assumption of an inevitable scientific progress is typically avoided in current historical debates. The extreme opposite position of social constructionism is no longer maintained by many either, since even its founders have started to distance themselves from it (cf. Latour 1992). A pluralistic understanding of science falls between these two extremes (cf. Soler et al. 2016; Vagelli et al. 2021; Chang 2021) and seems worthy of consideration.[4]

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[4] We note in passing that scientistic tendencies in the philosophy of science are also based on the assumption that science provides the only means to gain secure knowledge about nature (cf. Esfeld 2017: 10). Thus, to acknowledge the fundamental importance of contingent elements in its development is compromising this stance either.

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