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Einstein B→Z

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Quantum Mechanics and a Talk with Einstein (1925–1926)

During these critical years, atomic physics developed much as Niels Bohr had predicted it would during our walk over the Hain Mountain. The difficulties and inner contradictions that stood in the way of a true understanding of atoms and their stability seemed unlikely to be removed or even reduced—on the contrary, they became still more acute. All attempts to surmount them with the conceptual tools of the older physics appeared doomed to failure.

There was, for instance, the discovery by the American physicist, Arthur Holly Compton, that light (or more precisely X-rays) changes its wavelength when radiation is scattered by free electrons. This result could be explained by Einstein's hypothesis that light consists of small corpuscles or packets of energy, moving through space with great velocity and occasionally—e.g., during the process of scattering—colliding with an electron. On the other hand, there was a great deal of experimental evidence to suggest that the only basic difference between light and radio waves was that the former are of shorter length; in other words, that a light ray is a wave and not a stream of particles. Moreover, attempts by the Dutch physicist, Ornstein, to determine the intensity ratio of spectral lines in a so-called multiplet had produced very strange results. These ratios can be determined with the help of Bohr's theory. Now it appeared that, although the formulae derived from Bohr's theory were incorrect, a minor modification produced new formulae that fitted the experimental

results. And so physicists gradually learned to adapt themselves to a host of difficulties. They became used to the fact that the concepts and models of classical physics were not rigorously applicable to processes on the atomic scale. On the other hand, they had come to appreciate that, by skillful use of the resulting freedom, they could, on occasion, guess the correct mathematical formulation of some of the details.

In the seminars run by Max Born in Göttingen during the summer of 1924, we had begun to speak of a new quantum mechanics that would one day oust the old Newtonian mechanics, and whose vague outlines could already be discerned here and there. Even during the subsequent winter term, which I once again spent in Copenhagen, trying to develop Kramers' theory of dispersion phenomena, our efforts were devoted not so much to deriving the correct mathematical relationships as to guessing them from similarities with the formulae of classical theory. ✓

If I think back on the state of atomic theory in those months, I always remember a mountain walk with some friends from the Youth Movement, probably in the late autumn of 1924. It took us from Kreuth to Lake Achen. In the valley the weather was poor, and the mountains were veiled in clouds. During the climb, the mist had begun to close in upon us, and, after a time, we found ourselves in a confused jumble of rocks and undergrowth with no signs of a track. We decided to keep climbing, though we felt rather anxious about getting down again if anything went wrong. All at once the mist became so dense that we lost sight of one another completely, and could keep in touch only by shouting. At the same time it grew brighter overhead, and the light suddenly changed color. We were obviously under a patch of moving fog. Then, quite suddenly, we could see the edge of a steep rock face, straight ahead of us, bathed in bright sunlight. The next moment the fog had closed up again, but we had seen enough to take our bearings from the map. After a further ten minutes of hard climbing we were standing in the sun—at saddle height above the sea of fog. To the south we could see the peaks of the Sonnwend Mountains and beyond them the snowy tops of the Central Alps, and we all breathed a sigh of relief.

in spring 1960.

In atomic physics, likewise, the winter of 1924–1925 had obviously brought us to a realm where the fog was thick but where some light had begun to filter through and held out the promise of exciting new vistas.

In the summer term of 1925, when I resumed my research work at the University of Göttingen—since July 1924 I had been *Privatdozent* at that university—I made a first attempt to guess what formulae would enable one to express the line intensities of the hydrogen spectrum, using more or less the same methods that had proved so fruitful in my work with Kramers in Copenhagen. This attempt led to a dead end—I found myself in an impenetrable morass of complicated mathematical equations, with no way out. But the work helped to convince me of one thing: that one ought to ignore the problem of electron orbits inside the atom, and treat the frequencies and amplitudes associated with the line intensities as perfectly good substitutes. In any case, these magnitudes could be observed directly, and as my friend Otto had pointed out when expounding on Einstein's theory during our bicycle tour round Lake Walchensee, physicists must consider none but observable magnitudes when trying to solve the atomic puzzle. My attempt to apply this scheme to the hydrogen atom had come to grief on the complications of this particular problem. Accordingly, I looked for a simpler mathematical system and found it in the pendulum, whose oscillations could serve as a model for the molecular vibrations treated by atomic physics. My work along these lines was advanced rather than retarded by an unfortunate personal setback.

Toward the end of May 1925, I fell so ill with hay fever that I had to ask Born for fourteen days' leave of absence. I made straight for Heligoland, where I hoped to recover quickly in the bracing sea air, far from blossoms and meadows. On my arrival I must have looked quite a sight with my swollen face; in any case, my landlady took one look at me, concluded that I had been in a fight and promised to nurse me through the aftereffects. My room was on the second floor, and since the house was built high up on the southern edge of the rocky island, I had a glorious view over the village, and the dunes and the sea beyond. As I sat on my balcony, I had ample opportunity to reflect on Bohr's remark that part of infinity seems to lie within the grasp of those who look across the sea.

Apart from daily walks and long swims, there was nothing in Heligoland to distract me from my problem, and so I made much swifter progress than I would have done in Göttingen. A few days were enough to jettison all the mathematical ballast that invariably encumbers the beginning of such attempts, and to arrive at a simple formulation of my problem. Within a few days more, it had become clear to me what precisely had to take the place of the Bohr-Sommerfeld quantum conditions in an atomic physics working with none but observable magnitudes. It also became obvious that with this additional assumption I had introduced a crucial restriction into the theory. Then I noticed that there was no guarantee that the new mathematical scheme could be put into operation without contradictions. In particular, it was completely uncertain whether the principle of the conservation of energy would still apply, and I knew only too well that my scheme stood or fell by that principle.

Other than that, however, several calculations showed that the scheme seemed quite self-consistent. Hence I concentrated on demonstrating that the conservation law held, and one evening I reached the point where I was ready to determine the individual terms in the energy table, or, as we put it today, in the energy matrix, by what would now be considered an extremely clumsy series of calculations. When the first terms seemed to accord with the energy principle, I became rather excited, and I began to make countless arithmetical errors. As a result, it was almost three o'clock in the morning before the final result of my computations lay before me. The energy principle had held for all the terms, and I could no longer doubt the mathematical consistency and coherence of the kind of quantum mechanics to which my calculations pointed. At first, I was deeply alarmed. I had the feeling that, through the surface of atomic phenomena, I was looking at a strangely beautiful interior, and felt almost giddy at the thought that I now had to probe this wealth of mathematical structures nature had so generously spread out before me. I was far too excited to sleep, and so, as a new day dawned, I made for the southern tip of the island, where I had been longing to climb a rock jutting out into the sea. I now did so without too much trouble, and waited for the sun to rise.

What I saw during that night in Heligoland was admittedly not very much more than the sunlit rock edge I had glimpsed in

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the autumn of 1924, but when I reported my results to Wolfgang Pauli, generally my severest critic, he warmly encouraged me to continue along the path I had taken. In Göttingen, Max Born and Pascual Jordan took stock of the new possibilities, and in Cambridge the young English mathematician Paul Dirac developed his own methods for solving the problems involved, and after only a few months the concentrated efforts of these men led to the emergence of a coherent mathematical framework, one that promised to embrace all the multifarious aspects of atomic physics. Of the extremely intensive work which kept us breathless for a few months I shall say nothing here; instead, I shall report my talk with Albert Einstein following a lecture on the new quantum mechanics in Berlin.

date? 1926

At the time, the University of Berlin was considered the stronghold of physics in Germany, with such renowned figures as Planck, Einstein, von Laue and Nernst. It was here that Planck had discovered quantum theory and that Rubens had confirmed it by special measurements of thermal radiation; it was here that Einstein had formulated his general theory of relativity and his theory of gravitation in 1916. At the center of scientific life was the so-called physics colloquium, which probably went back to the time of Helmholtz and which was generally attended by the entire staff of the physics department. In the spring of 1926, I was invited to address this distinguished body on the new quantum mechanics, and since this was my first chance to meet so many famous men, I took good care to give a clear account of the concepts and mathematical foundations of what was then a most unconventional theory. I apparently managed to arouse Einstein's interest, for he invited me to walk home with him so that we might discuss the new ideas at greater length.

On the way, he asked about my studies and previous research. As soon as we were indoors, he opened the conversation with a question that bore on the philosophical background of my recent work. "What you have told us sounds extremely strange. You assume the existence of electrons inside the atom, and you are probably quite right to do so. But you refuse to consider their orbits, even though we can observe electron tracks in a cloud chamber. I should very much like to hear more about your reasons for making such strange assumptions."

"We cannot observe electron orbits inside the atom," I must have replied, "but the radiation which an atom emits during discharges enables us to deduce the frequencies and corresponding amplitudes of its electrons. After all, even in the older physics wave numbers and amplitudes could be considered substitutes for electron orbits. Now, since a good theory must be based on directly observable magnitudes, I thought it more fitting to restrict myself to these, treating them, as it were, as representatives of the electron orbits."

"But you don't seriously believe," Einstein protested, "that none but observable magnitudes must go into a physical theory?"

"Isn't that precisely what you have done with relativity?" I asked in some surprise. "After all, you did stress the fact that it is impermissible to speak of absolute time, simply because absolute time cannot be observed; that only clock readings, be it in the moving reference system or the system at rest, are relevant to the determination of time."

"Possibly I did use this kind of reasoning," Einstein admitted, "but it is nonsense all the same. Perhaps I could put it more diplomatically by saying that it may be heuristically useful to keep in mind what one has actually observed. But on principle, it is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe. You must appreciate that observation is a very complicated process. The phenomenon under observation produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness—we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions. When we claim that we can observe something new, we ought really to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our

VON NEUMANN!

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consciousness—function in such a way that we can rely upon them and hence speak of 'observations.'

"In the theory of relativity, for instance, we presuppose that, even in the moving reference system, the light rays traveling from the clock to the observer's eye behave more or less as we have always expected them to behave. And in your theory, you quite obviously assume that the whole mechanism of light transmission from the vibrating atom to the spectroscope or to the eye works just as one has always supposed it does, that is, essentially according to Maxwell's laws. If that were no longer the case, you could not possibly observe any of the magnitudes you call observable. Your claim that you are introducing none but observable magnitudes is therefore an assumption about a property of the theory that you are trying to formulate. You are, in fact, assuming that your theory does not clash with the old description of radiation phenomena in the essential points. You may well be right, of course, but you cannot be certain."

I was completely taken aback by Einstein's attitude, though I found his arguments convincing. Hence I said: "The idea that a good theory is no more than a condensation of observations in accordance with the principle of thought economy surely goes back to Mach, and it has, in fact, been said that your relativity theory makes decisive use of Machian concepts. But what you have just told me seems to indicate the very opposite. What am I to make of all this, or rather what do you yourself think about it?"

"It's a very long story, but we can go into it if you like. Mach's concept of thought economy probably contains part of the truth, but strikes me as being just a bit too trivial. Let me first of all produce a few arguments in its favor. We obviously grasp the world by way of our senses. Even when small children learn to speak and to think, they do so by recognizing the possibility of describing highly complicated but somehow related sense impressions with a single word, for instance, the word 'ball.' They learn it from adults and get the satisfaction that they can make themselves understood. In other words, we may argue that the formation of the word, and hence of the concept, 'ball' is a kind of thought economy enabling the child to combine very complicated sense impressions in a simple way. Here Mach does

not even enter into the question which mental or physical predispositions must be satisfied in man—or the small child—before the process of communication can be initiated. With animals, this process works considerably less effectively, as everyone knows, but we shan't talk about that now. Now Mach also thinks that the formation of scientific theories, however complex, takes place in a similar way. We try to order the phenomena, to reduce them to a simple form, until we can describe what may be a large number of them with the aid of a few simple concepts.

"All this sounds very reasonable, but we must nevertheless ask ourselves in what sense the principle of mental economy is being applied here. Are we thinking of psychological or of logical economy, or, again, are we dealing with the subjective or the objective side of the phenomena? When the child forms the concept 'ball,' does he introduce a purely psychological simplification in that he combines complicated sense impressions by means of this concept, or does this ball really exist? Mach would probably answer that the two statements express one and the same fact. But he would be quite wrong to do so. To begin with, the assertion 'The ball really exists' also contains a number of statements about possible sense impressions that may occur in the future. Now future possibilities and expectations make up a very important part of our reality, and must not be simply forgotten. Moreover, we ought to remember that inferring concepts and things from sense impressions is one of the basic presuppositions of all our thought. Hence, if we wanted to speak of nothing but sense impressions, we should have to rid ourselves of our language and thought. In other words, Mach rather neglects the fact that the world really exists, that our sense impressions are based on something objective.

"I have no wish to appear as an advocate of a naïve form of realism; I know that these are very difficult questions, but then I consider Mach's concept of observation also much too naïve. He pretends that we know perfectly well what the word 'observe' means, and thinks this exempts him from having to discriminate between 'objective' and 'subjective' phenomena. No wonder his principle has so suspiciously commercial a name: 'thought economy.' His idea of simplicity is much too subjective for me. In reality, the simplicity of natural laws is an objective fact as well,

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and the correct conceptual scheme must balance the subjective side of this simplicity with the objective. But that is a very difficult task. Let us rather return to your lecture.

"I have a strong suspicion that, precisely because of the problems we have just been discussing, your theory will one day get you into hot water. I should like to explain this in greater detail. When it comes to observation, you behave as if everything can be left as it was, that is, as if you could use the old descriptive language. In that case, however, you will also have to say: in a cloud chamber we can observe the path of the electrons. At the same time, you claim that there are no electron paths inside the atom. This is obvious nonsense, for you cannot possibly get rid of the path simply by restricting the space in which the electron moves."

CLOUD CHAMBER PATH

I tried to come to the defense of the new quantum mechanics. "For the time being, we have no idea in what language we must speak about processes inside the atom. True, we have a mathematical language, that is, a mathematical scheme for determining the stationary states of the atom or the transition probabilities from one state to another, but we do not know—at least not in general—how this language is related to that of classical physics. And, of course, we need this connection if we are to apply this theory to experiments in the first place. For when it comes to experiments, we invariably speak in the traditional language. Hence I cannot really claim that we have 'understood' quantum mechanics. I assume that the mathematical scheme works, but no link with the traditional language has been established so far. And until that has been done, we cannot hope to speak of the path of the electron in the cloud chamber without inner contradictions. Hence it is probably much too early to solve the difficulties you have mentioned."

"Very well, I will accept that," Einstein said. "We shall talk about it again in a few years' time. But perhaps I may put another question to you. Quantum theory as you have expounded it in your lecture has two distinct faces. On the one hand, as Bohr himself has rightly stressed, it explains the stability of the atom; it causes the same forms to reappear time and again. On the other hand, it explains that strange discontinuity or inconstancy of nature which we observe quite clearly when we

1926 He doesn't yet believe in photons

watch flashes of light on a scintillation screen. These two aspects are obviously connected. In your quantum mechanics you will have to take both into account, for instance when you speak of the emission of light by atoms. You can calculate the discrete energy values of the stationary states. Your theory can thus account for the stability of certain forms that cannot merge continuously into one another, but must differ by finite amounts and seem capable of permanent re-formation. But what happens during the emission of light? As you know, I suggested that, when an atom drops suddenly from one stationary energy value to the next, it emits the energy difference as an energy packet, a so-called light quantum. In that case, we have a particularly clear example of discontinuity. Do you think that my conception is correct? Or can you describe the transition from one stationary state to another in a more precise way?"

In my reply, I must have said something like this: "Bohr has taught me that one cannot describe this process by means of the traditional concepts, i.e., as a process in time and space. With that, of course, we have said very little, no more, in fact, than that we do not know. Whether or not I should believe in light quanta, I cannot say at this stage. Radiation quite obviously involves the discontinuous elements to which you refer as light quanta. On the other hand, there is a continuous element, which appears, for instance, in interference phenomena, and which is much more simply described by the wave theory of light. But you are of course quite right to ask whether quantum mechanics has anything new to say on these terribly difficult problems. I believe that we may at least hope that it will one day.

"I could, for instance, imagine that we should obtain an interesting answer if we considered the energy fluctuations of an atom during reactions with other atoms or with the radiation field. If the energy should change discontinuously, as we expect from your theory of light quanta, then the fluctuation, or, in more precise mathematical terms, the mean square fluctuation, would be greater than if the energy changed continuously. I am inclined to believe that quantum mechanics would lead to the greater value, and so establish the discontinuity. On the other hand, the continuous element, which appears in interference experiments, must also be taken into account. Perhaps one must

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imagine the transitions from one stationary state to the next as so many fade-outs in a film. The change is not sudden—one picture gradually fades while the next comes into focus so that, for a time, both pictures become confused and one does not know which is which. Similarly, there may well be an intermediate state in which we cannot tell whether an atom is in the upper or the lower state."

"You are moving on very thin ice," Einstein warned me. "For you are suddenly speaking of what we know about nature and no longer about what nature really does. In science we ought to be concerned solely with what nature does. It might very well be that you and I know quite different things about nature. But who would be interested in that? Perhaps you and I alone. To everyone else it is a matter of complete indifference. In other words, if your theory is right, you will have to tell me sooner or later what the atom does when it passes from one stationary state to the next."

"Perhaps," I may have answered. "But it seems to me that you are using language a little too strictly. Still, I do admit that everything that I might now say may sound like a cheap excuse. So let's wait and see how atomic theory develops."

Einstein gave me a skeptical look. "How can you really have so much faith in your theory when so many crucial problems remain completely unsolved?"

I must certainly have thought for a long time before I produced my answer. "I believe, just like you, that the simplicity of natural laws has an objective character, that it is not just the result of thought economy. If nature leads us to mathematical forms of great simplicity and beauty—by forms I am referring to coherent systems of hypotheses, axioms, etc.—to forms that no one has previously encountered, we cannot help thinking that they are 'true,' that they reveal a genuine feature of nature. It may be that these forms also cover our subjective relationship to nature, that they reflect elements of our own thought economy. But the mere fact that we could never have arrived at these forms by ourselves, that they were revealed to us by nature, suggests strongly that they must be part of reality itself, not just of our thoughts about reality.

"You may object that by speaking of simplicity and beauty I

am introducing aesthetic criteria of truth, and I frankly admit that I am strongly attracted by the simplicity and beauty of the mathematical schemes with which nature presents us. You must have felt this, too: the almost frightening simplicity and wholeness of the relationships which nature suddenly spreads out before us and for which none of us was in the least prepared. And this feeling is something completely different from the joy we feel when we have done a set task particularly well. That is one reason why I hope that the problems we have been discussing will be solved in one way or another. In the present case, the simplicity of the mathematical scheme has the further consequence that it ought to be possible to think up many experiments whose results can be predicted from the theory. And if the actual experiments should bear out the predictions, there is little doubt but that the theory reflects nature accurately in this particular realm."

"Control by experiment," Einstein agreed, "is, of course, an essential prerequisite of the validity of any theory. But one can't possibly test everything. That is why I am so interested in your remarks about simplicity. Still, I should never claim that I really understood what is meant by the simplicity of natural laws."

After talking about the role of truth criteria in physics for quite a bit longer, I took my leave. I next met Einstein a year and a half later, at the Solvay Congress in Brussels, where the epistemological and philosophical bases of quantum theory once again formed the subject of the most exciting discussions.

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