P. A. M. Dirac (8 August 1902–20 October 1984)

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Squat down and, like a child, observe what is happening, be prepared to discard any preconceived notions, stubbornly following the dictates of nature no matter where and how it leads you, otherwise you will not learn anything.

(Retranslated from the Russian translation of a passage from Thomas Hardy).

The outwardly calm life of P. A. M. Dirac was actually filled with adventures no less interesting than those that fall to the lot of treasure-hunters. Only these adventures were hidden from the uninitiated; for it is ony people close to physics who come to know how dramatic are its triumphs and how laborious is the road to them.

The labors of a scholar are seldom comprehensible to the outside observer, and many discoveries in natural science are made in areas far removed from day-to-day life; however, their impact on culture, technology and the evolution of human history often turns out to be significantly broader than might appear to the discoverer's contemporaries. It would not be an exaggeration to say that revolutions in science and technology, about which so much has been said, originate almost always with men at work in quiet studies, whose early results—records of experiments or pages of formulae—could have excited only a few others.

Dirac was one of those who laid the foundations of quantum physics. In the beginning, this was the most abstract of sciences; now, its practical applications are innumerable. The work of Dirac is perhaps the most abstract of all, but in it is found the basis of profound ideas, whose astonishing elaboration had to await the passage of many years. Many books[1)] have been written about the history of quantum mechanics; in them the roles of the participants in the great events of the time are described in detail. Each of them thought and worked in his own way. Dirac was especially distinguished by his understanding of physics. In these remarks we will dwell on the originality of Dirac.

We will limit ourselves only to a few illustrations of the way Dirac's mind work, that is to say, the psychology of his creativity, leaving aside the technical side of the history of science. Our presentation is based on his numerous lectures and interviews, in which he turned to those moments in his life when the solution to a difficult problem was revealed to him.

Let us begin, following the advice of Lewis Carroll's king, at the beginning.

On 28 July, 1925 Werner Heisenberg came to Cambridge, where he gave his paper "Anomalies in the Zeeman Effect" at the "Kapitza Club" (in the minutes of this club meeting, the title is given as "Zoology of Spectral Terms and Zeeman Botany").

P. A. M. Dirac, about 1930
(photograph from the archives of P. L. Kapitza)

The P. L. Kapitza Club played a large part in the intellectual life of Cambridge. It was opened on October 17, 1922; since that day almost all the physics problems of that era had been discussed at its meetings. The well-known English physicist J. Bernal remarked on the Kapitza Club, "It was a kind of grand inquisition on all important questions in physics; men with great names were 'summoned', heckled, and interrupted; Kapitza did most of the heckling, but no one minded because of his enthusiasm," (Ref. 3, p. 75). The meetings of the club took place on Tuesdays after dinner. In 1924, Dirac became a member of the club.[2)]

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The participation of Dirac in this club led to his friendship with P. L. Kapitza, and later on to a working collaboration with him. Their 1933 paper "The reflection of electrons from standing light waves"[6] is well-known. This work on the inverse Compton effect has been applied in our own time to the theory of processes observed in colliding beams. Less well-known is some experimental work on isotope separation into a rotating stream of gas, which was begun and then interrupted almost at its inception by the departure of P. L. Kapitza for Moscow.

But, we have digressed from our topic—the Heisenberg report.

In this report, Heisenberg emphasized the necessity for a new dynamic theory of the atom. In a conversation with Fowler, who was the scientific advisor of Dirac in Cambridge, Heisenberg also mentioned his own new results. Fowler asked Heisenberg to send him proofs of his paper, and in the middle of August 1925, Heisenberg's article arrived in the mail at Cambridge. Fowler assigned Dirac to analyze it.

In brief summary this is the prologue to Dirac's entrance into the new mechanics.

Dirac noted in Heisenberg's ideas the aspect which to Heisenberg himself appeared to be a difficulty of the theory rather than its triumph—the astonishing fact of noncommuting variables.[3)] "While classically $x(t)y(t)$ always equals $y(t)x(t)$, in quantum theory this is in general not the case," writes Heisenberg in his article.[7] In one of his lectures, Dirac remarks: "...I saw that the noncommutation was really the dominant characteristic of Heisenberg's theory...so I was led to concentrate on the idea of noncommutation and to see how the oridinary dynamics which people had been using until then should be modified to include it. (Ref. 3, p. 129).

In Heisenberg's formulation, the new method appeared to be limited. The first Heisenberg theory referred only to one-dimensional oscillators (although they could also be anharmonic). To solve the next fundamental problem—calculating the atomic levels of hydrogen—Pauli devised a special method (which was transformed in the hands of Fock into a beautiful theory based on four-dimensional symmetry).

It was only in Schrödinger's four papers "Quantization as a problem in eigenvalue theory. I-IV"[8] (the first of which was submitted on 27 January and published on 13 March 1926) that the solution to problems of the motion of particles in an arbitrary potential was presented.

After Heisenberg's paper, it still remained unclear how one should write down the general dynamic equations of the new theory.

In order to understand how one should deal with the new dynamic variables—the coordinate q and the momentum p, which are subject to the quantum conditions

$$pq - qp = \frac{h}{2\pi i}$$

(this condition first appears in a paper by M. Born and P. Jordan[9]) it was first of all necessary to understand what corresponded to this condition in classical dynamics.

The idea for the solution came in September of 1925. While on his customary Sunday walk, Dirac remembered about Poisson brackets, and on the next day, "I looked up Poisson brackets in Whittaker's 'Analytical Dynamics', and I found that they were just what I needed" (Ref. 5, p. 122).

The paper "The fundamental equations of quantum mechanics" by Dirac,[10] was submitted by Fowler on November 7 to the "Proceedings of the Royal Society of London", and was published less than a month later—1 December of the same year.[4)]

With this paper, we mark the beginning of a quantum dynamics based on Hamilton's method, which brought together in a natural way both the quantum theory of Heisenberg and the wave mechanics of Schrödinger. At the beginning of 1926, physicists were perplexed by the very possibility of there being two theories which at first glance seemed so different. Schrödinger gave a proof of their equivalence (in Ref. 11, submitted on March 18 and published on May 4, 1926). We note that Lanczos arrived at something very close to wave mechanics in an almost unnoticed paper (submitted already on 22 December 1925, and published on February 26, 1926), while, independently of Schrödinger, Pauli also gave a proof of the equivalence of the two theories in a letter to Jordan on April 12, 1926 (published only in 1973), and to Eckhart at the end of May 1926 (see, e.g., Ref. 4, p. 693).

In the form of the theory as presented by Dirac in his papers, no such problem with equivalence arose: the Heisenberg and Schrödinger pictures were simply different representations (i.e., equations written in different coordinate systems) of the same set of dynamic laws for the mechanics of noncommuting variables.

Yet another comment from Dirac: "At this stage I had an advantage over Heisenberg, because I didn't share his concerns. I did not have this fear of the whole theory collapsing. Its failure would not dumbfound me to the extent that it might dumbfound Heisenberg...I think that as a general rule the author of a new idea turns out to be far from the best candidate for developing this idea: the fear that something may be amiss can become too strong, and this fear prevents him from looking at a new method from an independent, external point of view, as the course of development requires" (Ref. 5, p. 121). But Dirac himself experienced a similar feeling, which made him hasten to publish the relativistic equation of the electron. In this work he limited himself to only the first approximation in the hydrogen atom problem. "You may wonder," he wrote 50 years later, "why I did not immediately go on to consider the higher approximations, but the reason is that I was really scared to do so. I was afraid that, in the higher approximations, the results might not come right, and I was so happy to have a theory that was correct in the first approximation, that I wanted to consolidate this success by publishing it in that form, without going on to risk a failure in the higher approximations. The higher approximations were worked out later by Darwin, who wrote and told me of his results, and I was very happy to hear that they agreed with observation." (Ref. 5, p. 143). Very likely the wise old scholar gives a correct explanation for the actions of the young scholar.

The opinions and convictions of a scholar are better revealed not in his comments on himself, but rather when he does justice to others. Dirac made a remarkable statement at one of the numerous symposia dedicated to the 100th birthday of Einstein, which was celebrated around the world in the fall of 1979. At this symposium in Munich (18–20 September), dedicated to the theme of "The impact of modern scientific ideas on society," Dirac was a speaker.[14]

In particular, he recounted how in 1906 the German physicist Kaufmann reported on the results of his measurements concerning the dependence of the electron mass on velocity. There were two models under discussion: the Lorentz-Einstein relativistic model (a model of an electron undergoing longitudinal compression while moving) and the Abraham model, in which the electron was represented by a solid sphere. Kaufmann explained that his experiments confirmed the Abraham model.

Dirac continued: "When Lorentz heard about these results, he was completely knocked off his track. He exclaimed, 'All my work has gone for nothing!' "[5)] Poincare perceived this to be a limitation on the transformation scheme. When Einstein heard about this, his reaction was entirely different. Einstein felt that his theory was mathematically so beautiful that it simply had to be correct, and if the experiments said otherwise, one had to wait and see: perhaps there was something wrong with the experiment. So, Einstein was not excited. He was firmly convinced of the correctness of his point of view, and adopted an attitude of suspended judgment toward experiment.

After a few years, the experiment was repeated, and the new results turned out in favor of Lorentz and Einstein- ...Einstein's position turned out to be correct. This position was characteristic of him; it demanded greater faith in his fundamental ideas, if only they were based on clear and beautiful mathematics, than in the results of experiment. Experimentalists always overestimate their results and are inclined to make errors. One should not allow them to confuse matters too much.[14]

In this story of Dirac's we see his scientific and life credo, which he never tired of repeating in various forms. When Dirac spoke of his work, it seemed to the listener that he was never so much explaining the existing world but, as a creator, fabricating his own, beautiful, mathematically rigorous version of it. Only at the end would he return to reality. Comparing his world with the real world, Dirac occasionally came up against the sort of unexpected obstacles which others would consider a destructive blow to the theory. But this specifically was not characteristic of Dirac.

A most remarkable story, in which Dirac's character appears in all its strength, is the story of the discovery of the equation which bears his name.

At the Solvay conference in October of 1927, Bohr approached Dirac. From here we quote Dirac himself: "Bohr came up to me and asked me, 'What are you working on now?' I said, 'I am trying to get a relativistic theory of the electron,' Then Bohr said, 'But Klein has already solved this problem.' I was a bit taken aback by this. I began to explain that Klein's solution of the problem, based on the Klein-Gordon equation, was not satisfactory because it could not be fitted in with my general physical interpretation for quantum mechanics. However, I was not able to explain very much to Bohr before the start of the lecture interrrupted our conversation, and I had to leave the question rather in the air" (Ref. 5, p. 121).

The point was that Dirac did not like the fact that the Klein-Gordon equation was of second order and did not admit a probabilistic interpretation. And although many thought that the problem had been solved, and that what was considered to be a probability density should be interpreted as a charge density, Dirac was not satisfied and set out to obtain an equation for a single electron rather than for a system of particles with different charges. He finally got the equation he wanted; its solutions, however, astonished him: "I found this equation predicted that the electron possessed a spin equal to 1/2, and a magnetic moment, and that the values of the spin and magnetic moment agreed with experiment. These results I had obtained were completely unexpected: I had assumed that the simplest solutions to my equation would describe spinless particles, and that spin would have to be introduced a posteriori..."[15]

Actually, Dirac had obtained an equation for a spin 1/2 electron from the requirement that such an equation should contain first and not second derivatives in time. There was a price to be paid for this success.

In order to decompose a sum of four squares into two linear factors, it was necessary to introduce matrices with 4 rows and 4 columns. The two-dimensional Pauli matrices, which described spin in the non relativistic case, clearly did not suffice. In Dirac's new theory the electron had an extra degree of freedom—the freedom, as it turned out, to make transitions to negative-energy states. This idea appeared to be so wild that one might have to repudiate all that had been accomplished. But Dirac found another way out—he chose to believe in the reality of the negative-energy states, and taking advantage of the Pauli principle he filled all such states with electrons, explaining that only the empty states—"holes"—could be observed in experiments. "Now, that was a serious difficulty. At that time, we had electrons carrying negative charge), and we had the protons carrying positive charge, and everyone felt pretty sure electrons and protons were the only elementary particles in Nature. It is true that Rutherford had sometimes considered the possibility of a neutron rather wistfully. He said it would be so useful for the experimenters if these neutrons did exist because they would provide ideal projectiles to shoot into atomic nuclei. They would not be disturbed at all by the electrons outside. People did not really have much faith in the existence of neutrons. It seemed to everyone self-evident that as there were just two kinds of electricity, there should be just two kinds of particles to carry them. People did not go beyond that. (Ref. 5, p. 144).

The actual course of development is well known. Pauli and Weyl showed that the mass of a hole must be the same as that of an electron, and, therefore, holes could not be protons. The situation was critical.

Dirac continued: "However, I did not want to abandon my theory altogether, and so I put it forward as a theory of electrons and protons.

Oppenheimer put forward a theory that the holes did have the same mass as the electrons, but there was some special reason in Nature why they were never observed. He could not say what this special reason was, but he just put it forward as something still to be explained. Oppenheimer was really close to the mark. These holes were particles with the same mass as the electron, and they had never been observed simply because experimenters had never looked for them in the right place.

I remember that, during my attendance at lectures given by experimenters in the Cavendish, there was one occasion, I am not quite sure whether it was 1926 or 1927, when, in the discussion after the lecture, the lecturer pointed out a rather curious fact which he had observed in his experiments. His experiments were done with tracks of particles in a Wilson chamber, in the presence of a magnetic field, and so they were all curved. Then if one knows the charge on a particle, one knows which way it is going. He was assuming that the particles had to be electrons, and then the curvature of the tracks indicated that they were moving into the source.

It was just mentioned casually. Nobody thought of examining this point in greater detail, but if they had examined it they would have been led to an important discovery...That just goes to show how an important discovery may be missed through people not attaching sufficient importance to something which they look upon as a curiosity and not worth further examination" (Ref. 5, p. 145).

One can also mention the converse: when an experimentalist believes in the correctness of his experiments, then a phenomenon which is not understood sometimes turns into a discovery. Good examples of such discoveries are Vavilov-Cherenkov radiation and the Mössbauer effect.

A few years later, Blackett observed positively charged particles, but he delayed publication, considering it to be necessary to make further experiments. Anderson was more courageous. He published his results (very similar to Blackett's results) and received the Nobel prize. An instructive story!

The Dirac equations were published on February 1, 1928 (the paper "The Quantum Theory of the Electron" was submitted on January 2, 1928,[16] and its second part was submitted a month later on February 2, 1928; it appeared in print on March 1, 1928.[17]

In 1930, Dirac published the first edition of "The Principles of Quantum Mechanics", which he revised three times.[18] From edition to edition, the author's view of the logical structure of quantum mechanics changed, and a comparison of all four editions gives a very interesting picture of how his ideas evolved.[6)]

Dirac's first edition was met guardedly; the new language of the author was unusual, while the rigor seemed excessive to many. A reviewer wrote that "the author bids us throw aside preconceived ideas regarding the nature of phenomena...We may describe this as the application of "pure thought" to physics.[20]

Even Heisenberg wrote concerning the German translation of Dirac's book: "With respect to several points, this reviewer was left with the impression that Dirac probably had presented quantum mechanics, in particular its physical content, in a more 'symbolic' way than is necessary."[21]

This view of Dirac's method persisted for a long time. It was only the subsequent development of quantum electrodynamics and, in particular, of new directions such as quantum chromodynamics and supersymmetry, that demonstrated the inevitability of the faith in the power of the methods of Hamiltonian mechanics utilized by Dirac not only in the papers mentioned above but also in the development of quantum statistics and in the theories of relativistic quantum fields. Dirac's book has entered into the library of classical physics texts, and ranks with the "Principia Mathematica" of Newton and "A Treatise on Electric and Magnetic Fields" of Maxwell, differing from them only in being more accessible to the contemporary reader. The distinctiveness of Dirac's book lies in its use of a new language, which has become the basic language of twentieth-century physicists. Words such as observable, commutation, the well-known "h bar" (the Serbian letter $\hbar$), the delta-function,[7)] the bracket notation for observables and matrix elements, operations such as encircling poles in the complex plane for fourier-transformed amplitudes, the $\delta_{\pm}$ functions and even functional integrals, all came to us from Dirac.

The elaboration of this new language (especially in the works of Feynman and Dyson) imparts to the journals and books of our time that uniqueness which distinguishes them from books and journals of the previous century. Here we can see an analogy with art, whose language always reflects the changes in human perception, and in its turn transforms these perceptions.

Probably the principal change in the language of physics after Dirac has been the penetration into it of diagrams, graphs which, like hieroglyphics, define not words but concepts which are common to phenomena which are occasionally quite far from one another. In this is revealed the beauty of both physics and of mathematics, the significance of which Dirac always affirmed.

Just as anyone, even a great man, Dirac could be mistaken. "I would consider the theory of complex variables a very beautiful theory, because of the great power that one has with Cauchy integrals. The same I feel with projective geometry, but not with some other branches of mathematics, such as the theory of sets and topology" (Ref. 3, p. 118).

This evaluation now seems naive to us. Topology in the theory of liquid helium and Cantor sets in the theory of nonlinear systems attest to the power and beauty of these disciplines. Any statement about the beauty of mathematids cannot be divorced from subjectivity, and is subject to the influence of time. And in this also, science is related to art. In this sense, Dirac's viewpoint is reminiscent of that of a painter or a poet. But nevertheless let us listen to what Dirac said later: "A beautiful theory has universality, and power to predict, to interpret, to set up examples and to work with them. Once you have the fundamental laws and you want to apply them, you don't need the principle of beauty any more, beause in treating practical problems one has to take into account many details and things become messy anyway." (Ref. 3, p. 118).

We conclude with a last somewhat unexpected quote: "I was always much interested in the beauty of mathematics, and this introduction to me of projective geometry stimulated me very much and provided, I would say, a lifelong interest...Projective geometry is a most useful tool for research, but I did not mention it in my published work. I do not think I have ever mentioned projective geometry in my published work (but I am not sure about that) because I felt that most physicists were not familiar with it. When I had obtained a particular result, I translated it into an analytic form and put down the argument in terms of equations...That applied also to my later work on spinors. One had quite a new kind of quantity to deal with; but for discussing the relationships between spinors, again, the ideas of projective geometry are very useful" (Ref. 5, p. 114).

Even a few years back these words would have led to confusion among physicists (they were pronounced in 1972). The appearance of twistor calculus and projective spaces in field theory has once again confirmed the prophetic gift of Dirac.

Reading Dirac, one succumbs to the power of human cognition, and one perceives the beauty of the physical world whose passionate troubadour was a most unusual man: Paul Adrian Maurice Dirac.

1)Of these we must single out three: Hund[1], Jemmer[2] and the multi-volume work by Mehra and Reichenberg[3], which served us as an important source. It is also helpful to acquaint oneself with the issue of Usp. Fiz. Nauk[4] dedicated to the 50th anniversary of quantum mechanics, in which were published reviews and translations of classical articles.

2)Dirac recalls: "That was not really a very convenient time for me because I was usually rather sleepy after dinner. I did my work mostly in the morning. Mornings I believe are the times when one's brain power is at its maximum, and towards the end of the day I was more or less dull, especially after dinner. I was not in the best frame of mind for taking in new information. But still it was well worth-while going to these meetings of the Kapitza Club." (Ref. 5, p. 118).

3)The term "commutation relations" was invented by Dirac; it replaced the term "permutation relations" which was then current. He thought the term "permutation" was used by physicists with reference to permutations of coordinates in many-body theory.

4)Let us not fail to note that intensive mutual contact, rapid mail service and (from our standpoint) unusually rapid publication were at least important if not absolutely necessary conditions for several physicists located in different cities to work effectively.

5)"Je suis donc au bout de mon latin" (in a letter to Poincare on Mar. 8 1906; see Ref. 13).

6)There are translations of three of these editions—the first, the second and the fourth—into Russian, a most uncommon occurrence in our publishing practices.

7)A concept close to the delta-function was introduced by Kirchhoff, and later by Heaviside and Hertz. However, most people have managed to forget about this.

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