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"A dedicated missionary". Charles Galton Darwin and the new quantum mechanics in Britain

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ABSTRACT

In this paper I discuss the work on quantum physics and wave mechanics by Charles Galton Darwin, a Cambridge wrangler of the last generation, as a case study to better understand the early reception of quantum physics in Britain. I argue that his proposal in the early 1920s to abandon the strict conservation of energy, as well as his enthusiastic embracement of wave mechanics at the end of the decade, can be easily understood by tracing his ontological and epistemological commitments to his early training in the Cambridge Mathematical Tripos. I also suggest that Darwin's work cannot be neglected in a study of quantum physics in Britain, since he was one of very few fellows of the Royal Society able to judge and explain quantum physics and quantum mechanics.

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1. Introduction

In his biography of the British physicist Charles Galton Darwin, the fellow physicist and life-long friend, George Paget Thomson, emphasised that his most outstanding contribution to science was, possibly, his ability to explain and to disseminate apparently difficult discoveries and theories in a very accessible language. He himself had benefited from Darwin's pedagogical skills in 1927 when, at the time Thomson was performing his experiments on the diffraction of cathode rays, Darwin updated him on the latest developments on wave mechanics and matrix mechanics that he had just learnt first hand in Copenhagen: "Darwin took with enthusiasm to the new ideas … and came back a dedicated missionary" (Thomson, 1963, p. 75).

The general history of quantum physics and early quantum mechanics presents us with a story in which the new theories were developed in the Continent, basically in Germany, Denmark and France, while most British physicist were either oblivious to or reacting against Planck's theory, due to their commitment to the old—classical—paradigm (Kragh, 1999; Mehra & Rechenberg, 1982). A similar account used to be told also in the case of relativity, but this simplified story has been seriously challenged by the work of Warwick (1992, 1993, 2003a) and others (in Glick, 1987), by underlining the importance of the unique Cambridge pedagogical tradition in explaining how most British physicists read Einstein's theory. In the epilogue of his book, Warwick (2003a) suggests the possibility of extending his historiographical thesis to the early history of quantum physics in Britain, something that only Jeff Hughes (1993, 1998) partly did with his analysis of the cultures of theoretical nuclear physics in Rutherford's Cavendish. Much work needs to be done to fulfil Warwick's expectations, and this paper serves as a contribution to fill this gap in the historiography of quantum physics by analysing the career of one of the first British practitioners and advocates of quantum theory.

Who are the British actors in the history of quantum physics and early quantum mechanics? The first big name to appear on the stage is Paul A. M. Dirac, about whose life and work much has been written (Kragh, 1991). Before him, the names of Darwin and Ralph H. Fowler appear only in the background against which Dirac develops his work. However, both Darwin and Fowler were *fellows* of the Royal Society, actively working as theoretical physicists on quantum related problems in the 1920s, and clearly deserve more attention by a non-whiggish history of quantum physics. Furthermore, they were both trained in Cambridge before

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the Great War, which allows us, at least *a priori*, to extend to them some aspects of Warwick's historiographical framework.

This paper focuses on the work of Darwin in the 1920s. We shall find a scientist actively working on quantum physics, with a self-imposed duty to spread the new ideas, with a tendency to grand speculation, and with intellectual, methodological and epistemological attachments to a *Mathematical Tripos* way of doing physics. The tensions between the old and the new, between the continuous and the discrete, between the ether and the quantum, between the wave and the particle, are a constant in his work. These tensions will show in the shift from a young revolutionary Darwin wanting to dispose of the conservation of energy in the early years of the decade, to a Darwin who, while fully embracing at the end of the decade the new wave mechanics of Schrödinger, is unwilling to accept the so-called Copenhagen interpretation of quantum mechanics, due to his profound epistemological and ontological commitments that find their roots in his early Cambridge training.

After some biographical notes on his early training as a physicist, I will focus on Darwin's attitude towards conservation of energy in the early 1920s. Here we shall find him in constant conversation with Bohr: the two scientists were considering the abolition of strict conservation of energy although their motivations for such a step were significantly different. The next section will look at the role of Darwin and Fowler in their job as referees on quantum matters for the Royal Society, a job that somehow turned them into important arbiters of the new physics in Britain. The last two sections will concentrate on Darwin's work in the period 1927–1930, when he fully embraced Schrödinger's wave mechanics, while, at the same time, distancing himself from the fundamental epistemological and ontological tenets of Heisenberg's and Bohr's matrix mechanics.

2. Darwin, a wrangler of the last generation

Charles Galton Darwin (1887–1962) was part of a saga of well-known scientists: his father was the Plumian Professor of Astronomy in Cambridge, his godfather was Lord Kelvin, and in his family there were a handful of F.R.S., including Charles Darwin. Born and brought up in that unique environment of Cambridge, Darwin belongs to the last generation of *wranglers*: in 1909, he finished fifth wrangler in the Cambridge *Mathematical Tripos* (MT), that *factory* of mathematicians and mathematical physicists for which any physical problem was expected to be solvable, with the aid of some dynamical mechanism, using the powerful methods of mathematical analysis. Warwick (2003a) characterises this generation as a breed of physicists committed "not only to the notion of an ether, but to a sophisticated conceptual structure and range of practical calculating techniques that were gradually acquired through years of coaching and problem solving. As they acquired these skills, the ether became an ontological reality" (pp. 396–397).

At the time of Darwin's studies in Cambridge, Joseph Larmor was the Lucasian professor and certainly one of the most influential persons in shaping the worldview that a student like Darwin would receive in the MT: "It was through examination questions, lectures, and textbooks that many wranglers trained circa 1900 learned to accept the central tenets of Larmor's theory and to apply its mathematical methods to tackle problems" (Warwick, 2003a, p. 362). This worldview materialised in what is known as the *Electromagnetic Theory of Matter* (ETM), which tried to be an all-embracing theory of matter and electricity in terms of only one entity, the ether, and one singularity in this ether, the electron. As Buchwald (1985) and Hunt (1991) explained in detail, Larmor developed the bases of his ETM theory between 1894 and 1897, following two central ideas by Joseph J. Thomson and George F. FitzGerald. Thomson (1881) had suggested that a charged body moving in an electromagnetic field would suffer friction, analogous to the one experienced by a solid body moving in a fluid. This phenomenon could be perfectly explained by attributing extra mass, of electromagnetic origin, to the moving body. From FitzGerald, Larmor was convinced that there was a need for some sort of atomic structure in the ether, which led Larmor to postulate the existence of what he first called *monads*, and later *electrons*, as singularities in the ether and atomic units of charge. With these two principles, Larmor created a theory of ether and matter, in which electrons, positive and negative, were the centres of radial strain in the ether, thus accounting for the electromagnetic phenomena in the ether, and the origin of all inertial mass. As Warwick (2003a) says, "Thomson's 'extra mass' now became the only mass, and the mechanics of the universe was reducible to electrodynamics" (p. 369).

But perhaps more relevant for our story is the fact that Larmor's ETM involved moving away from the typically Victorian project of maintaining a mechanical structure for the ether. Larmor's ether was dynamical: rather than devising a structure to account for its properties and behaviour, it was these properties that were at the very basis of the nature of the ether, turning the ether into a more abstract, more mathematical entity than the one used by previous generations of Maxwellians. Larmor's idea of the ether as the fundamental dynamical substance of the universe went beyond previous Maxwellian projects, since the mechanical properties of matter were only one manifestation of the dynamical properties of the underlying medium. In a way, it was the differential equations that defined the properties of the ether, and these required only two principles: the conservation of energy, and the principle of least action (cf. Warwick, 2003b, p. 361). As we shall see, these two principles became important elements in Darwin's intellectual career.

Another person worth mentioning is Darwin's private coach, Robert A. Herman, who was a strong supporter of Larmor's project. Senior wrangler in 1882, only 2 years after Larmor had obtained the same distinction, Herman became the last successful coach in the MT system, coaching all senior wranglers from 1903 until the abolition of the order of merit in 1909. By this time, "the prime job of a coach was to ensure that students were attending an appropriate range of courses and that they understood what they were being taught" (Warwick, 2003a, p. 282), as opposed to the more intensive private tuition by coaches in previous decades. But since Herman was also a college lecturer at Trinity (Darwin's college), he must have had a significant influence on Darwin's mathematical training.

Finally, we should mention James Jeans, a MT product himself (second wrangler in 1898) although at the time not in Cambridge, who published in 1908 his very influential *Mathematical Theory of Electricity and Magnetism*. This textbook (Jeans, 1908) was aimed at undergraduates wanting to master all the mathematical tools needed to make sense of Maxwell's *Treatise* and all ulterior developments on electromagnetic theory, and especially at students in and outside Cambridge wanting to be proficient in the methods and contents of the Mathematical Tripos. This book thus helps us understand the training of an MT student in Darwin's years. In this respect, it is significant that one-fourth of the book was devoted to Larmor's ETM with his theory of the electron and the dynamic ether, as well as the centrality of the conservation of energy and the principle of least action.

After his graduation, Darwin left the fenlands and moved to Rutherford's laboratory in Manchester, where he was appointed lecturer in Mathematical Physics. There, he did mathematical work on the scattering and diffraction of radiation, especially X-rays and α-rays, developing a theory on the structure of crystals as derived from the reflection of X-rays (Darwin, 1914); a work that, as Lawrence H. Bragg later recalled, "X-ray crystallographers have always regarded ... as one of his finest contributions to science" (in Thomson, 1963, p. 71). But more importantly, he was in Manchester at the time when Rutherford brought forward his structure of the atom, which Darwin fully embraced, and on which he would immediately do some theoretical work. To this time belong his *Theory of the Absorption and Scattering of* α *Rays* (Darwin, 1912) and his classical analysis of the possible orbits for an electron in a Rutherford-type atom (Darwin, 1913). The latter paper shows him in continuity with Larmor's intellectual project, using Lorentz's formula of the "deformable" electron, which "besides being apparently in best agreement with experiment, makes possible a complete integration of the equations of motion" (Darwin, 1913, p. 202). It neglects, however, the influence of radiation in the electronic orbits, since "there are, of course, very definite indications that the mechanism of radiation is in some way different from that given in the electromagnetic equations" (p. 208).

Manchester was also the place where Darwin developed his experimental skills. Training in the MT was exclusively mathematical, and there was little time for laboratory work. Students wanting to learn some basic skills in experimental physics normally waited until the end of their undergraduate years to acquire basic experimental know-how. The Cavendish laboratory under J. J. Thomson's directorship was the natural place for an MT graduate. There, he would have learnt how to master most experimental techniques in electricity and magnetism, as well as to manipulate vacuum pumps and discharge tubes (Kim, 2002, p. 165). In Manchester, however, experimental physics evolved around the new field of radioactivity, and, contrary to the wrangler culture of Cambridge, experiment was at the very foundation of physics. Rutherford "espoused a much more physical, intuitive approach to theory in which the preferred result was an easily visualisable picture, and preferable one suggestive of further experiment" (Hughes, 1998, p. 343). In this culture, Darwin managed to publish some papers on experimental work on X-rays and radioactivity in collaboration with Ernest Marsden and, especially, with Henry G. J. Moseley (Marsden & Darwin, 1912; Moseley & Darwin, 1913).

Also in Manchester, Darwin met Niels Bohr, who moved to Rutherford's department in March 1912 after a disappointing experience in J. J. Thomson's Cavendish. This meeting proved important first for Bohr since, as Heilbron and Kuhn (1969, pp. 237–245) discussed in detail, Bohr's early ideas on the structure of the atom were indirectly triggered by Darwin's work on the absorption and scattering of α-rays. In one of his first papers, Darwin (1912) had assumed that (i) the loss of energy of α-particles was due to interactions of these only with the electrons in a Rutherford atom, and (ii) that electrons could be considered to be free in the atom. In rejecting these two assumptions, Bohr shifted his interest from the behaviour of electrons to the structure of the atom. In a letter to his brother Harald on June 12, 1912, Bohr explains how "a couple of days ago I had a little idea for understanding the absorption of α-particles (the story is this: a young mathematician here, C. G. Darwin, (grandson of the right Darwin) has just published a theory about it, and I thought that it was not only incorrect mathematically ... but also very unsatisfactory in its basic conception), and I have worked out a little theory about it, which... can perhaps shed a little light on some things concerning the structure of atoms" (in Heilbron & Kuhn, 1969, p. 237). In the long run, it was Darwin who benefited most from this relationship. As we shall see in the next section, the letters that Darwin and Bohr exchanged in the years after the war speak of a relatively close friendship between both physicists. They also show that Darwin admired Bohr's desires to reform and to build a new physics that incorporated the tenets of the new quantum physics.

Certainly, without exhausting all that can be said on Darwin's training as a physicist, these few strokes of the brush help us locate him as a *wrangler* of the last generation, who was soon influenced by experimental and theoretical approaches different to the ones that characterised the Mathematical Tripos. As I shall try to show in this paper, Darwin's career in the 1920s suffered from the tension between these two cultures, a tension that can partly explain his initial desire to challenge and reform the very fundamentals of physics and his later refusal to go too far in this reformulation.

3. Darwin 1919–1922. Challenging the very fundamentals of physics

During the Great War, Darwin engaged in several war-related jobs: at the Royal Engineers, working with Lawrence Bragg on the detection of enemy guns by sound waves, and at the Royal Air Force working on the noise of aeroplanes. These were years of intense experimental and practical work related to the behaviour, interference and detection of waves. In 1919, he was appointed *fellow* and lecturer at Christ's College, Cambridge, which allowed him to go back to his *alma mater*. At the same time, Rutherford was taking over the directorship of the old Cavendish, putting an end to the 35 years of J. J. Thomson's reign in the laboratory. What Darwin found in the old university can be guessed from the letter he sent to Bohr on his arrival in Cambridge:

> What did you think [of] J.J. in the April Phil[osophical] Mag[azine]. I felt I wanted to shake him. It seems to disregard everything that has been done since about 1900. I am extraordinarily glad that Rutherford is coming here, as really it seems to me that physics and applied mathematics here are in an awful state. I am doing my inadequate best to talk to people about quanta; everybody accepts them here now (which is better than it was in 1914 at any rate), but I don't think most of them realize their fundamental importance or have studied the arguments in connection with them ... There are plenty of very intelligent people, only under the blighting influence of studying such things as strains in the ether, they none of them know what it is worth doing (*Archive for the History of Quantum Physics*, Darwin to Bohr, 30.05.1919, microfilm BSC 1, 4).

In this letter, Darwin appears as an advocate for quantum physics in a world where the theory of quanta had been either ignored or looked upon with a lot of distrust. As Darwin mentions, the situation had changed during the war. Much work needs to be done on how and why this change took place. For the time being, historians seem to agree on the central role played by James Jeans' 'conversion' to Planck's theory during the early 1910s and his *Report on Radiation and the Quantum Theory* for the Physical Society of London (Hudson, 1989; Jeans, 1914; Milne, 1952). This 90-page book was one of the main sources from which British physicists learnt about quantum theory in a systematic way during and immediately after the war. Upon his arrival in Cambridge, Darwin promptly organised, together with Francis W. Aston, and as part of the optional courses to be offered in the last year of the *Mathematical Tripos*, a series of lectures on quantum theory, thus initiating his *mission* for quantum physics in the world of Cambridge.[1]

[1] See the *Cambridge University Reporter* (1919–1922). In 1919 Darwin offered a course on 'Quantum Theory and Origin of Spectra'. This course changed to 'Recent

But what kind of quantum physics was Darwin advocating in the early years after the war? To summarise his approach briefly, one might say that Darwin was converted to the phenomenology of quantum physics, including aspects of Bohr's atom and Sommerfeld's orbits, but not to the fundamental assumptions of Planck's theory and especially not to the quantum of light. In a letter to Bohr drafted in the summer of 1919, Darwin summarised his views on the current situation of physics stating that further experimental and theoretical work "would force us to look for our modification in Planck rather than in Maxwell" (Darwin, 1919, p. 196); meaning that the solution to the contemporary problems in physics had to be sought in challenging the new theory of interaction between ether and matter (Planck and Einstein's laws of radiation), and not the many times confirmed Maxwellian framework for the behaviour of electromagnetic waves in the ether (which was in complete contradiction with the existence of free quanta of light). To do so, he was ready to give up, if needed, one fundamental principle of classical physics, i.e., the exact conservation of energy. Following a very inductive line of thought he ventured to say:

> I have long felt that the fundamental basis of physics is in a desperate state … In the absence of any direct positive guide as to the reform needed, … it may be possible to knock away the proofs of classical physics one by one and find, after a particular one has been removed, that our difficulties have been reconciled … the proofs of the contradictions all rest on the exact conservation of energy, and I therefore claim that the possibilities to be deduced from denying this should be thoroughly exhausted before further modifications are made (p. 194).

Darwin was, thus, one of the first physicists to advocate the non-conservation of energy to account for atomic phenomena, an idea that would resonate with Bohr for more than a decade. It is interesting, however, to see the way Darwin argued for this. As a Cambridge *wrangler*, he acknowledged the need for a mechanism to account for the quantum behaviour. But the mechanisms developed by Planck and Poincaré, based on the behaviour of resonators in a black body, were for Darwin unsatisfactory because they contained too many *ad hoc* hypotheses, which were needed precisely because they kept a strict conservation of energy:

> The models devised by Planck's second theory are very artificial. One has to conceive of every electron containing a complicated system of clockwork which can be wound up by the incoming radiation, and a governor capable of setting free the energy at a certain point, while the machinery is uncertain in its action according to a very definite law of probability. A similar model holds for the photoelectric effect. It is impossible to believe that if the science of the present time had not been saturated with the idea of conservation of energy, these complications would have been avoided by saying that there is no *exact* conservation in such cases … I maintain that the most promising outlook for a reconciliation of our difficulties is to suppose that energy is not exactly conserved (Darwin, 1919, p. 196, emphasis in the original).

Bohr drafted a quick response (which, by the way, was sent without further additions only two and a half years later), in which he sympathised with Darwin's ideas (Rosenfeld, Rudinger, & Aaserud, 1984, pp. 15–16). In 1919, Bohr partly agreed with Darwin's diagnosis on the fundamental problems of physics and with the abandonment of strict conservation of energy as a possible way out. As is well known, this possibility would emerge in 1924 at the core of the well-known paper by Bohr, Kramers, and Slater (1924) on a quantum theory of radiation.

Darwin spent the year 1922 as Visiting Professor at Caltech. There, he addressed his attention to a theory of optical dispersion that would embody his ideas on the conservation of energy. To stress his acceptance of the conservation of energy only as a statistical measure, he made an exercise of historical fiction saying that "had the photoelectric effect been discovered a century ago, it is probable that no one would have ever suggested that the status of the first law of thermodynamics was in any way different from that of the second" (Darwin, 1923, p. 25).

One might be tempted to call Darwin a revolutionary for wanting to abandon the strict conservation of energy, but this was only the consequence of an even more conservative attitude towards other elements of classical physics; i.e., (i) the wave theory of light, which "forms a consistent whole, but which at present only fits into the quantum theory with a good deal of difficulty", and (ii) the "complete truth" lies in dynamics, even if it does not necessarily include an exact conservation of energy, which is "only one of the consequences of the dynamical equations" (Darwin, 1923, p. 25). He certainly took for granted the validity of the wave theory "outside matter" (Darwin, 1923, p. 26) and, with it, "the exact conservation of energy in the ether; it is in the interchanges with matter that it need not be conserved" (Darwin, 1923, p. 26). And that was the old Maxwellian problem that had kept so many physicists busy since the publication of the *Treatise*: the interaction between ether and matter.[2] To put it differently, Darwin's openness to the abandonment of the strict conservation of energy was consistent with his *MT* background, since it was precisely in the interaction between ether and matter that Maxwell's system was not complete, and where any scientist with a tendency to grand speculation—as Darwin was—could feel free to suggest new ideas. For the same reason, the theory of a quantum of light as an alternative explanation to the propagation of light in the ether was totally out of the question, since Maxwell gave a complete account "outside matter" (Darwin, 1923, p. 26).

Darwin's theory for optical dispersion was rather vague. He assumed that, when an atom is struck by a wave, there is a certain chance that the atom emits a secondary spherical wave, which depends "*only on the nature of the atom and not at all on the incident force*" (Darwin, 1923, pp. 26–27, emphasis in the original). The dispersed wave would be the superposition of the incident and the secondary emitted waves, like in the classical case of a light-vector interacting with an ensemble of electrons. He went on showing how, since the spherical waves emitted by the individual atoms have random frequencies, the average extended to all the atoms would cancel out all the different frequencies, and the observed dispersed wave would have the same frequency as the incident beam. For the same reason, the energy balance could be on average conserved, while not necessarily so in individual events.

The big question for Darwin was, of course, the dynamics inside the atom that would trigger the emission of a spherical wave with a particular energy. His very tentative suggestion is as follows:

(*footnote continued*)
Developments on Spectrum Theory' the following year, and a joint course on isotopes with F. W. Aston in 1921. In 1922 R. H. Fowler gave his first special course on 'The Theory of Quanta'.

[2] Perhaps the most significant example of this Cambridge culture was J. J. Thomson, whose whole research project on discharge tubes can be understood as basically an attempt to give continuation to Maxwell's *Treatise* by understanding the interaction between ether and matter (see Navarro, 2005).

> We suppose that an atom is usually in its lowest quantum state. The motions of the electrons will sometimes lead to a favourable configuration and when this occurs in the presence of a changing electric force there is a chance that the atom may be jerked into a condition in some way associated with one of its higher quantised states. It at once starts radiating with a frequency corresponding to the return from that state to the lowest (Darwin, 1923, p. 29).

And he goes on with even more tentative assumptions, saying that the atom might actually go "into the higher quantised state, and then gives a wave of such amplitude and length that, but for the interference with the incident light, it would emit energy $hk_n/2\pi$" (p. 29). In other words, he assumes that a *certain configuration* of the components in the atom will trigger a wave with a particular energy, multiple of h. This was, of course, a very *ad hoc* supposition, without further justification, to make his theory match the quantum condition.

Darwin also hoped that this mechanism would be a guide to understanding the spontaneous emission of radiation by atoms:

> …we may perhaps extend our theory to cover pure emission; for though we have not postulated any precise relationship between electric force and electrons, it seems inevitable that there should be a rapidly changing electric force near a moving electron, and that this force would have a chance of jerking the atom to its higher state (p. 29).

As Darwin explained in a letter to Bohr, "I have left it open whether the upper states really are stationary, but in private may I say that I personally do not believe they are. I think that the atom always goes to the upper state and starts radiating at once" (Darwin to Bohr, 23.11.1922, in Rosenfeld et al., 1984, p. 17). With his suspicion over the real existence of upper stationary states, however, Darwin was aware that he was solving some problems by introducing others, like the accepted theory of specific heats, which "requires that a molecule should be able to remain in its higher states"; and, ultimately, to "reexamine the deduction of the formula for black radiation, for all present proofs are founded on theorems following out of the conservation of energy" (Darwin, 1923, p. 29).

On receiving a copy of this paper, Bohr reacted with little enthusiasm, not because of the challenge to the strict conservation of energy, but for other reasons. Actually, in this particular moment, the possible abandonment of the strict validity of the conservation of energy and momentum was not a problem for Bohr since he was also talking openly about this possibility. In his 1924 paper, which was intended as the first of three papers to explain and clarify his views on the current state of quantum physics, Bohr stated that "in its present formulation, the law of the conservation of momentum, as well as the law of the conservation of energy, do not permit cogent conclusions to be drawn concerning the *nature* of the processes. These laws rather only permit conclusions to be drawn concerning the *occurrence* of those processes which are conceivable according to the postulates of the quantum theory" (Bohr, 1924, p. 40, emphasis in the original).[3] This distinction between the *nature of the processes* and the *occurrence of those processes* expresses very well the reasons why both scientists were ready for a revolutionary step: quantum physics, as it stood in 1923, could not be seen to go deep enough into the *nature* of atomic processes, but was basically an heuristic theory to account for some observed phenomena.

Thus, it was not the conservation of energy that Bohr disliked from Darwin's proposal, but the challenge to the real existence of the stationary states. "Notwithstanding the formal beauty of your view, he said, I feel great difficulties, however, in accepting it. … I think that the great variety of experiments on resonance spectra claims a greater reality of the stationary states than you appear inclined to ascribe to them" (Bohr to Darwin, 21.12.1922, in Rosenfeld et al., 1984, pp. 17–18). While Darwin was willing to consider the stationary states as just a consequence of the interaction between the atom and a changing electric field (both in induced and in spontaneous radiation), Bohr was absolutely certain of their reality. They actually constituted the *first fundamental principle* of the quantum theory, and they had been the central assumption in his atomic model a few years earlier. It was obvious that Bohr could not accept Darwin's trivialisation of the stationary states. One might say that Bohr was more bound than Darwin to the force of empirical results concerning stationary states, or, following the above distinction, Bohr saw the existence of stationary states as part of the *nature* of the atom whereas Darwin was more inclined to consider them as just an *occurrence*.

4. Darwin and Fowler, the two British 'experts' in quantum physics

Darwin's most relevant work in the early years of the decade was a series of papers he published with Ralph H. Fowler on the partition of energy (Darwin & Fowler, 1922a and 1922b). Fowler was, at that time, a tutor and lecturer at Trinity College, Cambridge, where he had also been a student in mathematics (see McCrea, 1993). He had graduated 2 years after the abolition of the order of merit, which gave him more flexibility to focus his studies on his real interest in pure mathematics, rather than on mathematical physics. Actually, after his graduation in 1911, he began a career in pure mathematics, publishing a number of papers on differential equations, and it was only during his involvement in the war effort, inventing and directing operational anti-aircraft, that he shifted his attention to applied mathematics and theoretical and experimental physics. Unlike Darwin, who could learn the developments of the early quantum physics in the Manchester of Rutherford and Bohr, Fowler was self-trained in the theory of quanta. Like Darwin, he was one of the first British physicists to realise the fundamental importance of the new physics, to work on it, and to spread it among the British scientific public by, for example, translating into English many of the key papers that were appearing in German, as well as inviting people such as Kronig or Heisenberg to give lectures in Cambridge. It is also well known that Fowler became a sort of *theorist-in-residence* at the Cavendish, as well as Rutherford's son-in-law.

Darwin's work with Fowler consisted basically of a mathematical technique to calculate exactly the contour integral in the function for the partition of energy, something that since the times of Boltzmann could only be done approximately. Their approach was, in principle, both classical and quantum, the former being a limit case of the latter, following Bohr's correspondence principle: "the possible states of the system may be divided into cells; these cells are fixed and finite for quantized systems, but for the systems of classical mechanics must ultimately tend to zero in all their dimensions" (Darwin and Fowler, 1922b, p. 825). The real value of this work resided in the evaluation of the integral of the classical—continuous—case, rather than the summation of the quantum—discrete—system, which had no serious mathematical difficulty. For the argument of

[3] Hendry (1981) argues that, at this time, Bohr was rejecting Pauli's operationalist approach. At the time when Heisenberg was suggesting to do away completely with the notion of orbit, Bohr was still clinging to the wave theory of light and rejecting Einstein's quantum of light. At the time, Bohr was not yet the positivist he would later become. An evolution of his philosophy of science can be found in Murdoch (1987).

this paper, however, it is interesting to point out the fact that Darwin and Fowler justified their calculus as something valid for both quantum and classical physics, and emphasised that the two realms were united by the correspondence principle.

The direct collaboration between both Cambridge physicists ended in 1924, when Darwin was appointed the first Tait Professor of Natural Philosophy at the University of Edinburgh. Although now in different universities, Darwin and Fowler were the only two British physicists, *fellows* of the Royal Society, who could judge on quantum matters. This can be seen from the remaining referee reports for the *Proceedings of the Royal Society*, where they reviewed all papers related to quantum physics and quantum mechanics. This, in a way, turned them into the arbiters of quantum physics in Britain in the mid-1920s. Both thought that British physicists should be more active in the race to develop quantum physics, either by trying to solve specific problems (Fowler) or by giving alternative solutions (Darwin), and that is why they were keen to encourage the fast publication of the few papers on the topic submitted to the *Proceedings* (see Kragh, 1999, Chapter 10). One referee report in particular witnesses this. Dirac's (1926) paper on the "Quantum Mechanics and a Preliminary Investigation of the Hydrogen Atom" was published just 1 month after the reception of the manuscript. As is well known, Dirac's early work in quantum physics was developed under the guidance of Fowler and thus Darwin refereed his papers. In his report, he wrote that "the paper is a very important contribution to the newest developments of physical theory. It is a brilliant piece of work, and should if possible be published quickly, as it is in a field where others are working in competition. It would be a pity if the author were to miss the credit for this work through being late in publishing" (Referee Reports, *Archives of the Royal Society*).

As we shall see in the following sections, Darwin's approval of Dirac's work was only partial, since Darwin's own work after 1926 began to emphasise a priority of Schrödinger's wave mechanics over the matrix mechanics of Heisenberg, Pauli and Dirac. Fowler acted as referee to Darwin's papers and we can perceive a differentiating style between them, the former being less prone to speculation than the latter. In his report on the paper "Free Motion of the Wave Mechanics" (Darwin, 1927b), which shall be commented below, Fowler advised in the following terms:

> I don't quite like some of the remarks in the introduction as I think they are somewhat over bold, and I feel that a false contrast is drawn between matter and waves. The [subject matter?] is a calculus of linear operators, and matter and waves are equally forms of the same calculus in *all* respects. These points however are hardly inconsistent to the theme of the paper, and I do not recommend return to the author. It may even be that his remarks on these points be valuable just because they overstress the wave view, which has hitherto not been stressed enough. A very valuable paper (Referee Reports, *Archives of the Royal Society*, emphasis in the original).

Actually, only a few months earlier, Fowler had written a very accessible—and enthusiastic—paper in *Nature* to explain both matrix and wave mechanics, their equivalence, and their relationship with classical mechanics and the old quantum physics, where any supremacy of one view over the other was dismissed as "futile". Nevertheless, he condescended with the "majority of workers" who might find that "the wave mechanics, owing to the greater familiarity and convenience of its algebra, is the more powerful tool for solving any particular problem" (Fowler, 1927, p. 241).

Fowler's attention to quantum mechanics in the late 1920s diverged from Darwin's approach also because the former was focused on practical problems of quantum mechanics; i.e., he was more interested in applying the principles and latest developments of quantum mechanics to new problems, especially to problems of chemical valence and chemical structure, rather than in challenging and discussing the very fundamentals of quantum physics. As Gavroglu and Simoes (2002) argued, Fowler was instrumental in the establishment of an incipient research school on quantum chemistry in Cambridge in the 1920s, a school that included people such as J. Lenard-Jones, D. R. Hartree and C. A. Coulson, and that would eventually give way to a more established, Oxford-based, British research group in quantum chemistry from the 1930s.

Going back to Darwin, his work in the period 1924–1926 shows an increasing determination to underline the continuity, rather than the discontinuity, between classical physics and quantum physics, to the extent that his papers deal with totally classical matters, especially in optics. More often than not, he claims that classical and quantum approaches must be related, following Bohr's "correspondence principle", which, in turn, legitimises the use of the old physics. One of the clearest examples is his study of the Zeeman effect, which he tried—unsuccessfully—to explain, in 1925 and 1926, using totally classical mechanical models for the interaction between radiation and matter. In the second papers on this issue Darwin stated:

> There can be no doubt that the proper approach to the problem is through the New Mechanics of Heisenberg applied to the rotating electron of Uhlenberg [sic] and Goudsmit; but little has yet been published about the rotating electron, and the New Mechanics has many more fundamental problems to dispose of first, so that it is perhaps still not out of place to make what proves to be an exceedingly unambiguous application of the Correspondence Principle (Darwin, 1926, p. 315).

But a footnote at the end of this paper shows that the speed at which quantum mechanics was developing was overtaking Darwin: "Just as this paper was completed there has appeared a work by Heisenberg and Jordan … which fulfils this expectation. They have obtained the complete formula for the doublet system at all strengths of field" (Darwin, 1926, p. 335).

Was it, perhaps, time to change his approach to quantum mechanics and accept what was coming from Copenhagen? Darwin was genuinely interested in the problems of spectroscopy, statistics and atomic constitution, and he could see Bohr's school gaining huge momentum. But he was also certain that the tradition of physics to which he was heir could and should be pushed forward to give consistent explanations to quantum phenomena. In order to compare his and Bohr's new approach, he decided to spend some time in Copenhagen and learn and discuss the new physics first hand. As we shall see, the outcome was not surrender, but a firm conviction that classical physics was more powerful than Bohr and Heisenberg's methods.

5. Learning, but not totally accepting the new quantum mechanics

Darwin spent the spring of 1927 in Copenhagen (see Robertson, 1979, p. 156). There, he could discuss the latest developments that were taking place in Bohr's school, and developed his own work on wave mechanics, which he had already learnt and taken on board in 1926. It was after this trip, according to G.P. Thomson, that Darwin became a "missionary" of quantum mechanics in its wave formulation (Thomson, 1963, p. 75). During his stay at the Institute for Theoretical Physics, Darwin developed his equation for the spinning electron based on Schrödinger's wave mechanics, in response to Pauli's (1927) theory of the magnetic electron that used the language of matrix mechanics. The central focus of my discussion here is Darwin's rhetoric in this and the next papers, which manifests his attitude in the face of the new quantum mechanics; a rhetoric that shows his increasing distance from the mainstream of quantum matrix mechanics and his loyalties to certain aspects of the old Cambridge physics, such as the importance of mechanical models and the efficacy of the analytical methods learnt in the *Mathematical Tripos*, as well as the need to preserve as much as possible from the classical worldview. In this paper, Darwin (1927a) was in constant dialogue with Pauli and his method, to the extent that he devoted a full section of the paper for comparing both methods, emphasising that Pauli's was unable to give an idea about the reality behind the mathematics:

> Recently Pauli has published a paper on the same subject, and arrived at the same mathematical results, but owing to the fact that he is more disposed to regard the wave theory as *a mathematical convenience and less as a physical reality*, he stops short of the point which was the guiding principle to me, and refuses to interpret the two functions that we both obtain as formed from a vector. I shall therefore here develop somewhat fully the arguments and analogies which seem to me to show that the vector is the right form in which to regard it (p. 227, emphasis added).

I emphasise the last sentence because this is, somehow, the guiding light of Darwin's paper: the assumption that an electron behaves like a wave could help overcome the difficulties of visualization of the spinning electron. From his point of view, one of the main problems in the early theory of the spin was that "if we attack the problem of the spinning electron by regarding it as a rotating body … we lose all possible visualization" (p. 229), and this was against his way of facing problems in physics. This objection was met if "we take the analogy of light and assume that, just as there are two independent polarised components in a wave of light, so there are two independent components in the wave of an electron" (p. 230). The analogy with the mathematics of light went as far as to state that "anyone who rejects this argument for the electron [i.e., the conditions for invariance of the equations] must be prepared for consistency to reject the whole ordinary interpretation of the electromagnetic theory" (p. 238); or, in other words, the analogy between light and electrons was complete. For Darwin, denying the wave equations for the electron would be tantamount to denying the wave equations of the electromagnetic field.

This and all the papers written immediately after his first visit to Copenhagen have an increasing ontological and methodological flavour in favour of the wave treatment. That visit had convinced him of at least two things: (i) that quantum mechanics could be finally treated in continuity with the old physics by placing de Broglie's principle and Schrödinger's equation at the centre of the new physics; and (ii) that there was a radically new way to tackle the problems in physics that was gaining a lot of support among physicists, young and not so young—the last 'convert' being Bohr himself—a method that, however useful and efficient, was a threat to the very essence of the scientific endeavour. Perhaps one of the first clear statements in this respect appears at the end of 1927, in his paper "Free Motions of the Wave Mechanics," where he responded to the general picture that Bohr gave in Como, in September that year. There, Bohr (1927) had made what many people saw as the first public and definitive embracement of the new matrix mechanics, to which Darwin (1927b) now responded, with a hint of irony, saying that he could "show how a simple, even old-fashioned, technique is entirely adequate to deal with these very new problems" (p. 258). And he went on:

> The matrix and the wave mechanics have both been already developed to great lengths as a calculus of stationary states, but they have not yet got so far in what we may call dynamics, a description of the *progress* of events. More and more complicated phenomena have been fitted into the same scheme, but not much has been done in making this scheme intuitively understandable (p. 258, emphasis in the original).

We can see, yet again, one of Darwin's deep epistemological commitments: a physical theory needs to be an explanation of the dynamics of the process, not simply a mathematical relationship between initial and final states. After this, he mentioned the everlasting problem of wave/particle duality. Here he praised Bohr for his principle of complementarity by which—in Darwin's words—"the two lines of thought … do not come into conflict because they never meet" (p. 259). Darwin's understanding of complementarity was, however, idiosyncratic, since he did not really consider both approaches to be totally symmetric.

In support of the superiority of wave mechanics over matrix mechanics Darwin mentioned the fact that, when it comes to non-stationary states, the solution can be found by superposition of the stationary states, something that is quite natural in the classical wave theory of light, but totally *ad hoc* in the language of matrix mechanics: "It is a great merit of the wave theory that it invites this superposition; with matrices the corresponding generalisation does not suggest itself" (p. 260). Thus, the superiority of the wave treatment did not come from being more efficient, or from giving better results, but from it being in natural continuation with the old mathematical methods used in Maxwellian physics. And, if that was the case, why should one need to create a whole new mathematical system?

Actually, his work in the period 1927–1930 can be understood as a constant reaction to the advances of matrix mechanics. We have already seen that, in 1927, Darwin succeeded in giving a totally consistent alternative, from wave mechanics, to the theory of the spin developed by Pauli. The new challenge provided by Dirac's (1928) relativistic equation for the electron, however, proved to be more difficult to meet than Pauli's. The great problem here was that, while any relativistic approach to the wave equation was obtainable only as a first-order correction to the non-relativistic Hamiltonian, Dirac had made it possible to insert relativity conditions into the very structure of the equation for the electron. As Kragh (1991) explains, "in contrast to Pauli, Schrödinger, and Darwin, who all imagined that the problem of integrating spin and relativity would probably require some sophisticated model of the electron, Dirac was not at all interested in model-building" (p. 57).

On this occasion, Darwin had to face a theory with doubtless advantages from a formal point of view. Dirac's equation was invariant under relativistic transformations, it preserved the structure of Schrödinger's equation (linearity in $\partial/\partial t$), and it also included the spin as a consequence of the equation, not as an *ad hoc* hypothesis. Furthermore, it did not assume any model for the electron, as Pauli and Darwin's equations for the spin did. Dirac's electron was just a point-charge, and so he did not have to worry as to "why Nature would have chosen this particular model for the electron instead of being satisfied with the point charge" (Dirac, 1928, p. 610). By not having to worry about physical models, Dirac could make the bold suggestion of introducing such *unphysical* terms as 4×4 matrices, which were, on the other hand, only *natural* from a purely mathematical point of view (see Kragh, 1991, pp. 54–60). The only thing Darwin could now do was to prove that his own equations for the electron, obtained in the previous paper, were "an approximation to the new ones, derived by an approximate elimination of two of Dirac's four wave functions" (Darwin, 1928a, p. 655). With a certain sense of defeat, Darwin had to acknowledge that:

> Dirac's success in finding the accurate equations shows the great superiority of principle over the previous empirical method, but it is perhaps not without interest (at any rate to the present writer, who had projected but not begun such work) to consider whether the empirical method could have led by way of improved approximations to the accurate result … On the whole, it seems not impossible that one might with much labour have arrived at some sort of eliminant of Dirac's equation (p. 664).

Although the previous paragraph might look like a statement of partial surrender, Darwin was not giving up, also because Dirac's equation was not without problems. Actually, the difficulties in trying to translate Dirac's equations into the language of wave mechanics reassured Darwin of the validity of the latter. His subsequent papers were an attempt to deal with different problems associated with the wave mechanics of the electron, such as their diffraction and magnetic moments, where "a comparison is made between an electron wave and a light wave, and the resemblance may be loosely expressed by saying that a light-quantum is an electron without charge or mass" (Darwin, 1928b, p. 631). The connection between the old and the new, between classical and quantum, should not be disposed of.

Darwin's support for the wave formalism was not only based on his commitments to the old physics, but also supported by the experimental and visual evidence provided by his friend G. P. Thomson on the diffraction of electrons. As Darwin was putting wave mechanics into action, Thomson was turning his experimental display for the study of positive rays into a device for the observation of diffraction patterns of cathode rays (see Navarro, forthcoming). It was in the summer of 1927 that Darwin spent a few weeks in Aberdeen, at the house of the Thomsons, where the two friends could discuss the developments and implications of quantum wave dynamics. Those conversations were influential in the way G. P. Thomson received quantum mechanics. At the same time, the pictures of the diffraction patterns of electron beams that Thomson obtained in the autumn of that same year confirmed to Darwin the superiority of a visual wave mechanics over the purely mathematical approach of Heisenberg, Pauli, Dirac and others.

6. On scaffoldings and realities

Matrix mechanics and especially Dirac's method was proving too difficult to counteract with the traditional methods of wave mathematics, and support for the new methods was growing. Darwin was not rejecting them, but he was sticking to his old approach for a number of reasons. First, and perhaps most obvious, that was the method in which he had been trained and which he had mastered without difficulty, to the extent that in numerous occasions he refers to them as the *natural* approach. Second, matrix mechanics seemed to rely only on formal-mathematical properties of the equations without any obvious connection to representation. For Darwin, the need to have a visual representation was a *sine qua non* condition for true physics and, *therefore*, also for its semi-popular understanding. What could not be imagined could not be real. Furthermore, it was only natural to expect continuity between the old and the new physics, and the wave formulation provided precisely that. His mood in the early 1930s can be captured in the preface to his *The New Conceptions of Matter*:

> I do not know how far my presentation will find favour with experts; it is certainly not the same as that adopted in some of the more technical works that have recently appeared, and yet I believe that it makes a consistent formulation of the foundations, which only needs the help of mathematics to yield all the results of the theory so far obtained. However that may be, I have little hesitation in saying that, if it is to be at all possible to present the new mechanics in popular or even semi-popular terms, it must be done more or less on the lines I have adopted here (Darwin, 1931, preface).

The New Conceptions of Matter compiles Darwin's lectures at the Lowell Institute of Boston, a very prestigious forum for public lectures, in the spring of 1931. It is a very interesting document because it gives us a very fresh view of his thoughts at the time. His was not an audience of experts in quantum mechanics and, therefore, he felt free to give his particular approach to the latest developments, mixing mathematics, physical interpretations, and philosophical speculation. His aim for the course was to give a consistent picture of the latest developments in atomic physics, so that far from "the insidious dangers of pedantry" that such a topic might lead to, "any surviving reader will speak no longer of the mysteries of science, but, shall we say, of *the naturalness of Nature*" (p. 2, my emphasis). And this is precisely the guiding idea of the course: to show that, deep down, there is nothing in the new physics that cannot be understood by the layman, because the new is in continuity with the old physics.

Certainly, Darwin says, progress in science comes from the appearance of unexpected experimental phenomena and, more often than not, from the mathematical development of already existing theories. In any case, it is the latter, i.e. mathematical reasoning, that usually guides "the technique of discovery" and, thus, "every *new* body of discovery is mathematical in form, because there is no other guidance that we can have" (p. 3, emphasis in the original). However, mathematical form is only the guidance, the method, the technique to open up new avenues for science; in no case can it constitute the final end of any science. After some time, "as familiarity grows we find unsuspected analogies with our previous experience, and the engineer gets his chance of replacing the mathematician" (p. 3); the purely mathematical formulation becomes accessible by way of familiar, realistic, mechanical images.

That is precisely what Darwin thought could already then be done, in 1931, with the corpus of wave mechanics. The task was to liberate physics from the "scaffolding" that had helped its development but which was then hiding the *real* thing:

> Now the new discoveries which I am to describe were only begun five years ago, and so it is hardly surprising that they are still partly covered with the scaffolding of mathematical formalism. But I hope to show that the time has already come, when it is possible to free the structure of the atom from a good deal of this scaffolding and so to gain something of an intuitive view of what the world is really like (p. 4).

The book is structured in such a way that the undulatory nature of particles emerges as a natural consequence of the very nature of a wave, something that had already been, in however embryonic form, in the work of mathematicians in the 19th century. The continuity between classical physics and quantum mechanics appears, thus, only natural:

> We recall that Hamilton worked out very exact analogies between the behaviour of rays of light and particles of matter. He showed that one could construct the parabola of a projectile by the same process as one calculates the path of rays of light through a medium, using in both cases the Principle of Least Action … Hamilton did not carry his methods further than this, because he can hardly have dreamt that the reason why his analogy was so perfect was that the things described were exactly the same. But de Broglie pushed the matter to its

> logical conclusion by saying that if light and matter are refracted in the same way, then they ought to be diffracted in the same way too (p. 107).

At this point we should pause for a moment and go back to the early days of Darwin as an MT student in Cambridge. As explained in Section 2 of this paper, Larmor's ETM informed the worldview that a *wrangler* of the last generation would receive. The principle of least action was one of the central mathematical tools on which the whole system rested. And now, 20 years later, Darwin was faithful to that approach, arguing that de Broglie's principle was a natural generalisation of Hamilton's analogy between the trajectories of corpuscles and the behaviour of rays of light. But there is another, more general idea of Larmor, which surfaces again and again in Darwin's explanations: the naturalness of the new physics.

Central to Larmor's epistemology was what Warwick (2003b) describes as his "natural history of physics", or the idea that new developments in physics should stem as an evolution of, rather than a rupture from, previous theories. ETM was, for Larmor, the natural continuation of a train of thought that could be traced back to names such as Newton, Lagrange, Young, Fresnel, Faraday, Maxwell, Helmholtz or Hertz. And that can be seen in many of his writings, especially in his *Aether and Matter* (Larmor, 1900), where historical arguments are used in support of his ETM. In Warwick's (2003b) words, "The reader of *Aether and Matter* was therefore primed with a long 'Historical Survey' which recounted the history of physics in such a way that it made the concept of an electrodynamic aether populated with tiny centres of intrinsic strain seem the inevitable outcome of the development of physical theory. The abandonment of this cumulative evolutionary process could only mean the end of real progress in physics" (p. 373). And traces of this mentality are clear in Darwin's writings in the late 1920s and early 1930s. If the new undulatory physics had to make sense it could only do so in continuity with classical physics. That was the only way physics could remain 'natural' and, therefore, epistemologically relevant. In other words, Darwin's insistence on the superiority of wave mechanics over quantum mechanics stems from his belief that physics could not content itself with pure pragmatic or conventionalist interpretations. Like Larmor, for whom it would have been "practically impossible to have drawn any sharp distinction between the real nature of the physical world, and the mode by which it was apprehended" (Warwick, 2003b, p. 369), Darwin could not regard true intelligibility without some direct or indirect visualisation of the solutions given to a particular physical problem.

That brings us, then, to the key question of what reality was like for Darwin: undulatory or corpuscular. On this topic he attributed an ontological primacy to the wave via the wave function, while the corpuscular aspect appeared as a consequence of the process of measurement. This asymmetry between both aspects of matter in favour of the wave picture had already been present in his more formal papers. The clearest example was a 1929 paper in which he analysed the collision of particles from a wave point of view, which showed "that there is no need to invoke particle-like properties in the unobserved parts of any occurrence, since the wave function ψ will give all the necessary effects" (Darwin, 1929, p. 392). And, thus, the central physical entity was neither the particle nor the wave but the wave function.

The problem was, of course, to understand what exactly the wave function was, and how it could be understood if it was to be something more than a mathematical object. Darwin was keen to see ψ as something real, however unobservable, and not just as a measure of all the possible states of a particular system. And to justify his take, he made a clearly antipositivistic claim using the example that could most hurt any positivistic philosopher: atomic theory. As he conveniently recalled, "it is doubtful whether we should ever have had a theory of relativity or a statistical theory of thermodynamics, if the condition of observability had been imposed when scientists first began to study dynamics or the theory of gases" (p. 392). In the same way atoms were not observable entities 100 years before, the problem of the observability of ψ could be, for the time being, set aside, and we could content ourselves with its *interpretation*. In any case, it was obvious for Darwin that any future visualisation of ψ would come from its relationship with the language of waves, which were, for any *MT* physicist, clearly visualisable.

What was yet the status of this *interpretation*? Darwin saw *interpretation* in physics in total parallelism with the old philosophical problem of the relationship between sign and meaning, between image and concept. He suggested that "we can put the inexplicable feature of the quantum theory, the irreconcilability of wave and particle, in exactly the place where we have got in any case to have an inexplicability, in the transfer from objective to subjective" (p. 393). The gap between the observable and the real was similar to the gap between the subjective and the objective, or, borrowing from Kant's terminology, between the phenomena and the noumena. "Following this line of argument—Darwin (1929) went on—we are led to the conception of a sub-world which contains no mention of observation at all. In this sub-world ψ is an unthinkably complicated function of all the variables associated with all the particles of the world; nothing definite is happening at all, but it expresses simultaneously everything that could possibly happen" (p. 393).

The introduction of what Darwin calls a "sub-world" seems to involve a sort of Leibnizian idea by which reality is constituted by all possible states, not only by those that really take place. It would be our consciousness—our measurements—that actualise one particular state among the many possible:

> The sub-world of ψ expresses in its own way everything that happens; but it is a dead world, not involving definite events, but instead the potentiality for all possible events. It becomes animated by our consciousness, which so to speak cuts sections of it when it makes observations. These observations are described in a language and by means of rules which are foreign to the sub-world; the quantum itself enters for the first time … whereby we can talk of atoms, electrons and light-quanta (Darwin, 1929, pp. 393–394).

Going back to the book *The New Conceptions of Matter*, Darwin (1931) explained the same idea in the more popular style of the book:

> We may describe the relation of wave and particle by saying that the wave does not know how near its particle went to the nucleus, and so cannot tell what sort of a hyperbola it is describing, and therefore the wave must do the best it can by offering all possible alternatives … The point to be emphasised here is that the wave does not tell us what happens, but tells us everything that may happen with the appropriate probability (pp. 150–151).

And later on, to make the analogy clearer:

> if we try to pay attention only to the wave aspect the trouble … is that nothing ever happens. The waves are like the expert advisers of a government, who tell the ministers what will be the consequences of every proposed policy, but … refrain from ever deciding which policy should be chosen; it requires the decision of the minister to change the deliberation into action (p. 155).

The book was met with mixed reactions. Reviewers agreed that it was well written, amenable and, most importantly, clear in what it wanted to convey.[4] But when it came to the actual content, both scientific and philosophical, there were some strong criticisms. Particularly interesting is the review in *Nature*, by the British physicist Herbert Dingle, who rejected the whole programme of Darwin as being excessively naïve, by assuming that any mathematical concept could find a correspondence in the objects of experience. For Dingle (1932), "the whilom correspondence was a merely superficial characteristic, and … its breakdown is not a temporary inconvenience but an indication that … we have [gone] below the surface" (p. 184). As a clear example he mentioned the problems that lay at the core of Darwin's *naturalness* in taking the notion of wave as an object of experience, since these do not exist except in a medium. And, of course, the topic clashed with the old problem of the existence of an electromagnetic ether, a notion which Darwin (1931) could not totally dispense of if he was to give some real representation to the waves of matter:

> In recent times the idea of the aether has taken on a much vaguer and more satisfactory shape, merely as space endowed with certain properties. It has been defined as the "subject of the verb to undulate," and it acts as this subject very well. Its properties are not to be expressed in such crude ideas as elastic jellies, but are merely those things that we require it to do. With the advent of Einstein's relativity, it has often been said that the aether has been abolished. As far as concerns to elastic jellies, that is certainly so—if they still had adherents before relativity—but it is nevertheless useful to have a subject of the verb to undulate, and relativity is from this point of view merely a new and much more fundamental quality of the aether … we are merely going to demand of the aether that it shall be a true universal carrier (p. 23).

This comment helps us understand, once again, that Darwin's *naturalness* is circumscribed by what looked natural to him, a Cambridge wrangler of the last generation.

The book is dedicated "as a tribute of admiration and affection to Niels Bohr, the acknowledged leader among those who have shown how the secrets of natural philosophy may be understood" (Darwin, 1931, dedication); but its contents lie on the antipodes of Bohr's ideas at the time. Darwin and Bohr remained good friends, but their philosophical paths had diverged: the latter had shifted towards a more positivistic interpretation of physics. By dedicating the book to him, Darwin was not only acknowledging his respect and friendship; he was also situating his own opinions in connection with the mainstream of matrix mechanics and, by so doing, hoping for legitimacy.

7. Conclusion

The New Conceptions of Matter conveys optimism. It is designed to present a complete picture of the status of quantum physics, as if a new synthesis had almost completely been achieved with wave mechanics. That synthesis would place the new physics in continuity with previous theories, in the tradition of Newton and Maxwell. To understand this optimism, in this paper I have analysed the way Darwin embraced, developed and explained quantum physics and wave quantum mechanics throughout the 1920s, emphasising the aspects in which there is continuity with his training in the Cambridge *Mathematical Tripos*. Certainly, other aspects in his early career, especially his time in Manchester, where he could learn new theoretical and experimental approaches with Bohr and Rutherford, are important to grasp fully the complexities of his science. In this regard, I do not want to claim that Darwin is the typical or paradigmatic example in the reception of quantum physics in Britain. To do so, one should wait for more work to be done on other British actors in the development of quantum theory in the 1910s and 1920s. The claim of this paper is that Darwin's work in the 1920s can be understood, at least partly, in reference to his early training.

For instance, we have seen in Section 3 that a young Darwin was keen to do away with the strict conservation of energy in a move that could be interpreted as revolutionary, since few British physicists would have dared venture a similar suggestion. However, as I tried to show in that section, his reasons for this dramatic hypothesis stem from other solid commitments that have their origins in the typically MT way of doing physics, rather than from a desire to start physics totally afresh. By that I do not mean that the rejection of the conservation of energy was a typically MT solution, which it clearly was not, but that Darwin's reasons for doing so were consistent with his epistemological and ontological commitments acquired in the MT.

Similar to Warwick's (2003b) analysis of the philosophy of Joseph Larmor, epistemology and ontology were for Darwin two sides of the same coin. As argued in the last sections of the paper, the primacy he attributed to wave mechanics came from the fundamental need that any theory be visualisable. That need was not only pragmatic, but also had deep ontological reasons. Faithful to his MT upbringing, for a theory to be potentially true it had to be visualisable. What could not be imagined could not be real. That is why matrix mechanics could be a very interesting scaffolding to obtain new results, but not the real theory, since it did not account for the thing in itself. The naturalness of a theory could only come from being visualisable, and, therefore, in some sort of continuity with the Newtonian and Maxwellian traditions.

A theory-centred historiography of science could perhaps dispose of most of Darwin's work, since not much of what he did really changed the subsequent conceptual development of quantum mechanics. As I said in the Introduction, his role is usually relegated to the background of the stage on which Dirac and others start working under the new paradigm in the late 1920s. But that does not explain how a whole generation of British scientists was entangled with the new physics. Darwin, and also Fowler and a few others, knew and accepted quantum theory. He was in constant intellectual exchange with Bohr and other scientists in the Continent, and he was aware and understood all that was taking place in theoretical physics, to the extent that, as we saw in his referee reports, he understood and promoted Dirac's work. But there was a seriously philosophical bias that prevented him from accepting matrix mechanics *tout court*: not because it was false or not useful, but because it was not real physics. Darwin's early career may thus help us understand the approach to quantum physics by *wranglers* of the last generation.

[4] I have found reviews on the book in *Nature* (Dingle, 1932), *Physical Review* (Almy, 1932) and *The Analyst* (Jordan Lloyd, 1932).

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