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Fresh Fields (1926-1927)

If I were asked what was Christopher Columbus' greatest achievement in discovering America, my answer would not be that he took advantage of the spherical shape of the earth to get to India by the western route—this idea had occurred to others before him—or that he prepared his expedition meticulously and rigged his ships most expertly—that, too, others could have done equally well. His most remarkable feat was the decision to leave the known regions of the world and to sail westward, far beyond the point from which his provisions could have got him back home again.

In science, too, it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hazardous leap forward. With his relativity theory, Einstein had abandoned the concept of simultaneity, which was part of the solid ground of traditional physics, and, in so doing, outraged many leading physicists and philosophers and turned them into bitter opponents. In general, scientific progress calls for no more than the absorption and elaboration of new ideas—and this is a call most scientists are happy to heed. However, when it comes to entering new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do. How great their reluctance could be had been brought home to me at the Leipzig Congress, and I fully expected that similar obstacles would be placed in the path of atomic physics. *

During the first few months of 1926, at about the same time that I delivered my lecture in Berlin, Göttingen first became

familiar with the work of the Viennese physicist, Erwin Schrödinger, who was approaching atomic theory from an entirely fresh side. The year before, Louis de Broglie in France had drawn attention to the fact that the strange wave-particle dualism which, at the time, seemed to prevent a rational explanation of light phenomena might be equally involved in the behavior of matter, for instance of electrons. Schrödinger developed this idea further and, by means of a new wave equation, formulated the law governing the propagation of material waves under the influence of an electromagnetic field. In Schrödinger's model, the stationary states of an atomic shell are compared with the stationary vibrations of a system, for instance of a vibrating string, except that all the magnitudes normally considered as energies of the stationary states are treated as frequencies of the stationary vibrations. The results Schrödinger obtained in this way fitted in very well with the new quantum mechanics, and Schrödinger quickly succeeded in proving that his own wave mechanics was mathematically equivalent to quantum mechanics; in other words, that the two were but different mathematical formulations of the same structures. Needless to say, we were delighted by this new development, for it greatly strengthened our confidence in the correctness of the new mathematical formulation. Moreover, Schrödinger's procedure lent itself readily to the simplification of calculations that had severely strained the powers of quantum mechanics.

Unfortunately, however, the physical interpretation of the mathematical scheme presented us with grave problems. Schrödinger believed that, by associating particles with material waves, he had found a way of clearing the obstacles that had so long blocked the path of quantum theory. According to him, these material waves were fully comparable to such processes in space and time as electromagnetic or sound waves. Such obscure ideas as quantum jumps would completely disappear. I had no faith in a theory that ran completely counter to our Copenhagen conception and was disturbed to see that so many physicists greeted precisely this part of Schrödinger's doctrine with a sense of liberation. The many talks I had had with Niels Bohr, Wolfgang Pauli and many others over the years had convinced me that it was impossible to build up a descriptive time-space model of

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interatomic processes—the discontinuous element Einstein had mentioned to me in Berlin as a characteristic feature of atomic phenomena saw to that. Admittedly, this was no more than a negative feature, and we were still a long way from a complete physical interpretation of quantum mechanics, yet we were certain that we must get away from the idea of objective processes in time and space.

Now Schrödinger's interpretation—and this was its great novelty—simply denied the existence of these discontinuities. Thus when an atom passes from one stationary state to the next, it was no longer said to change its energy suddenly and to radiate the difference in the form of an Einsteinian light quanta. Radiation was the result of quite a different process, namely, of the simultaneous excitation of two stationary material vibrations whose interference gives rise to the emission of electromagnetic waves, e.g., light. This hypothesis seemed to me too good to be true, and I mustered what arguments I could to show that discontinuities were a fact of life, however inconvenient. The simplest argument was, of course, Planck's radiation formula, whose empirical correctness no one could doubt and which, after all, had led Planck to his discrete energy quanta.

Toward the end of the 1926 summer term, Sommerfeld invited Schrödinger to address the Munich seminar. I had been working in Copenhagen once again and had familiarized myself with Schrödinger's methods by applying them to the study of the helium atom. I had finished the work while taking a brief holiday on Lake Mjösa in Norway, had stuffed the manuscript into my rucksack and had set out on unmade paths from Gudbrandsdal, across several mountain chains, to Sogne Fjord. After a short stay in Copenhagen, I finally went on to Munich, where I intended to spend the rest of the vacation with my parents—and so I could be present at Schrödinger's lecture, and discuss his theory with him in person. The audience included the director of the Institute for Experimental Physics in the University of Munich, Wilhelm Wien, who was extremely skeptical of Sommerfeld's "atomisticism."

Schrödinger first of all explained the mathematical principles of wave mechanics by using the hydrogen atom as an illustration. All of us were delighted to see his elegant and simple solution by

conventional methods of a problem that Wolfgang Pauli had been able to solve only with great difficulty using quantum mechanics. Unfortunately, Schrödinger went on to discuss his own interpretation of wave mechanics, and his arguments left me quite unconvinced. During the subsequent discussion, I therefore raised a number of objections, and, in particular, pointed out that Schrödinger's conception would not even help explain Planck's radiation law. For this I was taken to task by Wilhelm Wien, who told me rather sharply that while he understood my regrets that quantum mechanics was finished, and with it all such nonsense as quantum jumps, etc., the difficulties I had mentioned would undoubtedly be solved by Schrödinger in the very near future. Schrödinger himself was not quite so certain in his own reply, but he, too, remained convinced that it was only a question of time before my objections would be removed. My arguments had clearly failed to impress anyone—even Sommerfeld, who felt most kindly toward me, succumbed to the persuasive force of Schrödinger's mathematics.

And so I went home rather sadly. It must have been that same evening that I wrote to Niels Bohr about the unhappy outcome of the discussion. Perhaps it was as a result of this letter that he invited Schrödinger to spend part of September in Copenhagen. Schrödinger agreed, and I, too, sped back to Denmark.

Bohr's discussions with Schrödinger began at the railway station and were continued daily from early morning until late at night. Schrödinger stayed in Bohr's house so that nothing would interrupt the conversations. And although Bohr was normally most considerate and friendly in his dealings with people, he now struck me as an almost remorseless fanatic, one who was not prepared to make the least concession or grant that he could ever be mistaken. It is hardly possible to convey just how passionate the discussions were, just how deeply rooted the convictions of each, a fact that marked their every utterance. All I can hope to do here is to produce a very pale copy of conversations in which two men were fighting for their particular interpretation of the new mathematical scheme with all the powers at their command.

Schrödinger: "Surely you realize that the whole idea of quantum jumps is bound to end in nonsense. You claim first of all

that if an atom is in a stationary state, the electron revolves periodically but does not emit light, when, according to Maxwell's theory, it must. Next, the electron is said to jump from one orbit to the next and to emit radiation. Is this jump supposed to be gradual or sudden? If it is gradual, the orbital frequency and energy of the electron must change gradually as well. But in that case, how do you explain the persistence of fine spectral lines? On the other hand, if the jump is sudden, Einstein's idea of light quanta will admittedly lead us to the right wave number, but then we must ask ourselves how precisely the electron behaves during the jump. Why does it not emit a continuous spectrum, as electromagnetic theory demands? And what laws govern its motion during the jump? In other words, the whole idea of quantum jumps is sheer fantasy."

Bohr: "What you say is absolutely correct. But it does not prove that there are no quantum jumps. It only proves that we cannot imagine them, that the representational concepts with which we describe events in daily life and experiments in classical physics are inadequate when it comes to describing quantum jumps. Nor should we be surprised to find it so, seeing that the processes involved are not the objects of direct experience."

Schrödinger: "I don't wish to enter into long arguments about the formation of concepts; I prefer to leave that to the philosophers. I wish only to know what happens inside an atom. I don't really mind what language you choose to discuss it. If there are electrons in the atom, and if these are particles—as all of us believe—then they must surely move in some way. Right now I am not concerned with a precise description of this motion, but it ought to be possible to determine in principle how they behave in the stationary state or during the transition from one state to the next. But from the mathematical form of wave or quantum mechanics alone it is clear that we cannot expect reasonable answers to these questions. The moment, however, that we change the picture and say that there are no discrete electrons, only electron waves or waves of matter, then everything looks quite different. We no longer wonder about the fine lines. The emission of light is as easily explained as the transmission of radio waves through the aerial of the transmitter, and what seemed to be insoluble contradictions have suddenly disappeared."

Bohr: "I beg to disagree. The contradictions do not disappear; they are simply pushed to one side. You speak of the emission of light by the atom or more generally of the interaction between the atom and the surrounding radiation field, and you think that all the problems are solved once we assume that there are material waves but no quantum jumps. But just take the case of thermodynamic equilibrium between the atom and the radiation field—remember, for instance, the Einsteinian derivation of Planck's radiation law. This derivation demands that the energy of the atom should assume discrete values and change discontinuously from time to time; discrete values for the frequencies cannot help us here. You can't seriously be trying to cast doubt on the whole basis of quantum theory!"

Schrödinger: "I don't for a moment claim that all these relationships have been fully explained. But then you, too, have so far failed to discover a satisfactory physical interpretation of quantum mechanics. There is no reason why the application of thermodynamics to the theory of material waves should not yield a satisfactory explanation of Planck's formula as well—an explanation that will admittedly look somewhat different from all previous ones."

Bohr: "No, there is no hope of that at all. We have known what Planck's formula means for the past twenty-five years. And, quite apart from that, we can see the inconstancies, the sudden jumps in atomic phenomena quite directly, for instance when we watch sudden flashes of light on a scintillation screen or the sudden rush of an electron through a cloud chamber. You cannot simply ignore these observations and behave as if they did not exist at all."

Schrödinger: "If all this damned quantum jumping were really here to stay, I should be sorry I ever got involved with quantum theory."

Bohr: "But the rest of us are extremely grateful that you did; your wave mechanics has contributed so much to mathematical clarity and simplicity that it represents a gigantic advance over all previous forms of quantum mechanics."

And so the discussions continued day and night. After a few days Schrödinger fell ill, perhaps as a result of his enormous effort; in any case, he was forced to keep to his bed with a feverish cold. While Mrs. Bohr nursed him and brought in tea

and cake, Niels Bohr kept sitting on the edge of the bed talking at Schrödinger: "But you must surely admit that . . ." No real understanding could be expected since, at the time, neither side was able to offer a complete and coherent interpretation of quantum mechanics. For all that, we in Copenhagen felt convinced toward the end of Schrödinger's visit that we were on the right track, though we fully realized how difficult it would be to convince even leading physicists that they must abandon all attempts to construct perceptual models of atomic processes.

During the next few months the physical interpretation of quantum mechanics was the central theme of all conversations between Bohr and myself. I was then living on the top floor of the Institute, in a cozy little attic flat with slanting walls and windows overlooking the trees at the entrance to Faelled Park. Bohr would often come into my attic late at night, and we constructed all sorts of imaginary experiments to see whether we had really grasped the theory. In so doing, we discovered that the two of us were trying to resolve the difficulties in rather different ways. Bohr was trying to allow for the simultaneous existence of both particle and wave concepts, holding that, though the two were mutually exclusive, both together were needed for a complete description of atomic processes. I disliked this approach. I wanted to start from the fact that quantum mechanics as we then knew it already imposed a unique physical interpretation of some magnitudes occurring in it—for instance, the time averages of energy, momentum, fluctuations, etc.—so that it looked very much as if we no longer had any freedom with respect to that interpretation. Instead, we would have to try to derive the correct general interpretation by strict logic from the ready-to-hand, more special interpretation.

For that reason I was—certainly quite wrongly—rather unhappy about a brilliant piece of work Max Born had done in Göttingen. In it, he had treated collisions by Schrödinger's method and assumed that the square of the Schrödinger wave function measures, in each point of space and at every instant, the probability of finding an electron in this point at that instant. I fully agreed with Born's thesis as such, but disliked the fact that it looked as if we still had some freedom of interpretation; I was firmly convinced that Born's thesis itself was the

necessary consequence of the fixed interpretation of special magnitudes in quantum mechanics. This conviction was strengthened further by two highly informative mathematical studies by Dirac and Jordan.

Luckily, at the end of our talks, Bohr and I would generally come to the same conclusions about particular physical experiments, so that there was good reason to think that our divergent efforts might yet lead to the same result. On the other hand, neither of us could tell how so simple a phenomenon as the trajectory of an electron in a cloud chamber could be reconciled with the mathematical formulations of quantum or wave mechanics. Such concepts as trajectories or orbits did not figure in quantum mechanics, and wave mechanics could only be reconciled with the existence of a densely packed beam of matter if the beam spread over areas much larger than the diameter of an electron.

Since our talks often continued till long after midnight, and did not produce a satisfactory conclusion despite protracted efforts over several months, both of us became utterly exhausted and rather tense. Hence Bohr decided in February 1927 to go skiing in Norway, and I was quite glad to be left behind in Copenhagen, where I could think about these hopelessly complicated problems undisturbed. I now concentrated all my efforts on the mathematical representation of the electron path in the cloud chamber, and when I realized fairly soon that the obstacles before me were quite insurmountable, I began to wonder whether we might not have been asking the wrong sort of question all along. But where had we gone wrong? The path of the electron through the cloud chamber obviously existed; one could easily observe it. The mathematical framework of quantum mechanics existed as well, and was much too convincing to allow for any changes. Hence it ought to be possible to establish a connection between the two, hard though it appeared to be.

It must have been one evening after midnight when I suddenly remembered my conversation with Einstein and particularly his statement, "It is the theory which decides what we can observe." I was immediately convinced that the key to the gate that had been closed for so long must be sought right here. I decided to go on a nocturnal walk through Faelled Park and to think further

about the matter. We had always said so glibly that the path of the electron in the cloud chamber could be observed. But perhaps what we really observed was something much less. Perhaps we merely saw a series of discrete and ill-defined spots through which the electron had passed. In fact, all we do see in the cloud chamber are individual water droplets which must certainly be much larger than the electron. The right question should therefore be: Can quantum mechanics represent the fact that an electron finds itself approximately in a given place and that it moves approximately with a given velocity, and can we make these approximations so close that they do not cause experimental difficulties?

A brief calculation after my return to the Institute showed that one could indeed represent such situations mathematically, and that the approximations are governed by what would later be called the uncertainty principle of quantum mechanics: the product of the uncertainties in the measured values of the position and momentum (i.e., the product of mass and velocity) cannot be smaller than Planck's constant. This formulation, I felt, established the much-needed bridge between the cloud-chamber observations and the mathematics of quantum mechanics. True, it had still to be proved that any experiment whatsoever was bound to set up situations satisfying the uncertainty principle, but this struck me as plausible a priori, since the processes involved in the experiment or the observation had necessarily to satisfy the laws of quantum mechanics. On this presupposition, experiments are unlikely to produce situations that do not accord with quantum mechanics. "It is the theory which decides what we can observe." I resolved to prove this by calculations based on simple experiments during the next few days.

Here, too, I was helped by the memory of a conversation I once had with Burkhard Drude, a fellow student in Göttingen. When discussing the difficulties involved in the concept of electron orbits, he had said that it ought to be possible, in principle, to construct a microscope of extraordinarily high resolving power in which one could see or photograph the electron paths inside the atom. Such a microscope would not, of course, work with ordinary light rays, but perhaps with gamma rays. Now this ran

counter to my hypothesis, according to which not even the best microscope could cross the limits set by the uncertainty principle. Hence I had to demonstrate that the principle was obeyed even in this case. This I managed to do, and the proof strengthened my confidence in the consistency of the new interpretation. After a few more calculations of this kind, I sat down and summarized my results in a long letter to Wolfgang Pauli. His encouraging reply from Hamburg cheered me considerably.

Then Niels Bohr returned from his skiing holiday, and we had a fresh round of difficult discussions. For Bohr, too, had pursued his own ideas on wave-corpuscle dualism. Central to his thought was the concept of complementarity, which he had just introduced to describe a situation in which it is possible to grasp one and the same event by two distinct modes of interpretation. These two modes are mutually exclusive, but they also complement each other, and it is only through their juxtaposition that the perceptual content of a phenomenon is fully brought out. At first, Bohr raised a number of objections against the uncertainty principle, which he probably considered too special a case of the general rule of complementarity. But he soon afterward realized—manfully assisted by the Swedish physicist, Oskar Klein, who was also working in Copenhagen—that there was no serious difference between the two interpretations, and that all that mattered now was to represent the facts in such a way that despite their novelty they could be grasped and accepted by all physicists.

The matter was thrashed out in the autumn of 1927 at two physics conferences: the General Physics Congress in Como, at which Bohr gave a comprehensive account of the new situation, and the Solvay Congress in Brussels. In accordance with the wishes of the Solvay Foundation, the latter was attended by a small group of specialists anxious to discuss the problems of quantum theory in detail. We all stayed at the same hotel, and the keenest arguments took place, not in the conference hall but during the hotel meals. Bohr and Einstein were in the thick of it all. Einstein was quite unwilling to accept the fundamentally statistical character of the new quantum theory. Needless to say, he had no objections against probability statements whenever a particular system was not known in every last detail—after all,

the old statistical mechanics and thermodynamics had been based on just such statements. However, Einstein would not admit that it was impossible, even in principle, to discover all the partial facts needed for the complete description of a physical process. "God does not throw dice" was a phrase we often heard from his lips in these discussions. And so he refused point-blank to accept the uncertainty principle, and tried to think up cases in which the principle would not hold.

The discussion usually started at breakfast, with Einstein serving us up with yet another imaginary experiment by which he thought he had definitely refuted the uncertainty principle. We would at once examine his fresh offering, and on the way to the conference hall, to which I generally accompanied Bohr and Einstein, we would clarify some of the points and discuss their relevance. Then, in the course of the day, we would have further discussions on the matter, and, as a rule, by supertime we would have reached the point where Niels Bohr could prove to Einstein that even his latest experiment failed to shake the uncertainty principle. Einstein would look a bit worried, but by next morning he was ready with a new imaginary experiment more complicated than the last, and this time, so he avowed, bound to invalidate the uncertainty principle. This attempt would fare no better by evening, and after the same game had been continued for a few days, Einstein's friend Paul Ehrenfest, a physicist from Leyden in Holland, said: "Einstein, I am ashamed of you; you are arguing against the new quantum theory just as your opponents argue about relativity theory." But even this friendly admonition went unheard.

Once again it was driven home to me how terribly difficult it is to give up an attitude on which one's entire scientific approach and career have been based. Einstein had devoted his life to probing into that objective world of physical processes which runs its course in space and time, independent of us, according to firm laws. The mathematical symbols of theoretical physics were also symbols of this objective world and as such enabled physicists to make statements about its future behavior. And now it was being asserted that, on the atomic scale, this objective world of time and space did not even exist and that the mathematical symbols of theoretical physics referred to possibilities rather than

Einstein's quote

to facts. Einstein was not prepared to let us do what, to him, amounted to pulling the ground from under his feet. Later in life, also, when quantum theory had long since become an integral part of modern physics, Einstein was unable to change his attitude—at best, he was prepared to accept the existence of quantum theory as a temporary expedient. "God does not throw dice" was his unshakable principle, one that he would not allow anybody to challenge. To which Bohr could only counter with: "Nor is it our business to prescribe to God how He should run the world."

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