A brief history of quantum-mechanical ideas

Jean Louis Van Belle, *Drs, MAEc, BAEc, BPhil*

6 May 2020

Summary

In this paper, we pick some less well-known contributions of great minds to the history of ideas from the proceedings of the Solvay Conferences. We hope to show there was nothing inevitable about the new physics winning out. In fact, we suggest modern-day physicists may usefully go back to some of the old ideas – most notably the idea that elementary particles do have some shape and size – and that they should try somewhat harder to explain intrinsic properties of these particles – such as their angular momentum and magnetic moment – in terms of classical physics. The contributions which we discuss are those of Ernest Rutherford, Joseph Larmor, Hendrik Antoon Lorentz and Louis de Broglie.

Contents

The battle of ideas 1

Rutherford's idea of an electron 6

- Introduction 6
- Rutherford's contributions to the 1921 Solvay Conference 8
- Ernest Rutherford's idea of an electron in 1921 10
- Thinking backward and forward 11

Joseph Larmor's idea of an electron 12

The views of H.A. Lorentz on the new physics 14

Louis de Broglie's mistake 17

A brief history of quantum-mechanical ideas

The battle of ideas

Dr. Oliver Consa traces all of the nonsense in modern physics back to the Shelter Island (1947), Pocono (1948) and Oldstone (1949) Conferences.[1] However, the first Solvay Conference that was organized after WW II (1948) – whose theme was 'Elementary Particles'[2] – was also part of the watershed between good and bad ideas, and Niels Bohr and Robert Oppenheimer – both on the side of the bad ideas, unfortunately – pretty much dominated it. Bohr does so by providing the introductory lecture '*On the Notions of Causality and Complementarity*', while Oppenheimer's paper on the '*Electron Theory*' sets the tone for subsequent Solvay Conferences—most notably the one that would consecrate quantum field theory (QFT), which was held 13 years later (1961).

When going through the proceedings, it is quite obvious that Paul Dirac is pretty much the only one asking Oppenheimer and the other new physics *gurus* critical questions.[3] Hence, while there are many junctions in the history of ideas, this discussion between Oppenheimer and Dirac on the 'Electron Theory' paper in 1948 is surely worth mentioning. We think both Oppenheimer and Dirac commit historical blunders here.

Indeed, Oppenheimer's use of perturbation theory to arrive at some kind of 'new' model of an electron based on Schwinger's version of quantum field theory is not only very confusing but also does not add any value to classical approaches.[4]

---

[1] Oliver Consa, *Something is rotten in the state of QED*, February 2020.

[2] The library of the Free University of Brussels (VUB/ULB) has digitized the proceedings of all Solvay Conferences from 1911 to 1973. The reader who is fluent in French is, therefore, in a position to verify all references. We are not aware of a freely available English translation of all of the papers and discussions, which add up to several thousands of pages. However, you may be luckier than us when googling: if you find it, please do let us know.

[3] Albert Einstein is often mentioned as an opponent of Bohr and Heisenberg in the context of these Solvay Conferences but, truth be told, Einstein's interventions in these Conferences are limited, and they do not suggest much opposition to the new theories. If there were 'heated discussions' between Heisenberg and Einstein, or 'ardent debate' – or anything such as a 'Bohr-Einstein controversy' – then it is not documented in the formal proceedings. Quotes attributed to either Einstein ("God does not play dice!") or Heisenberg ("Stop telling God what to do!") may have been made during casual conversation – or, more probably, decades later – but surely *not* during the formal proceedings. Einstein may have felt he should help H.A. Lorentz to come to mutually agreed upon conclusions during those Conferences – rather than add to the discussions or debates – because, being the very young scientist he was at the time, he would not have been invited to the first Solvay Conference without the strong support of H.A. Lorentz. Einstein not interfering all that much may explain why he was such a consistent presence: he was, effectively, a member of the Solvay scientific committee from the very first conference (1911) – representing, in typical style, a country (Austria, *not* Germany) rather than an institution or just being a member in some personal capacity – till 1948. He did not attend the 1921 Conference, however, to mark his disagreement with the decision of the committee to *not* invite German scientists. He was also *not* a member of the 1951 scientific committee but the reason for that might well be age or a lack of interest, of course: Einstein was 72 years in 1951, and would die four years later (1955).

[4] The anomalous magnetic moment of an electron is not anomalous at all and can be explained using classical physics (see p. 20 of our paper on classical quantum physics).

However, Dirac also chooses to disappear into the mist of history by continuing to defend his totally *un*defendable electron equation.[5] Indeed, it is very intriguing that Dirac does *not* follow through on his own conclusion:

> "Only a small part of the wavefunction has a physical meaning. We now have the problem of picking out that very small physical part of the exact solution of the wave equation."[6]

It is particularly weird because Dirac had already clearly stated his *guts* intuition in regard to what to pick out 15 years before. Indeed, at the occasion of his Nobel Prize lecture (1933), he said this:

> "The variables give rise to some rather unexpected phenomena concerning the motion of the electron. These have been fully worked out by Schrödinger. It is found that an electron which seems to us to be moving slowly, must actually have a very high frequency oscillatory motion of small amplitude superposed on the regular motion which appears to us. As a result of this oscillatory motion, the velocity of the electron at any time equals the velocity of light. This is a prediction which cannot be directly verified by experiment, since the frequency of the oscillatory motion is so high and its amplitude is so small. But one must believe in this consequence of the theory, since other consequences of the theory which are inseparably bound up with this one, such as the law of scattering of light by an electron, are confirmed by experiment." (Paul A.M. Dirac, *Theory of Electrons and Positrons*, Nobel Lecture, December 12, 1933)

What Schrödinger and Dirac had stumbled upon is, quite simply, the *physical* ring current that generates the magnetic moment of the electron. The British physicist and chemist Alfred Lauck Parson had already proposed such ring current model for the electron back in 1915, when the magnetic properties of the electron had not yet been experimentally proved. Arthur Compton had actually jumped on it and used it to write a series of articles on the size and shape of an electron. He did so from 1917 to 1921[7] and, as we will see in a moment, the ring current model of an electron was regularly referred to by other prominent contemporaries, including Ernest Rutherford, H.A. Lorentz and other warriors in the battle of ideas then.

Unfortunately, all would be swept away by the abstract theories of the new generation of quantum physicists led by the younger Heisenberg with strong support from the older Niels Bohr. In a rather desperate attempt to save classical physics by destroying it[8], they simply refused to accept an electron might have some size and shape: they preferred to model the motion of electrons and other elementary particles, and the atom itself, by thinking of it as a pointlike *wavicle* obeying abstract wave equations.

This development has, obviously, led nowhere: we are a hundred years later now, and physicists are still utterly unable to explain the basic properties of elementary particles such as their charge radius, their

[5] We are not quite sure what Dirac wanted to model with his wave equation but wave equations usually model the properties of the medium, which is the vacuum here. If the vacuum has any properties, then they have been modelled already by Maxwell's equations.

[6] Proceedings of the 8th Solvay Conference (1948), *Discussion du rapport de M. Oppenheimer*, p. 282.

[7] The history here is well documented by the Wikipedia article on Parson's model, which is also referred to as the toroidal ring or *magneton* model.

[8] When everything is said and done, one may say that Schrödinger, Heisenberg and other young wolves basically adapted classical Hamiltonian mechanics by fitting quantum-mechanical operators and wavefunctions into it.

angular momentum, and their magnetic moment. Likewise, they are also unable to explain basic diffraction and interference phenomena from first principles.

We are *not* joking here. Just as an example, we may mention that we were briefly in touch with the PRad experimenters who put an end to the rather ridiculous 'proton radius puzzle' by re-confirming the previously established 0.83-0.84 range for the effective charge radius of a proton.[9] We had sent them our own classical back-of-the-envelope calculation of the Compton scattering radius of a proton based on the ring current model, which is in agreement with these measurements and courteously asked what alternative theories they were suggesting.[10] Their spokesman replied this:

> "There is no any theoretical prediction in QCD. Lattice [theorists] are trying to come up [with something] but that will take another decade before any reasonable number [may come] from them."

One wonders if there is actually any real interest in solving these puzzles. The PRad team may have been nominated for a Nobel Prize in Physics—we surely hope so because, in contrast to other Nobel Prize laureates, the PRad team surely deserve one—but isn't it rather incongruous to finally firmly establish the size of a proton in some expensive experiment while, at the same time, admit that protons should not have any size according to mainstream theory?

We should not be surprised, of course. Wave equations – linear differential equations – are useful if one wants to model the properties of a *medium* in which *physical* waves (think of water or sound waves, for example[11]) propagate. In fact, whether or not you believe there must be some medium in which electromagnetic waves propagate – an *aether*, relativistic spacetime, or the *vacuum*[12] – we do have

---

[9] See: https://www.jlab.org/prad/collaboration.html.

[10] See p. 15-16 of our paper on classical physics. The email exchange was beginning of February 2020.

[11] For a standard textbook derivation of the wave equation, see Chapter 47 of Richard Feynman's *Lectures* on Mechanics, Radiation and Heat.

[12] The term suggest it cannot have any properties: if the vacuum is nothing, then it cannot have any properties, right? However, Maxwell's equations – and the discovery of the *absolute* velocity of light (light always travels at lightspeed (299792458 m/s, *exactly*) – do actually model properties, including the property that an influence or a *signal*, including light, cannot travel faster than the speed of light. Many scientists therefore continued to refer to the concept of an *aether* long after Einstein's relativity theory had already been well established (Joseph Larmor, whom we will quote shortly, is one of them, and he does so to refer to these properties). We may usefully re-quote Robert B. Laughlin here:

> "The word 'ether' has extremely negative connotations in theoretical physics because of its past association with opposition to relativity. This is unfortunate because, stripped of these connotations, it rather nicely captures the way most physicists actually think about the vacuum. […] The modern concept of the vacuum of space, confirmed every day by experiment, is a relativistic ether. But we do not call it this because it is taboo."

We may also usefully quote from Lorentz' answer to Larmor as he mentions the concept in the discussions during the 1921 Solvay Conference:

> "As for the aether, even the physicists who still talk about it have stripped the concept of anything it might have in common with matter. I was a believer in an immobile aether myself but I realize that, because of relativity, we cannot talk about any force acting on the aether. However, I still think of the aether as the seat of electromagnetic energy (*"le siège de l'énergie électromagnétique"*)."

wave equations for it: Maxwell's equations, to be precise. However, when modeling simple or complex systems that have some *internal structure*—atoms (think of Schrödinger's wave equation here), electrons (think of Dirac's wave equation), or protons (which is what some others tried to do, but we will let you do some googling here yourself), Maxwell's equations have their limits. They are, after all, to be used to model the properties of the medium, which is the vacuum here. That's what they should be used for. Hence, trying to use them to explain how an atomic system – consisting of electrons, protons and neutrons that all interact[13] – is bound to lead to abstract generalizations that cannot possibly reflect all of the degrees of freedom in the system that is being analyzed.

Let us go back to that 1948 Solvay Conference, where Dirac challenges the new approach based on perturbation theory (as presented by Oppenheimer) by making the following comment:

> "All the infinities that are continually bothering us arise when we use a perturbation method, when we try to expand the solution of the wave equation as a power series in the electron charge. Suppose we look at the equations without using a perturbation method, then there is no reason to believe that infinities would occur. The problem, to solve the equations without using perturbation methods, is of course very difficult mathematically, but it can be done in some simple cases. For example, for a single electron by itself one can work out very easily the solutions without using perturbation methods and one gets solutions without infinities. think it is true also for several electrons, and probably it is true generally : we would not get infinities if we solve the wave equations without using a perturbation method."

Of course, Dirac is very much aware of another problem too: the wavefunctions that come out as solutions dissipate away. Real-life electrons – or any real-life matter-particle, really – do not do that. In fact, we refer to them as being *particle-like* because of their integrity—an integrity that, we believe, is modeled by the Planck-Einstein relation.[14] Oppenheimer knew that too obviously (if Dirac's theory

[13] Think of the *coupling* of their magnetic moment, for example, which explains the hyperfine structure (including the Lamb shift) of the spectrum of atoms. See our paper on the Lamb shift in this regard.

[14] Prof. H. Pleijel, the Chairman of the Nobel Committee for Physics of the Royal Swedish Academy of Sciences in 1933, sums this rather particularly property of the new 'matter waves' rather well in his ceremonial speech for the 1933 Nobel Prize, which was awarded to Heisenberg for nothing less than "the creation of quantum mechanics":

> "Matter is formed or represented by a great number of this kind of waves which have somewhat different velocities of propagation and such phase that they combine at the point in question. Such a system of waves forms a crest which propagates itself with quite a different velocity from that of its component waves, this velocity being the so-called group velocity. Such a wave crest represents a material point which is thus either formed by it or connected with it, and is called a wave packet. […] As a result of this theory on is forced to the conclusion to conceive of matter as not being durable, or that it can have definite extension in space. The waves, which form the matter, travel, in fact, with different velocity and must, therefore, sooner or later separate. Matter changes form and extent in space. The picture which has been created, of matter being composed of unchangeable particles, must be modified."

This should sound familiar to you. The problem is this: this is everything *but* true! Real-life particles – electrons or atoms traveling in space – do *not* dissipate: matter does *not* change form and extent in space! It is most remarkable that nonsense like this has survived critical thought for almost a hundred years now!

As for using the Planck-Einstein relation in particle modeling, we refer the reader to our classical or realist interpretation of quantum mechanics.

worked, there would have been no need for a new theory) and so Dirac immediately adds the following:

> "We would not get infinities if we solve the wave equations without using a perturbation method [but] if we look at the solutions which we obtain in this way, we meet another difficulty: namely we have the run-away electrons appearing. Most of the terms in our wave functions will correspond to electrons which are running away[15], in the sense we have discussed yesterday and cannot correspond to anything physical. Thus nearly all the terms in the wave functions have to be discarded, according to present ideas. Only a small part of the wave function has a physical meaning."[16]

Again, this small part of the wavefunction is, of course, the real electron, and it is the ring current or *Zitterbewegung* electron! It is the trivial solution that Schrödinger had found, and which Dirac mentioned very prominently in his 1933 Nobel Prize lecture.[17] The other part of the solution(s) is (are), effectively, bizarre oscillations which Dirac here refers to as 'run-away electrons'. With the benefit of hindsight, one wonders why Dirac did not see what we see now.[18] In any case, the gist of the matter is that Dirac was rather easy prey in this 1948 discussion—no match at all for the brilliant Oppenheimer. Dirac's defense of his meaningless wave equation is, therefore, stubbornly foolish[19]: he clearly loved it too much to jettison it.

"*Kill your darlings*" is a common piece of advice given to unexperienced writers. Dirac should have thought about it, but then it is hard to criticize a theory for which one got a Nobel Prize, isn't it?[20] Here

---

[15] See our remarks in footnote 14: wavefunctions dissipate away. The matter-particles they purport to describe obviously do *not*.

[16] See pp. 282-283 of the report of the 1948 Solvay Conference, *Discussion du rapport de Mr. Oppenheimer.*

[17] See the quote from Dirac's 1933 Nobel Prize speech in this paper.

[18] One of our correspondents wrote us this however: "Remember these scientists did not have all that much to work with. Their experiments were imprecise – as measured by today's standards – and tried to guess what is at work. Even my physics prof in 1979 believed Schrödinger's equation yielded the *exact* solution (electron orbitals) for hydrogen." Hence, we are probably too harsh in our judgment here.

[19] I think it is significant that Paul A.M. Dirac was no longer invited nor even mentioned in the 1951 Solvay Conference. Niels Bohr and Wolfgang Pauli made subsequent remarks that clearly show Paul Dirac was the only one who actually dared to challenge what had, by then, become mainstream theory.

[20] The Wikipedia article on Paul Dirac mentions Dirac boasting to the younger Feynman at a conference: "I have an equation. Do you have one too?" We do not know if this story is true, nor are we aware of the context in which this might have been said. Whatever the story, it is obvious from his *Principles of Quantum Mechanics* that Dirac believed the task of physicists was to model what he referred to as the 'equations of motion' for elementary particles. He showed no inclination to think about their possible shape, size or other fundamental properties. I am grateful to a fellow amateur physicist who pointed out that, in 1962, Dirac did finally think about some electron model. He got it published by the Royal Society but it attracted little attention, and rightly so because it hardly explained anything. We may quote the abstract:

> "It is proposed that the electron should be considered classically as a charged conducting surface, with a surface tension to prevent it from flying apart under the repulsive forces of the charge. Such an electron has a state of stable equilibrium with spherical symmetry, and if disturbed its shape and size oscillate. The equations of motion are deduced from an action principle and a Hamiltonian formalism is obtained. The energy of the first excited state with spherical symmetry is worked out according to the Bohr-Sommerfeld method of quantization, and is found to be about 53 times the rest-energy of the electron. It is suggested

again, the Nobel Prize Committee for Physics may actually have consecrated bad theory that, as a result of the award, became difficult – if not impossible – to criticize or backtrack on.

We think it is significant that Paul A.M. Dirac was no longer invited nor even mentioned in the 1951 Solvay Conference which, as a result, was much more consensual.[21] We will, therefore, not comment on it—nor will we comment on the later conferences (not in this paper, at least). The battle for ideas was over by then anyway. The question we should ask ourselves today is this: was this unavoidable? **Was all of this a logical historical evolution?** Was there a *choice*, in other words?[22]

We think there was—and we also think there still *is* a choice. We think the good ideas lost out and that, after 100 years of a non-theory or a non-explanation[23], we should go back to them. Let us, therefore, effectively try to trace them back. Of course, the proceedings of the Solvay Conferences are several thousands of pages and we will, therefore, limit ourselves to what we think of as crucial papers and interventions. We will start with a fragment from Rutherford's presentation on *The Structure of the Atom* at the 1921 Solvay Conference.

Rutherford's idea of an electron

Introduction

The New Zealand-born Ernest Rutherford came to be known as the father of nuclear physics. He was the first to provide a reliable estimate of the order of magnitude of the size of the nucleus. To be precise, in the 1921 paper – which is the one we will talk about here – he came up with an estimate of about 15 fm for massive nuclei, which is pretty much the current estimate for the size of an uranium nucleus. In light of the rather primitive equipment at the time, this is more than impressive!

Rutherford's experiments also helped to significantly enhance the Bohr model of an atom, culminating - just before WW I started - in the Bohr-Rutherford model of an atom.[24] This Bohr-Rutherford model of an atom explained the (gross structure of the) hydrogen spectrum perfectly well, but it could not explain its finer structure—read: the orbital *sub*-shells which, as we all know now (but not very well then), result from the different *states* of angular momentum of an electron and the associated magnetic moment.

Let us quickly illustrate the issue by the two diagrams below, which we copied from Feynman's *Lectures*. As you can see, the idea of subshells is not very relevant when looking at the *gross* structure of the hydrogen spectrum because the energy levels of all subshells are (very nearly) the same. However, the

> that this first excited state may be considered as a muon. The present theory has no electron spin, so it cannot agree accurately with experiment."

Frankly, this sounds like a bit of a joke to us: is this *all* what Dirac could come up with—in 1962?

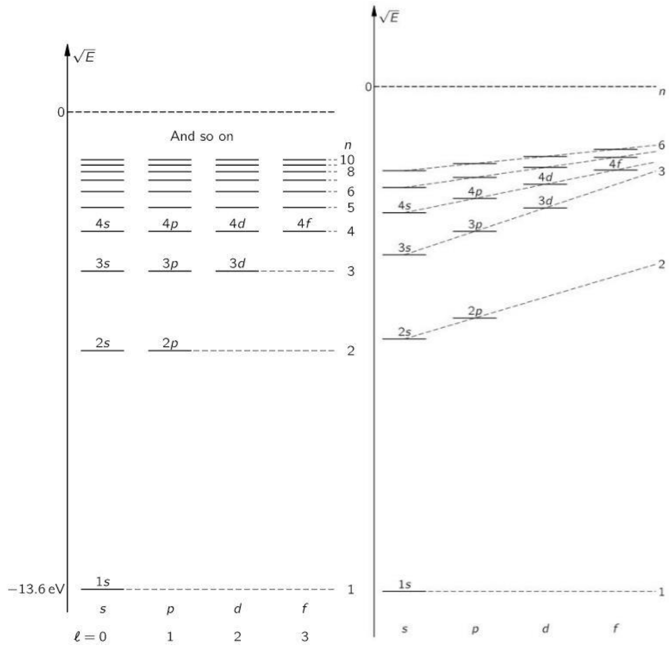
[21] Apart from Paul Dirac, Niels Bohr and Wolfgang Pauli commented on Oppenheimer's paper, but their remarks clearly show Paul Dirac was the only one who actually dared to challenge what had, by then, become mainstream theory.

[22] We request the reader to *not* try to connect the concepts of determinism and indeterminism at the atomic level to the same concepts at the level of societal debates.

[23] See our paper on the difference between a scientific theory and an explanation.

[24] See: E. Rutherford, *Phil. Mag. 27*, 488.

diagram on the right-hand side shows that the Bohr model of an atom—which is nothing but an exceedingly simple application of the E = *h·f* equation[25]—cannot explain the splitting of lines for a lithium atom. Nor can it explain the splitting of spectral lines when we apply a stronger or weaker magnetic field while exciting the atoms so as to induce emission of electromagnetic radiation.



Rutherford was well aware of this problem and, as we will see in a moment, by 1921 he probably already had some ideas on how to fix it—because a significant part of his paper talks about the magnetic moment of electrons, even if the evidence of such magnetic moment was rather rough at the time!

However, using the *new physics*, Schrödinger would pre-empt him with his wave equation—which is why Feynman and other modern physicists claim this equation is "*the most dramatic success in the history of the quantum mechanics*" or, more modestly, a "*key result in quantum mechanics*" at least![26]

Such dramatic statements are wildly exaggerated. First, an even finer analysis of the emission spectrum (of hydrogen or whatever other atom) reveals that Schrödinger's wave equation is also incomplete: the hyperfine splitting, the Zeeman splitting (anomalous or not) in a magnetic field, or the (in)famous Lamb shift are to be explained not only in terms of the magnetic moment of the electron but also in terms of the magnetic moment of the nucleus and its constituents (protons and neutrons)—or of the *coupling* between those magnetic moments.[27] The coupling between magnetic moments is, in fact, the only *complete and correct* solution to the problem, and it cannot be captured in a wave equation. Indeed, we should mention once again that simple mathematical tools such as second-order differential equations

---

[25] See p. 4-6 of our paper on classical quantum physics.

[26] See Chapter 19 of Feynman's *Lectures* on Quantum Mechanics, from which we also copied the two textbook illustrations. The other quote is from the Wikipedia article on Schrödinger's equation.

[27] See our paper on the Lamb shift here.

are – quite simply – *not sophisticated enough* to capture the complexity of the atomic *system*.

Moreover, even the *solution* to the wave equation – the *wavefunction* – does not adequately represent the physical reality of a spinning particle. Indeed, as we pointed out previously[28], the current *convention* in regard to the use of the imaginary unit (*i*) in the wavefunction does *not* capture the spin *direction* and, therefore, makes abstraction of the *direction* of the magnetic moment too! **The wavefunction, therefore, models theoretical spin-zero particles which do *not* exist.** In short, we cannot hope to represent anything *real* with wave equations and wavefunctions.

Secondly, we would think the following question is even more important: what use is an 'explanation' in terms of a wave equation or a wavefunction if we cannot explain what the wave equation or the wavefunction actually *represents*? As Feynman famously writes: "Where did we get [Schrödinger's equation] from? Nowhere. It is not possible to derive it from anything you know. It came out of the mind of Schrödinger, invented in his struggle to find an understanding of the experimental observations of the real world."[29]

Our best guess is that it may, somehow, model the (local) diffusion of energy or mass densities as well as the spherical and non-spherical geometries of the electron orbitals. We explored such interpretations in our very first paper(s) on quantum mechanics.[30] However, the truth is that such interpretations are probably incomplete or plain irrelevant because of what we mentioned a few times already: wave equations are probably *not* the tools we need to describe simple or complex systems that have some internal structure—atoms (think of Schrödinger's wave equation here), electrons (think of Dirac's wave equation), or protons (which is what some others tried to do but, as mentioned before, we will let you do some *googling* here yourself).

We will let you think about this because we need to move on. We need to get back to the matter at hand here, which is Rutherford's idea of an electron back in 1921. What can we say about it?

Rutherford's contributions to the 1921 Solvay Conference

From what you know, from what we write above, and from the title of Rutherford's 1921 paper (*La Structure de l'Atome*) you will understand that Rutherford's research focus was *not* on electrons: his prime interest was in explaining the atomic structure and in solving the mysteries of *nuclear* radiation—most notably the emission of *alpha*- and *beta*-particles as well as highly energetic *gamma*-rays by unstable or radioactive nuclei. In short, the nature of the electron was not his prime interest.

However, this intellectual giant was, of course, very much interested in whatever experiment or whatever theory that might contribute to his thinking, and that explains why, in his contribution to the 1921 Solvay Conference—which materialized as an *update* of his seminal 1914 paper on *The Structure of the Atom*—he devotes considerable attention to Arthur Compton's work on the scattering of light from electrons which, at the time (1921), had not even been published yet (Compton's seminal article on (Compton) scattering[31] was published in 1923 only).

---

[28] See, for example, our paper on the difference between a scientific theory, a calculation and an explanation.

[29] Chapter 16 of Feynman's *Lectures* on Quantum Mechanics.

[30] See, for example, our paper on a possible geometric explanation of Schrödinger's equation.

[31] Arthur Compton, A Quantum Theory of the Scattering of X-rays by Light Elements, Phys. Rev. **21**, 483 (1 May

It is also very interesting that, in the very same 1921 paper—whose 30 pages are more than a multiple of his 1914 article[32] and later revisions of it (see, for example, the 1920 version of the same article, which actually has wider circulation on the Internet)—Rutherford also offers some short reflections on the *magnetic properties of electrons* while referring to Parson's ring current model which, in French, he refers to as "*l'électron annulaire de Parson*."

It is, of course, somewhat strange that we should translate Rutherford's paper for the 1921 Solvay Conference back in English—as we are sure the original paper must have been translated from English to French rather than the other way around. However, it is what it is, and so here we will do what we feel we should do: translate some of Rutherford's remarks during the 1921 Solvay Conference on the state of research regarding the electron at that time.

These remarks on the electron are, in fact, part of a larger piece on the emission of β particles by radioactive nuclei which, as it turns out, are nothing but high-energy electrons (or their anti-matter counterpart—positrons). In fact, we should—before we proceed—draw attention to the fact that the physicists at the time had no clear notion of the concepts of protons and neutrons. This is, indeed, another remarkable historical contribution of the 1921 Solvay Conference because, as far as we know, this is the first time Rutherford talks about the *neutron* hypothesis, so let us quickly say something about it.

First, we should note that it is quite remarkable that Rutherford does *not* advance the neutron hypothesis to explain the atomic mass of atoms combining what we know think of as protons and neutrons (Rutherford regularly talks of a mix of 'positive and negative electrons' in the nucleus—neither the term proton or neutron was in use at the time) but *as part of a possible explanation of nuclear fusion reactions in stars or stellar nebulae*. This is, indeed, his response to a question during the discussions on Rutherford's paper on the possibility of nuclear synthesis in stars or nebulae from the French physicist Jean Baptiste Perrin who, independently from the American chemist William Draper Harkins, had proposed the possibility of hydrogen fusion just the year before (1919):

> "We can, indeed, think of enormous energies being released from hydrogen nuclei merging to form helium—much larger energies than what can come from the Kelvin-Helmholtz mechanism.[33] I have been thinking that the hydrogen in the nebulae might come from particles which we may refer to as 'neutrons': these would consist of a positive nucleus with an electron at an exceedingly small distance (*"un noyau positif avec un électron à toute petite distance"*). These would mediate the assembly of the nuclei of more massive elements. It is, otherwise, difficult to understand how the positively charged particles could come together against the repulsive force that pushes them apart—unless we would envisage they are driven by enormous velocities."

Rutherford is immediately requested to elaborate his point by the Danish physicist Martin Knudsen, who asks him this follow-up question: "What's the difference between a hydrogen atom and this neutron?"

1923)

[32] Ernest Rutherford, The Structure of the Atom, Phil. Mag. 27, 488 (1914).

[33] The reader can *google* what this is about.

Rutherford's answers is this: "In a neutron, the electron would be *very* much closer to the nucleus."

In light of the fact that it was only in 1932 that James Chadwick would *experimentally* prove the existence of neutrons (and positively charged protons), we are, once again, deeply impressed by the foresight of Rutherford and the other pioneers here: the predictive power of their theories and ideas is, effectively, truly amazing by any standard—including today's. We should, perhaps, also add that we fully subscribe to Rutherford's intuition that a neutron should be a composite particle consisting of a proton and an electron[34]—but that is a different discussion altogether.

We must come back to the matter at hand, and that is the electron. Before we proceed, however, we should highlight one other contextual piece of information here: at the time, very little was known about the nature of α and β particles. We *now* know that *beta*-particles are electrons, and that *alpha*-particles combine two protons and two neutrons (as such, they are nothing but a helium nucleus). That was *not* known in the 1920s, however: Rutherford and his associates could basically only see positive or negative particles coming out of these radioactive processes.[35] Hence, it would seem Rutherford is not quite sure if the beta-particle is, effectively, an electron. This further underscores how good his intuitions were, and how much knowledge he was able to gain from rather limited sets of data.

Ernest Rutherford's idea of an electron in 1921

We thank the reader for patiently waiting. So here is the translation of some of Rutherford's remarks on the electron as part of his paper on atomic structure. Needless to say, the *italics*, boldface and additions between [brackets] are not Rutherford's but ours, of course.

> "We may think the same laws should apply in regard to the scattering [*"diffusion"*] of α and β particles.[36] However, we see marked differences. Anyone who has carefully studied the photographs from the Wilson cloud chamber of *beta*-particles will note the trajectories show a regular *curvature.* Such curved trajectories are even more obvious when they are illuminated by X-rays. Indeed, A.H. Compton noted that these trajectories seem to end in a converging helical path turning right or left. To explain this, ***Compton assumes the electron acts like a magnetic dipole whose axis is more or less fixed, and that the curvature of its path is caused by the magnetic field*** [from the (paramagnetic) materials that are used].
>
> Further examination would be needed to make sure this curvature is not some coincidence, but the general impression is that the hypothesis may be quite right. We also see similar curvature and helicity with α particles in the last millimeters of their trajectories.[37]

---

[34] See our paper on the possible nature of protons and neutrons.

[35] Needless to say, they could infer quite a lot from these trajectories.

[36] Earlier in his paper, Rutherford noted that, based on the scattering patterns and other evidence, the force around the nucleus must respect the inverse square law near the nucleus—moreover, it must also do so *very* near to it. To be precise, Rutherford mentions the experiments confirm the inverse square law remains valid to as close as 40 fm (4×10^{-12} cm) from the pointlike nucleus.

[37] We should note here that α-particles are, obviously, also charged particles and, hence, they may also have some magnetic moment. However, we think Rutherford's remark in regard to α particles also following a curved or helical path in the "last millimeters" of their path must be exaggerated or over-enthusiastic: the order of magnitude of the magnetic moment of protons and neutrons is much smaller and, in any case, they tend to cancel

> The idea that an electron has magnetic properties is still sketchy and we would need new and more conclusive experiments before accepting it as a scientific fact. However, ***it would surely be natural to assume its magnetic properties would result from a rotation of the electron***. *Parson's ring electron model* ["*électron annulaire*"] *was specifically imagined to incorporate such magnetic polarity* ["*polarité magnétique*"].
>
> A very interesting question here would be to wonder whether such rotation would be some intrinsic property of the electron or if it would just result from the rotation of the electron in its atomic orbital around the nucleus. Indeed, James Jeans usefully reminded me any asymmetry in an electron should result in it rotating around its own axis at the same frequency of its orbital rotation.[38]
>
> We should also wonder if an electron might acquire some rotational motion from being accelerated in an electric field and if such rotation, once acquired, would persist when decelerating in an(other) electric field or when passing through matter. If so, some of the properties of electrons would, to some extent, depend on their past." (Ernest Rutherford, 1921 (3rd Solvay Conference), italics and boldface added).

Each and every sentence in these very brief remarks is wonderfully consistent with the ring current model of an electron and/or more modern-day modelling of electron behavior[39]—modern *non-mainstream* modeling of electrons we should say, of course, but then the addition is superfluous because mainstream physicists stubbornly continue to pretend electrons have no internal structure nor *physical* dimension, so they do not have any electron model. While, in light of the numerous *experimental* measurements of the effective charge radius as well as of the dimensions of the physical space in which photons effectively interfere with electrons, such mainstream assumptions seem completely ridiculous, this is unfortunately the rather sad state of physics today.

Thinking backward and forward

We think that Rutherford and others would have been able to adapt their model of an atom to better incorporate the magnetic properties not only of electrons but also of the nucleus and its constituents (protons and neutrons) *if they would have had the time and space*. Unfortunately, scientists at the time seem to have been swept away by the new ideas and charisma of Bohr, Heisenberg and others, as well as by the mathematical brilliance of the likes of Sommerfeld, Dirac, and Pauli, of course! They, therefore, decided to take a new road instead. We now know that new road has not led us very far. We concur with Oliver Consa's scathing but essentially correct appraisal of the current *sorry state* of physics:

> "QED should be the quantized version of Maxwell's laws, but it is not that at all. QED is a simple addition to quantum mechanics that attempts to justify two experimental discrepancies in the Dirac equation: the Lamb shift and the anomalous magnetic moment of the electron. The reality

each other out. Also, because of the rather enormous mass of α particles (read: helium nuclei) as compared to electrons, the effect would probably not be visible in a Wilson cloud chamber.

[38] The reader can easily imagine or verify this for himself: think of an asymmetric object going around in a circle and returning to its original position. In order to return to the same orientation, it must rotate around its own axis one time too!

[39] Think, for example, of the Dirac-Kerr-Newman electron model of Alexander Burinskii.

> is that QED is a bunch of fudge factors, numerology, ignored infinities, hocus-pocus, manipulated calculations, illegitimate mathematics, incomprehensible theories, hidden data, biased experiments, miscalculations, suspicious coincidences, lies, arbitrary substitutions of infinite values and budgets of 600 million dollars to continue the game. Maybe it is time to consider alternative proposals. Winter is coming."[40]

We suggest we should just go back where we went wrong: it may be warmer there, and thinking both backward as well as forward must be a much more powerful problem-solving technique than relying only on expert guessing on what linear differential equation(s) might give us the *S*-matrix![41]

Let us get back to the Solvay Conference. Rutherford was an intellectual giant, but he was just one of the many presenters at the 1921 Solvay Conference. What about the others? Again, we cannot possibly give a complete overview of the Conference but we may single out one or more lesser known scientists who were present there. Joseph Larmor – whom we know from the *Larmor* frequency of precession of atomic or elementary magnetic dipoles – was one of them, so let us see what he had to say back in 1921.

Joseph Larmor's idea of an electron

Joseph Larmor is surely *not* among the more famous participants in the Solvay Conferences. He only joined the 1921 Conference, together with Charles Glover Barkla and others, and his one and only substantial intervention there is limited to some remarks and questions following a presentation by H.A. Lorentz on the *Theory of Electrons*, during which Lorentz highlights all of the issues in regard to what was then supposed to be the understanding of what an electron actually *is* (which, in my not-so-humble-view, is still pretty much the state of our *current* understanding of it). We find his one intervention – and Lorentz' reply to it – very interesting though[42]:

> "I understand that Mr. Lorentz was given the task to give an overview of how electrons behave inside of an atom. That requires an overview of all possible theories of the electron. That is a highly worthwhile endeavor which, in itself, would already justify the holding of this Conference. However, Mr. Lorentz might have paid more attention to the viewpoint that the electron has some structure, and that its representation as a simple distribution of electric charge can only

[40] Oliver Consa, Something is rotten in the state of QCD, February 2020.

[41] An *S*-matrix (scattering matrix) relates all likely or possible initial and final states of some system or process: it shows us *what* happens, but it does not tell us *how* things happen. As such, we do not think much of it: it is a calculation instead of an explanation. See our paper on the difference between a theory, a calculation and an explanation.

[42] We should, again, note that Joseph Larmor's intervention was probably in English and it is, therefore, somewhat strange that we should translate back from the French to English. As for H.A. Lorentz, he was fluent in French, English, and German (and his native language, of course—Dutch), so he may also have replied in English. We may mention, in this regard, that Marcel de Broglie had already mobilized his younger brother, Louis de Broglie, for the very first Solvay Conferences—if only to help with translation. As the historian Jagdish Mehra notes, he was only nineteen years old when attending the first Solvay Conference and it made a deep impression on him. He would later write: "With all the ardor of my youth, I was swept away by my enthusiasm for the problems discussed and I resolved to devote all my efforts to understanding the true nature of the mysterious quanta that Max Planck had introduced ten years earlier."

be provisional: electrons explain electricity, but electricity does not explain electrons. However, the description of an electron in terms of a charge distribution is, for the time being, all we can imagine. In the past, we thought of the *atom* as an indivisible unit – a fundamental building block – and we imagined it as a swirling ring. That idea is gone now, and the electron has now taken the place of the atom as an indestructible unit. All we can know about it, is how it influences other bodies. If this influence is transmitted all across the *aether*, we need to be able to express the relations between the electron and the aether[43], or its force field in the space that surrounds it. It may have other properties, of course, but physics is the science that should analyze the influence or force of one body upon others.

> The question we should raise here is whether or not an electron *formed by a perfectly uniform current ring* can grab onto the aether in a physical sense, and how it does so if its configuration does not change." (Joseph Larmor, 1921, italics added)

Larmor then talks about the (possible) use of the energy-momentum tensor to address the latter question, which is a very technical discussion which is of no concern to us here. Indeed, the question on how to use tensors to model how an electron would interact with other charges or how it would create an electromagnetic field is, effectively, a rather standard textbook topic now.[44] What grabbed my attention here was, effectively, not the technicality of the question in regard to the *exact* machinery of the electromagnetic force or field. It was **Larmor's description of the electron as a perpetual or persistent current ring** (the French reference to it is this: *un electron formé par un courant annulaire parfaitement uniforme*), and his language on it, which indicates he thought of it as a rather obvious and natural idea!

In other words, his intervention strongly suggests that Parson's 1915 toroidal ring model – the precursor to Schrödinger's *Zitterbewegung* model and modern-day ring current models – was apparently pretty well established at the time! In fact, Rutherford's lecture on the *Structure of the Atom* at the 1921 Conference further confirms this, as he also talks about Parson's *électron annulaire* (ring electron) and the apparent magnetic properties of the electron. Of course, Larmor's belief that the electron was not pointlike should, in fact, not surprise us in light of his rather famous work on the quantum-mechanical precession of the magnetic moment of an electron, but we actually were not aware of Joseph Larmor's own views in regard to its possible *reality.* In fact, we are only guessing here but his rather strong views on its reality may explain why the scientific committee − which became increasingly dominated by scientists in favor of the Bohr-Heisenberg interpretation of physical reality (basically saying we will never be able to understand it) − did *not* extend an invitation to Larmor to attend the all-important Solvay conferences that would follow the 1921 Conference and, most notably, the 1927 Conference that split physicists between realists and... Well... Non-realists, we should say, I guess. :-)

Lorentz' immediate reaction to Larmor mentioning the idea of a *swirling ring* (in French: *un anneau tourbillon*), which is part of his reply to Larmor's remarks, is equally interesting:

[43] See footnote 12 for some remarks on the idea of the *aether* and the vacuum. We also mention Lorentz' response to this topic there.

[44] In case you'd be interested, you can check my blog on it or, else, (re-)read Chapters 25, 26 and 27 of Feynman's *Lectures* on electromagnetism.

> "There is a lot to be said for your view that electrons are discontinuities in the aether. [...] The energy-momentum formulas that I have developed should apply to all particles, with or without structure. The idea of a rotating ring [in French: *anneau tournant*] has a great advantage when trying to explain some issues [in the theory of an electron]: **it would not emit any electromagnetic radiation. It would only produce a magnetic field in the immediate space that surrounds it.** [...]" (H.A. Lorentz, 1921, boldface and italics added)

Isn't that just *great*? Lorentz' answer to Larmor's question surely does *not* solve all of the problems relating to the interpretation of the electron as a current ring, but it sure answers that very basic question which proponents of modern quantum mechanics usually advance when talking about the so-called failure of classical physics: electrons in some electron orbital in an atom should radiate their energy out, but so they do *not*. Let us actually quote from [45] Feynman's *Lectures* on Quantum Mechanics here:

> "Classically, the electrons would radiate light and spiral in until they settle down right on top of the nucleus. That cannot be right."

Surely You're Joking, Mr. Feynman! Here is the answer of the classical quantum theorists: superconducting rings of electric current do not radiate their energy out either, do they?

Let us continue, however. Next, we would like to present the views of the scientist who took most of the burden of actually organizing the Solvay Conferences on his shoulders and who, among all of these intellectual giants, may well be the 'giant of giants': Hendrik Antoon Lorentz. We actually already let him talk in 1921 above, so let us move forward in time to see what he had to say at the even more (in)famous Solvay Conference of 1927.

The views of H.A. Lorentz on the new physics

It is part of scientific lore that the 1927 Solvay Conference was the real battlefield on new physics, and that the generals on the two sides were Heisenberg and Einstein, respectively. However, while Heisenberg is, effectively, all over the place—presenting, together with Max Born, the main paper (simply titled *'Quantum Mechanics'*[46]) at the Conference as well as actively participating in discussions on this and the other papers[47]—the proceedings of the conference reveal that Einstein hardly intervened.

The same reports of the proceedings also reveal that 'battlefield stories' such as Heisenberg telling Einstein to "stop telling God what to do" or - vice versa - Einstein declaring "God doesn't play dice" are what they are: plain gossip or popular hear-say. Neither Heisenberg nor Einstein ever said that—or not at the occasion of the 1927 Solvay Conference, at least! Instead, we see very nuanced and very deep

[45] See: Chapter 2, Section 4 of Feynman's *Lectures* on Quantum Mechanics

[46] Max Born and Werner Heisenberg, *La Mécanique des Quanta*, 5th Solvay Conference (1927).

[47] The other key players are Louis de Broglie, who presents his views on the matter-wave, and Erwin Schrödinger, who presents a paper on wave mechanics. Niels Bohr then sums everything in the last intervention (*The Quantum Postulate and the New Atomistic Theory*, in French: *Le postulat des quanta et le nouveau développement de l'atomistique*), which elicits the response from H.A. Lorentz that we have translated in this paper. The papers of W.L. Bragg and Arthur Compton are interesting as well, but clearly did not do much to stop the new movement.

philosophical statements—on both sides of the so-called 'divide' or 'schism'. We think the intervention of the Dutch scientist Hendrik Antoon Lorentz stands out among of these. In fact, his intervention is quite unlike his other remarks and interventions. It stands out because it is somewhat emotional and, with the benefit of hindsight once more, because it is also very *historical*: H.A. Lorentz – who was, without any doubt, clearly *the* driving force behind all of the pre-WW II Solvay Conferences till 1927 – would, effectively, die a few months later.[48] In fact, the 1927 conference proceedings have both the sad announcement of his demise as well his interventions—such was the practice of actually physically printing stuff at the time.

GENERAL DISCUSSION OF THE NEW IDEAS ON CAUSALITY, DETERMINISM AND PROBABILITY[49]

M. LORENTZ: "I would like to draw your attention to the difficulties in these theories. We are trying to represent phenomena. We try to form an image of them in our mind. Till now, we always tried to do using the ordinary notions of space and time. These notions may be innate; they result, in any case, from our personal experience, from our daily observations. To me, these notions are clear, and I admit I am not able to have any idea about physics without those notions. The image I want to have when thinking physical phenomena has to be clear and well defined, and it seems to me that cannot be done without these notions of a system defined in space and in time.

To me, the electron is a particle which, at any moment, must be at some specific point in space, and if I think it should be somewhere else at the next moment, then I need to be able to think of its trajectory, which is a line in space. And if that electron meets an atom and penetrates it and if it, after several adventures, leaves that atom, then I need to have some theory in which that electron conserves its individuality. In other words, I actually think of a trajectory of the same electron *within* the atom.

Now, it may be difficult to develop such theory but, *a priori*, this should not be impossible. Hence, I would think that, in your new theories, you would still have electrons. It is possible, of course, that these electrons may be subject to transformations. I am willing to think of electrons as some kind of cloud, but even then I would look for the event that produces these transformations. If one would like to tell me that such (re)search is not allowed by invoking some principle, then I would be much annoyed. I would probably reply that, what we cannot do now, we may be able to do at a later point. Even we abandon our old ideas, we should still be able to translate things back to those old ideas. Hence, I would like to conserve the old ideal: to talk about the things that happen in this world in clear and well-defined images. I am willing to accept new theories, but only on the condition that they should allow me to translate things back to these clear and well-defined images.

I am not so well acquainted with the ideas that have just been revealed, but I would think of them like this. Let us effectively take the case of an electron leaving an atom, and let us assume there has been an emission of a photon.[50] We should then consider the wave system that corresponds to the atom and the

---

[48] Lorentz died of a very trivial cause: *erysipelas*, commonly known as St Anthony's fire. Hence, this may well have been one of his very last public statements—if not *the* last.

[49] This general discussion follows all of the presentations by Heisenberg, Schrödinger, Bohr, etcetera. See the proceedings of the Conference itself: even if the reader is not familiar with French, he will be able to understand the titles of the papers and recognize their authors.

[50] Lorentz uses the terms photon and a quantum of light interchangeably.

electron, respectively. We will have a new system before and after the emission of a photon. This wave system may then be described by some ψ function in a (mathematical) space with multiple dimensions, and it would probably satisfy some differential equation. The new wave mechanics should then effectively describe this function before and after the emission of the light quantum, and we can imagine other experiences, such as a beam of particles inside a Faraday cylinder, for example, and [but] we would again have to take into account the individuality of the electrons, and also of the photons. Now, I can understand that the $\psi\psi^*$ expression[51] would effectively give us the probability of the electron or the photon being in some clearly defined space and, perhaps, that we would have to content ourselves with this information.

However, the examples that have been given by Mr. Heisenberg tell me that I should not hope to be able to learn anything more—that this probability is all that I can possibly hope to learn from the experiment. However, I would think such notion of probability would come at the end, and as a conclusion, to the theoretical considerations of any experiment—*not* as an *a priori* axiom.

Hence, while I am prepared to admit that the conditions of an experiment are such that, from a practical point of view, we would have indeterminism, I would still keep my deterministic belief in fundamental phenomena, of which we cannot talk, perhaps. Perhaps a deeper mind could know the movements of these electrons? Can we not keep determinism as an object of faith? *Why do we have to elevate indeterminism to a philosophical principle?*"

[…]

As my translation of the latter paragraph involves some interpretation, we would like to quote the same in the original French language:

> "Je pense que cette notion de probabilité [in the new theory] serait à mettre à la fin, et comme conclusion, des considérations théoriques, et non pas comme axiome *a priori,* quoique je veuille bien admettre que cette indétermination correspond aux possibilités expérimentales. Je pourrais toujours garder ma foi déterministe pour les phénomènes fondamentaux, dont je n'ai pas parlé. Est-ce qu'un esprit plus profond ne pourrait pas se rendre compte des mouvements de ces électrons. Ne pourrait-on pas garder le déterminisme en en faisant l'objet d'une croyance? *Faut-il nécessairement ériger l' indéterminisme en principe*?"

What a beautiful statement: **why should we elevate indeterminism to a philosophical principle?**

Let us move to the next: we think we should, perhaps, say a few words about the man who invented the matter-wave: *Comte* Louis de Broglie.[52] We all know that Louis de Broglie was the first to postulate the wave nature of matter – and of the electron in particular – in his 1924 PhD thesis, for which he was praised by Albert Einstein. His 1927 paper for the Solvay Conference is, therefore, highly significant. So where did he go wrong?

---

[51] Lorentz refers to the standard interpretation of the absolute square of the wavefunction.

[52] Louis de Broglie would become the 7th *Duc* de Broglie when his older brother *Maurice de Broglie* – the 6th Duc de Broglie – passed away. Maurice de Broglie was an active participant in the earlier Solvay Conferences and was, therefore, in a good position to introduce his younger brother to physics and all of the great minds of the time.

Louis de Broglie's mistake

The ring current model of an electron incorporates the wavelike nature of an electron: the frequency of the oscillation is the frequency of the circulatory or oscillatory motion (*Zitterbewegung*) of the pointlike electric charge. Hence, the *intuition* of Louis de Broglie that an electron must have a frequency was, effectively, a stroke of genius. However, here too we wonder why he did not consider to further build on Parson's ring current model of an electron, especially in light of that nasty property of a wave *packet*: it dissipates away. Real-life electrons stay together.

Let us have a closer look at his paper for the 1927 Solvay Conference, titled *La Nouvelle Dynamique des Quanta*, which we may translate as *The New Quantum Dynamics*. The logic is, by now, well known: we think of the particle as a wave *packet* composed of waves of slightly different frequencies ν_i.[53] This leads to a necessary distinction between the *group* and *phase* velocities of the wave. The group velocity corresponds to the classical velocity *v* of the particle, which is often expressed as a fraction or *relative* velocity $\beta = v/c$.

The assumption is then that we know how the *phase* frequencies ν_i are related to *wavelengths* λ_i. This is modeled by a so-called dispersion relation, which is usually written in terms of the *angular* frequencies $\omega_i = 2\pi\cdot\nu_i$ and the *wave numbers* $k_i = 2\pi/\lambda_i$.[54] The relation between the frequencies ν_i and the wavelengths λ_i (or between angular frequencies ω_i and wavenumbers k_i) is referred to as the *dispersion* relation because it effectively determines if and how the wave packet will disperse or dissipate. Now, we already mentioned we need a wave *equation* only to model the properties of the *medium* – which, supposedly, is the *vacuum* here – through which these waves travel.

We can, for example, use the Schrödinger equation *without* the Coulomb term[55]:

$$\frac{\partial\psi}{\partial t} = i\frac{\hbar}{2m_{eff}}\nabla^2\psi = \frac{\partial\psi}{\partial t} = i\frac{\hbar}{m}\nabla^2\psi$$

What is m_{eff}? It is the concept of the *effective* mass of an electron which, in our ring current model, corresponds to the relativistic mass of the electric charge as it *zitters* around at lightspeed.[56] The

---

[53] We invite the reader to check our *exposé* against de Broglie's original 1927 paper in the Solvay Conference proceedings. We will try to stick closely to the symbols that are used in this paper, such as the *nu* (ν) symbol for the frequency.

[54] The concept of an angular frequency (*radians* per time unit) may be more familiar to you than the concept of a wavenumber (*radians* per distance unit). Both are related through the velocity of the wave (which is the velocity of the *component* wave here, so that is the *phase* velocity v_p):

$$v_p = \nu_i \cdot \lambda_i = \frac{\omega_i}{2\pi}\cdot\frac{2\pi}{k_i} = \frac{\omega_i}{k_i}$$

[55] For Schrödinger's equation in free space or the same equation with the Coulomb potential see Chapters 16 and 19 of Feynman's *Lectures* on Quantum Mechanics respectively. Note that we moved the imaginary unit to the right-hand side, as a result of which the usual *minus* sign disappears: $1/i = -i$.

[56] See Dirac's description of Schrödinger's *Zitterbewegung* of the electron for an explanation of the lightspeed motion of the charge. For a derivation of the $m = 2m_{eff}$ formula, we refer the reader to our paper on the ring current model of an electron, where we write the effective mass as $m_{eff} = m_\gamma$. The *gamma* symbol (γ) refers to the photon-like character of the charge as it zips around some center at lightspeed. However, unlike a photon, a charge carries charge. Photons do not.

question now is: are we talking one wave or many waves? Let us first make the analysis for one wave only, assuming that we can write ψ as some *elementary* wavefunction $\omega = a \cdot e^{i\theta} = a \cdot e^{i \cdot (k(x - \omega t)}$. Now, two complex numbers $a + i \cdot b$ and $c + i \cdot d$ are equal if, and only if, their real and imaginary parts are the same, and the $\partial\psi/\partial t = i \cdot (\hbar/m) \cdot \nabla^2\psi$ equation amounts to writing something like this: $a + i \cdot b = i \cdot (c + i \cdot d)$. Remembering that $i^2 = -1$, you can then easily figure out that $i \cdot (c + i \cdot d) = i \cdot c + i^2 \cdot d = -d + i \cdot c$. The $\partial\psi/\partial t = i \cdot (\hbar/m) \cdot \nabla^2\psi$ wave equation therefore corresponds to the following *set* of equations[57]:

- $Re(\partial\psi/\partial t) = -(\hbar/m) \cdot Im(\nabla^2\psi) \Leftrightarrow \omega \cdot \cos(kx - \omega t) = k^2 \cdot (\hbar/m) \cdot \cos(kx - \omega t)$
- $Im(\partial\psi/\partial t) = (\hbar/m) \cdot Re(\nabla^2\psi) \Leftrightarrow \omega \cdot \sin(kx - \omega t) = k^2 \cdot (\hbar/m) \cdot \sin(kx - \omega t)$

It is, therefore, easy to see that ω and k must be related through the following dispersion relation[58]:

$$\omega = \frac{\hbar \mathrm{k}^2}{\mathrm{m}} = \frac{\hbar c^2 \mathrm{k}^2}{\mathrm{E}}$$

So far, so good. In fact, we can easily verify this makes sense if we substitute the energy E using the Planck-Einstein relation $E = \hbar \cdot \omega$ and assuming the wave velocity is equal to c, which should be the case if we are talking about the same vacuum as the one through which Maxwell's electromagnetic waves are supposed to be traveling[59]:

$$\omega = \frac{\hbar \mathrm{k}^2}{\mathrm{m}} = \frac{\hbar c^2 \mathrm{k}^2}{\mathrm{E}} = \frac{\hbar c^2 \mathrm{k}^2}{\hbar \omega} = \frac{c^2 \mathrm{k}^2}{\omega} \Leftrightarrow \frac{\omega^2}{\mathrm{k}^2} = \frac{(2\pi f)^2}{(2\pi/\lambda)^2} = (f\lambda)^2 = c^2 \Leftrightarrow c = f\lambda$$

We know need to think about the question we started out with: one wave or many component waves? It is fairly obvious that if we think of many component waves, each with their own frequency, then we need to think about different values m_i or E_i for the mass and/or energy of the electron as well! How can we motivate or justify this? The electron mass or energy is known, isn't it?

This is where the *uncertainty* comes in: the electron may have some (classical) velocity or momentum for which we may not have a *definite* value. If so, we may assume different values for its (kinetic) energy and/or its (linear) momentum may be possible. We then effectively get various *possible* values for m, E and p which we may denote as m_i, E_i and p_i respectively. We can, then, effectively write our dispersion relation and, importantly, *the condition for it to make physical sense* as:

$$\omega_i = \frac{\hbar \mathrm{k}^2}{\mathrm{m}_i} = \frac{\hbar c^2 \mathrm{k}_i^2}{\mathrm{E}_i} = \frac{\hbar c^2 \mathrm{k}_i^2}{\mathrm{E}_i} = \frac{\hbar c^2 \mathrm{k}_i^2}{\hbar \omega_i} = \frac{c^2 \mathrm{k}_i^2}{\omega_i} \Leftrightarrow \frac{\omega_i^2}{\mathrm{k}_i^2} = c^2 \Leftrightarrow c = f_i \lambda_i$$

We refer the reader to previous papers on how one can now relate the uncertainties in the (kinetic)

---

[57] We invite the reader to double-check our calculations. If needed, we provide some more detail in one of our physics blog posts on the geometry of the wavefunction.

[58] If you *google* this (check out the Wikipedia article on the dispersion relation, for example), you will find this relation is referred to as a *non-relativistic* limit of a supposedly relativistically correct dispersion relation, and the various authors of such accounts will usually also add the 1/2 factor because they conveniently (but *wrongly*) forget to distinguish between the *effective* mass of the *Zitterbewegung* charge and the total energy or mass of the electron as a whole.

[59] We apologize if this sounds slightly ironic but we are actually astonished Louis de Broglie and so many others shamelessly assume superluminal speeds for wave velocities, even if it is a *phase* rather than a group velocity.

energy and the (linear) momentum of our particle using the relativistically correct energy-momentum relation and also taking into account linear momentum is a *vector* and, hence, we may have uncertainty in both its *direction* as well as its magnitude. Such explanations also provide for a geometric interpretation of the *de Broglie* wavelength.[60]

We come to the following conclusions:

1. If there is a matter-wave, then it must travel at the speed of light and *not*, as Louis de Broglie suggests, at some superluminal velocity.
2. If the matter-wave is a wave *packet* rather than a single wave with a precisely defined frequency and wavelength, then such wave packet will represent *our limited knowledge* about the momentum and/or the velocity of the electron. The uncertainty is, therefore, not inherent to Nature, but to our limited knowledge about the initial conditions.

We may now refer the reader to de Broglie's 1924 thesis, his 1927 Solvay paper (even the original would have been in French, unfortunately) or whatever other English-language article or paper from de Broglie that the reader is able to *google*. He or she can then compare the logic above with de Broglie's logic, and understand where Louis de Broglie disappears into the mist. Indeed, in physics – perhaps even more than in other sciences – Aristoteles' maxim applies too: *a small mistake in the beginning is a big one in the end*.[61]

It is, therefore, quite essential to go back where de Broglie went wrong, rather than try to fix the problem later—as Louis de Broglie would later try to do himself by introducing the concept of pilot waves and other metaphysical nonsense. Here again, we think the Nobel Prize Committee could have used some common sense before awarding him the Nobel Prize two years later (in 1929, to be precise): **wavefunctions may dissipate, but real-life particles definitely do *not***.[62] Hence, a matter-wave that quickly dissipates in space cannot possibly represent anything real. At best, it can model our own uncertainty *in regard to reality*, but not reality itself.

We may, in this regard, also usefully note here that Heisenberg initially preferred to use the German term *Ungenauigkeit* to describe the apparent randomness in our mathematical description of Nature. *Ungenauigkeit* translates as imprecision and it is a concept that is valid in classical mechanics as well. Indeed, it is just what it is: an imprecision, as opposed to the weird metaphysical quality which Heisenberg would later claim it to be and which, without any precise definition, physicists now refer to as 'uncertainty'. We may, therefore, use Lorentz's question as an affirmative statement: **there is no need whatsoever to elevate indeterminism to a philosophical principle**.

In fact, without the assumption of determinism at the most elementary level of analysis, science would basically not be science: we would relegate it to the realm of *beliefs*.

---

[60] See pp. 72-74 and 94-97 of our popular book (*The Emperor Has No Clothes: A Realist Interpretation of Quantum Mechanics*). We have more detailed mathematical analysis in our other papers.

[61] This is usually quoted from Aquinas *De Ente et Essentia* (Quia parvus error in principio magnus est in fine) but Thomas Aquinas actually quotes 'The Philosopher', which is Aristoteles.

[62] One should note that small initial uncertainties quickly become much larger ones: this is a common, in fact, the foundation of chaos theory.