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Remarks on the History of the Exclusion Principle

Wolfgang Pauli

Institute for Advanced Study, Princeton, New Jersey

THE HISTORY OF THE DISCOVERY of the exclusion principle, for which I have received the honor of the Nobel Prize award this year, goes back to my student days in Munich. While, in school in Vienna, I had already obtained some knowledge of classical physics and the then-new Einstein relativity theory, it was at the University of Munich that I was introduced by Sommerfeld to the structure of the atom—somewhat strange from the point of view of classical physics. I was not spared the shock which every physicist accustomed to the classical way of thinking experienced when he came to know of Bohr's "Basic postulate of quantum theory" for the first time. At that time there were two approaches to the difficult problems connected with the quantum of action. One was an effort to bring abstract order to the new ideas by looking for a key to translate classical mechanics and electrodynamics into quantum language which would form a logical generalization of these. This was the direction which was taken by Bohr's Correspondence Principle. Sommerfeld, however, tried to overcome the difficulties which blocked the use of the concepts of kinematical models by a direct interpretation of the laws of spectra in terms of integral numbers, following, as Kepler once did in his investigation of the planetary system, an inner feeling for harmony. Both methods, which did not appear to me irreconcilable, influenced me. The series of whole numbers 2, 8, 18, 32 . . . , giving the lengths of the periods in the natural system of chemical elements, was zealously discussed in Munich, including the remark of the Swedish physicist, Rydberg, that these numbers are of the simple form $2n^2$ if "n" takes on all integer values. Sommerfeld tried especially to connect the number 8 and the number of corners of a cube.

A new phase of my scientific life began when I met Niels Bohr personally for the first time. This was in 1922, when he gave a series of guest lectures at Göttingen in which he reported on his theoretical investigations on the periodic system of elements. I shall recall only briefly that the essential progress made by Bohr's considerations at that time was in explaining by means of the spherically symmetric atomic model the formation of the intermediate shells of the atom, and the general properties of the rare earths. The question as to why all electrons for an atom in its ground state were not bound in the innermost shell had already been emphasized by Bohr as a fundamental problem in his earlier works. In his Göttingen lectures he treated particularly the closing of this innermost K shell in the helium atom and its essential connection with the two noncombining spectra of helium, the ortho- and parahelium spectra. However, no convincing explanation for this phenomenon could be given on the basis of classical mechanics. It made a strong impression on me that Bohr at that time and in later discussions was looking for a general explanation which should hold for the closing of every electron shell and in which the number 2 was considered to be as essential as 8 in contrast to Sommerfeld's approach.

During these meetings in Göttingen Bohr came to me one day, accompanied by his assistant, Oskar Klein (now professor in Stockholm), and asked me whether I could come to him in Copenhagen for a year. He needed a collaborator in the editing of his works, which he wanted to publish in German. I was much

Professor Wolfgang Pauli was honored for the award of the Nobel Prize in physics at a dinner on 10 December 1945 at the Institute for Advanced Study, Princeton. Dr. Frank Aydelotte, director of the Institute, welcomed the guests and called on Professor Hermann Weyl to offer a toast, which was seconded by Professor Albert Einstein and Erwin Panofsky. Professor Pauli then gave the principal address.

Science prints an amplified version of Professor Pauli's address together with somewhat shortened accounts of Dr. Aydelotte's introduction and Professor Weyl's encomium.

surprised, and after considering a little while I answered with that certainty of which only a young man is capable: "I hardly think that the scientific demands which you will make on me will cause me any difficulty, but the learning of a foreign tongue like Danish far exceeds my abilities." The result was a hearty burst of laughter from Bohr and Klein, and I went to Copenhagen in the fall of 1922, where both of my contentions were shown to be wrong. The first words I learned were the integers. The way in which such simple numbers as 50, 70, and 90 are expressed in Danish—in a complicated fashion as half-multiples of 20—particularly impressed me, but I understood the idea and could easily recognize the words. But the half-integers used by Landé as magnetic quantum numbers to explain the anomalous Zeeman effect presented much greater difficulties for me. "Anomalous Zeeman effect" refers to a type of splitting of the spectral lines in a magnetic field which is different from the normal triplet. The anomalous type of splitting was on the one hand especially fruitful because it exhibited beautiful and simple laws, but on the other hand it was hardly understandable, since very general assumptions concerning the electron, using classical theory as well as quantum theory, always led to the simple triplet. A closer investigation of this problem left me with the feeling that it was even more unapproachable. A colleague who met me strolling rather aimlessly in the beautiful streets of Copenhagen said to me in a friendly manner, "You look very unhappy"; whereupon I answered fiercely, "How can one look happy when he is thinking about the anomalous Zeeman effect?" I could not find a satisfactory solution at that time, but succeeded, however, in generalizing Landé's analysis for the simpler case (in many respects) of very strong magnetic fields (*Z. Phys.*, 1923, **16**, 155). This early work was of decisive importance for the finding of the exclusion principle.

When in 1923 Bohr made his first trip to the United States, I returned, as an assistant, to the University of Hamburg, where soon afterwards I gave my inaugural lecture as *Privatdozent* on the periodic system of elements. The contents of this lecture appeared very unsatisfactory to me, since the problem of the closing of the electronic shells had been clarified no further. The only thing that was clear was that a closer relation of this problem to the theory of multiplet structure must exist. I therefore tried to examine again critically the simplest case, the doublet structure of the alkali spectra. I arrived at the result that the point of view then orthodox—according to which a finite angular momentum of the atomic core was the cause of this doublet structure—must be given up as incorrect. In the fall of 1924 I published some of my arguments (*Z. Phys.*, 1925, **31**, 373) that, instead of the angular momentum of the closed shells of the atomic core, a new quantum theoretic property of the electron had to be introduced which I called "a two-valuedness not describable classically." At this time a paper by the English physicist, Stoner, appeared (*Phil. Mag.*, 1924, **48**, 719), containing not only improvements in the classification of electrons in subgroups, but also the essential remark that the number of energy levels of a single electron in the alkali metal spectra for a given value of the principle quantum number in an external magnetic field is the same as the number of electrons in the closed shells of the rare gases which corresponds to this principal quantum number. On the basis of my earlier results on the classification of spectral terms in a strong magnetic field the general formulation of the exclusion principle became clear to me. The fundamental idea can be formulated in the following way: The complicated numbers of electrons in closed subgroups reduce to the simple number *one* if the division of the groups by giving the values of the 4 quantum numbers of an electron is carried so far that every degeneracy is removed. A single electron already occupies an entirely nondegenerate energy level. The exposition of this general formulation of the exclusion principle was made in Hamburg in the spring of 1925 (*Z. Phys.*, 1925, **31**, 765) after I was able to verify some additional conclusions during a visit to Tübingen, with the help of the spectroscopic material assembled there.

If one pictures by boxes the nondegenerate states of an electron in an atom, the exclusion principle maintains that a box can contain no more than one electron. This, for example, makes the atoms much larger than if many electrons could be contained in the innermost shell. Quantum theory maintains that other particles such as photons or light particles show opposite behavior; that is, as many as possible fill the same box. One can call particles obeying the exclusion principle the "antisocial" particles, while photons are "social." However, in both cases sociologists will envy the physicists on account of the simplifying assumption that all particles of the same type are exactly alike.

With the exception of experts on the classification of spectral terms, people found it difficult to understand the exclusion principle, since no meaning in terms of a model was given to the fourth degree of freedom of the electron. The gap was filled by Uhlenbeck and Goudsmit's idea of electron spin, which made it possible to understand the anomalous Zeeman effect. Since that time the exclusion principle has been closely connected with the idea of spin. Although at first I strongly doubted the correctness of this idea because of its classical mechanical character, I was finally converted to it by Thomas' calculations on the magnitude of doublet splitting. On the other hand, my earlier doubts as well as the cautious expression, "classically nondescribable two-valuedness," experienced a certain verification during later developments, as Bohr was able to show on the basis of wave mechanics that the electron spin cannot be measured by classically describable experiments (as, for instance, deflection of molecular beams in external electromagnetic fields) and must therefore be considered as an essentially quantum mechanical property of the electron.[1]

This is not the place to go into the details of the subsequent developments. On the one hand, the validity of the exclusion principle for all elementary particles of spin ½ was shown (for example, not only for electrons but also for neutrons and protons). This gave the principle a more general and universal meaning, and it found application to the problem, still not completely solved, of nuclear structure. On the other hand, the exclusion principle could not be deduced from the new quantum mechanics and wave mechanics, but remains an independent principle which excludes a class of mathematically possible solutions of the wave equation. This excess of mathematical possibilities of the present-day theory, as compared with reality, is in my opinion one of several indications that in the region where it touches on relativity theory, quantum theory has not yet found its final form.

The history of the exclusion principle is thus already an old one, but its conclusion has not yet been written. The essential advance of physics rests on the creative imagination of the experimental as well as the theoretical investigator, and, contrary to expensive applications of known principles, cannot be forced by planning on a grand scale. Therefore it is not possible to say beforehand where and when one can expect the further development of the basic principles of present-day physics, of which the problem of the exclusion principle is a part. We know, however, that this further development can take place only in the same atmosphere of free investigation and unhampered exchange of scientific results between nations that existed at the time of the disclosure of the exclusion principle. I am therefore very glad to be able to give this short historical survey here in Princeton's Institute for Advanced Study, which in the difficult years of the war, by support of pure and free research irrespective of applications, made it possible for me and others to continue our scientific work.

[1] See Rapport du Sixieme Conseil Solvey de Physique, Paris, 1932, pp. 217–225.

Introductory Remarks

Frank Aydelotte, *Director*

Institute for Advanced Study, Princeton, New Jersey

IT GIVES ME GREAT PLEASURE, ladies and gentlemen, to welcome you on behalf of the Trustees and Faculty of the Institute for Advanced Study on this happy occasion. The awarding of a Nobel Prize to a newly appointed member of our Faculty is just the kind of endorsement of our choice that we value and that we ought to expect. In one sense Pauli's is our fourth Nobel Prize: one member of our Faculty, Einstein; one former member of our Board of Trustees, Carrel; and one former member of the Institute, Rabi, have already received Nobel Prizes. That, however, is a little like the statement in the Believe-It-or-Not cartoon that Lindberg was the sixty-fourth person to fly across the Atlantic ocean. In a real sense Pauli is our first, just as Lindberg was the first to succeed in the design which he attempted.

About one-seventh of the 215 Nobel Prizes so far awarded have gone to citizens of the United States. In view of the size of our educational organization, that is not too many—indeed, it is not enough. We cannot yet claim Pauli's as an American achievement; the exclusion principle was formulated by him twenty years ago while he was still an Austrian. We hope that in a few weeks we shall have the pleasure of welcoming Dr. and Mrs. Pauli to American citizenship.

It is fitting that Pauli should become an American citizen and a member of the Faculty of the Institute for Advanced Study. American civilization, American scholarship, American art and letters, are products of the great European tradition. Our task is not to separate ourselves from that tradition but to support it and to enrich it by the productive work of American scholars. In these days when all men's minds in every field of endeavor are disturbed by the conflict between individualism and collectivism, we may be thankful for the fact that our scholarly tradition is one of individualism. The Nobel Prizes are one evidence of that philosophy. The organization of the Institute for Advanced Study is another. The fundamental plan of this institution is to provide opportunity for individual effort, not for what is called planned research. Our newspapers and magazines,