CRITICISM OF DIRAC

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A NEW PERSPECTIVE BY K. STRANG

Dirac and the relativistic wave equation

Schrödinger initially tried to develop a relativistic wave equation (i.e. one that would be invariant under Lorentz transformations and in accordance with the STR) but was unable to do so, partly I believe because of the inchoate notion of 'spin'. Dirac set himself this task.

'In the events leading up to Dirac's relativistic theory of electrons, the introduction of spin was of decisive importance. Even if a theory of spin was not yet worked out, it was inevitably clear in 1926 that spin and relativity had to be integrated if quantum theory could account for the peculiarities in spectroscopy.' [*The Genesis of Dirac's Relativistic Theory of Electrons*, Helge Kragh Science and Society: Quantum History, Vol 4 Routledge] [1]

Once again the camps were divided with Pauli and Heisenberg on the side of mathematical formalism and C. Darwin advocating wave mechanics and treating the spin as a vector wave.

'In contrast with Pauli, Darwin was devoted to wave mechanics. In a series of papers beginning in 1927 he extended Schrödinger's theory to cover spin phenomena, and showed that the anomalous Zeeman effect, as well as other spectroscopic puzzles, could in this way be reproduced. Darwin's programme was "to proceed by empirically constructing a pair of equations to represent the fine-structure of the hydrogen spectrum." This empirical approach, which he shared with Pauli, consisted in expressing the Hamiltonian by a sum of terms including spin effects and first-order relativistic effects, . . . Darwin solved his wave equation by a lengthy mathematical analysis based on spherical harmonics and showed that it gave the correct result . . . The main difference between Darwin's work and Pauli's lay in their different interpretations of the spin wave function. Darwin interpreted the two-component wave function as the electron's "vector wave", to be understood in analogy with a light wave. Pauli objected to Darwin's interpretation, and pointed out that only by introducing spin as an extra coordinate in the two scalar wave functions, could spin be properly incorporated in the quantum mechanical transformation theory. Despite their complete equivalence, Darwin's and Pauli's theories brought about a very different impact on the further development of physics. Historically, Pauli's contribution turned out to be the most important of the two. At the time when the spin theories emerged, each physicist received them in accord with his own stand regarding quantum mechanics. Wave mechanicists preferred Darwin's approach, while most others were in favour of Pauli's way of presenting the problems.' [ibid.]

Schrödinger wrote to Darwin:
'You can easily guess that I am very much interested in your work on the spinning electron and that I infinitely prefer your view of a vector-wave to that of my friend Pauli, whose formalism I am hardly able to understand.' (ibid, Letter from Schrödinger to Darwin, 4 October 1927]

Dirac's approach was to abandon experiment and classical models or theories of any kind:

'…it is more important to have beauty in one's equations than to have them fit experiment. …if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress.' [*ibid*]

and in praise of Pauli's work:

'It consists in abandoning from the beginning any attempt to follow the classical theory. One does not try to take over into the quantum theory the classical treatment of some model, which incorporates the empirical facts, but takes over the empirical facts directly into the quantum theory. The method provides a very beautiful example of the general quantum theory, and shows that this quantum theory is no longer completely dependent on analogies with the classical theory, but can stand on its own feet.' *[ibid*, P.A.M. Dirac, *Lectures on Modern Quantum Mechanics*,; undated, but probably from October 1927.]

and his general outlook, contrary to most of the great philosopher-physicists of the nineteenth century:

'Any physical or philosophical ideas that one has must be adjusted to fit the mathematics. Not the other way round. Too many physicists are inclined to start from preconceived physical ideas, and then try to develop them and find a mathematical scheme that

incorporates them. Such a line of attack is unlikely to lead to success' [P.A.M. Dirac, .]

So, Dirac was not interested in creating a more sophisticated model of the electron and treated it throughout as a point charge.

Dirac's equation was hailed as a great success despite the fact its solution led to negative energies. In classical physics, negative energy solutions are discarded as meaningless, but not so in Quantum mechanics:

'which allows discontinuous transitions between the discrete energy levels which are the solutions of the wave equation' [Malcolm Longair *Quantum Concepts in Physics* p 334]

The difficulty is that Schrödinger argued there were no discontinuous transitions and he was proved correct by experiments written up in Nature 2019 [*To Catch and Reverse a Quantum Jump Mid-Flight* Z.K. Miner and Others, 03/06/2019, pp200-204]

If there are no discontinuous transitions then negative energies are redundant and so too the litany of bizarre features that this result spawned, such as antiparticles, antimatter and infinite energies around the point charge electron in an electric field.[2] My own view is that his arid equation provides nothing illuminating about the nature of reality: quite the reverse, it has obfuscated and retarded the progress of physics for decades. It is based on the incorrect characterisation of both light and matter and as such should be consigned to the waste-paper basket.

In the desire to consign the achievements of classical physics and philosophy to history, and promote their own brand of thinking, this band of knaves sidelined Schrödinger, Einstein and others and developed a mad-hatter version of physics, a second order derivative: the craziness of crazy. Their insistence on mathematical formalism over all other considerations is designed to mask their lack of philosophical imagination, which they then elevate to an article of faith which cannot be challenged.

Conclusions

The pursuit of the particle model has led to inconsistencies, infinities and other absurdities which have had a cumulative effect. Added to which are the many experimental results intended to prove a particular part of the overall theory but relying on unproved assumptions made by other parts of the overall theory. For example the Casimir effect relies on renormalisation for one of the elements in its equation. A thorough list of such examples can be found in Oliver Consa's paper, *Something is wrong in the state of QED*, October 2021. In it he points out how QED survived and Feynman, Schwinger and Tomonaga awarded the Nobel prize despite much trenchant criticism being levelled against QED.

'Another critic was Oppenheimer, as Dyson relates:
"When after some weeks I had a chance to talk to Oppenheimer, I was astonished to discover that his reasons for being uninterested in my work were quite the opposite of what I had imagined. I had expected that he would disparage my program as merely unoriginal, a minor adumbration of Schwinger and Feynman. On the contrary, he considered it to be fundamentally on the wrong track. He thought adumbrating Schwinger and Feynman to be a wasted effort, because he did not believe that the ideas of Schwinger and Feynman had much to do with reality. I had known that he had never appreciated Feynman, but it came as a shock to hear him now violently opposing Schwinger, his own student, whose work he had acclaimed so enthusiastically six months earlier. He had somehow become convinced during his stay in Europe that physics was in need of radically new ideas, that this quantum electrodynamics of Schwinger and Feynman was just another misguided attempt to patch up old ideas with fancy mathematics." [61]

According to Dyson, Fermi also did not agree with this new way of conducting science: "When Dyson met Fermi, he quickly put aside the graphs he was being shown indicating agreement between theory and experiment. His verdict, as Dyson remembered, was "There are two ways of doing calculations in theoretical physics. One way, and this is the way I prefer, is to have a clear physical picture of the process you are calculating. The other way is to have a precise and self-consistent mathematical formalism. You have neither." When a stunned Dyson tried to counter by emphasizing the agreement between experiment and the calculations, Fermi asked him how many free parameters he had used to obtain the fit. Smiling after being told "Four," Fermi remarked, "I remember my old friend Johnny von Neumann used to say, with four parameters I can fit an elephant, and with five I can make him wiggle his trunk." There was little to add."

Nor do the particle physicists solve the ontological problem by re-describing waves as 'ensembles' of particles in order to apply a statistical approach analogous to Brownian Motion and avoid the problem of the location of discrete particles. Max Born highlights the inadequacy of the move from single discrete particles to ensembles of particles:

‘The quantum theoretical description . . . does not answer . . . the question of where a certain particle is at a given time. In this respect the quantum theory is in agreement with the experimentalists, for whom microscopic coordinates are also out of reach, and who therefore only count instances and indulge in statistics. This suggests that quantum mechanics similarly only answers properly-put statistical questions and says nothing about the course of individual phenomena.’ [Max Born, P*hysical Aspects of Quantum* Mechanics, Nature, 119 (1927) p354-357]

The particle physicists insofar as they are obsessed with locating discrete objects at a minute scale are all tilting at windmills. They are quite delusional in thinking that they are adding anything to the corpus of knowledge of ontology. Solving practical problems utilising mathematics is one thing; making claims about ontology something quite different.

Notes: 1 The collection of essays *Quantum Histories* includes an essay by L Wassel which provides a detailed account of how Schröodinger arrived at his prior conclusions on gases and his alternative to the Einstein-Bose statistical method.

2 There is a note on Renormalisation in the additional material section of the essay on Fields