Recollections of an Exciting Era.

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I.

I am very glad I came to Varenna to attend this summer school. I have learned a great deal here, not only individual facts about the history of science, which I have picked up from various lectures, but I have learned to appreciate the point of view of the historian of science. It is really a very different point of view from that of the research physicist. The research physicist, if he has made a discovery, is then concerned with standing on the new vantage point which he has gained and surveying the field in front of him. His question is, where do we go from here? What are the applications of this new discovery? How far will it go in elucidating the problems which are still before us? What will be the prime problems now facing us?

He wants to rather forget the way by which he attained this discovery. He proceeded along a tortuous path, followed various false trails, and he does not want to think of these. He feels perhaps a bit ashamed, disgusted with himself, that he took so long. He says to himself: What a lot of time I wasted following this particular track when I should have seen at once that it would lead nowhere. When a discovery has been made, it usually seems so obvious that one is surprised that no one had thought of it previously. With that point of view, one does not want to remember all the work that led up to the making of the discovery.

Now, that is just the opposite to what the historian of science wants. He wants to know the various influences at work, the various intermediate steps, and he may even have some interest in the false trails. These are quite contradictory points of view. Most of my life has been spent with the point of view of the research physicist, and that involves forgetting as quickly as possible the various intermediate steps.

However, with the understanding of what the historians of science are concerned with, I have tried to think over the past, and have done my best to remember the various incidents, things that happened 50 years ago. I have tried to figure out the influences, the effect of the various teachers that I had and the training that I received, to see how these things led me to the style of work which I followed in later life.

I have given as the title « Recollections of an Exciting Era », and this era should be counted as beginning in 1919. At that time, a wonderful thing happened. Relativity burst upon the world, with a tremendous impact. Suddenly everyone was talking about relativity. The newspapers were full of it. The magazines also contained articles written by various people on relativity, not always for relativity but sometimes against it. Relativity was understood in a very wide sense, and was taken up by philosophers and by people in all walks of life.

It is easy to see the reason for this tremendous impact. We had just been living through a terrible and very serious war. It was also in some ways a rather dull war. The overall picture did not change very much, except for heavy casualties which we were continually reading about. The position of the front line changed very little with the various attacks, just maybe advanced or receded a few hundred yards, and that was all.

Then this terrible war came to an end rather suddenly. The result was that everyone was sick and tired of the war. Everyone wanted to forget it. And then relativity came along as a wonderful idea leading to a new domain of thought. It was an escape from the war. The impact that relativity produced I think has never been equalled either before or since by any scientific idea catching the public mind.

This impact of relativity involved simultaneously the special theory and the general theory. Now, the special theory was actually very much older, dating from 1905, but no one knew anything about it except a few specialists in the universities. The ordinary person had never heard of EINSTEIN. Suddenly EINSTEIN was on everyone's lips. EINSTEIN was rather a remote figure in a foreign country. A person who was much more present was EDDINGTON. He was the leader of relativity in England at that time. He was the great authority whom everyone listened to with the greatest respect, and he was rather regarded as the chief exponent of relativity. EINSTEIN was in the remote background.

At this time I was a student of engineering at Bristol University, and of course I was caught up in this excitement produced by relativity. We discussed it very much. The students discussed it among themselves, but had very little accurate information to go on. Relativity was a subject that everybody felt himself competent to write about in a general philosophical way. The philosophers just put forward the view that everything had to be considered relatively to something else, and they rather claimed that they had known about relativity all along.

You can get a sample of what the various articles on relativity were like from what HOLTON told us (see p. 266). He actually read out an extract of some writings of Sir Oliver LODGE. Sir Oliver LODGE was rather critical of relativity. But you see, with writings of that style, there is nothing very precisely stated, so we engineering students were caught up in a discussion of a question about which we had very little hard and fast facts.

I first got some accurate information about relativity through attending a course of lectures by BROAD. Now, BROAD was a philosopher who looked at things from the philosophical point of view. He was a lecturer in Bristol University in philosophy at that time. He later became a professor in Cambridge and died just a few years ago. He gave a course of about ten or twelve lectures on relativity, discussing it from the point of view of philosophy. Several of the engineering students attended his course at the beginning, but they rather dropped out. I stayed on till the end. I tried very hard to understand philosophy. The engineering students with me had a very practical outlook and they said that these philosophical questions are of no use to an engineer, so they stopped coming. However, I thought there might be something in philosophy, and I did my best to understand the philosopher's point of view. I had also read a little about philosophy. I had read all through Mill's book on logic.

However, my attempts to appreciate philosophy were not very successful. I felt then that all the things that philosophers said were rather indefinite, and came to the conclusion eventually that I did not think philosophy could contribute anything to the advance of physics. I did not immediately have that point of view, but I came to it only after a lot of thought, and studying what philosophers said, in particular BROAD.

Now, I not only heard from BROAD what the general outlook of the philosopher was; he gave some exact information about special relativity, also about general relativity. I remember (I think it was in the second or third lecture) he wrote a formula on the blackboard

$$ds^2 = dx^2 + dy^2 + dz^2 - c^2 dt^2 . \tag{1}$$

Now, when I saw that minus sign, it produced a tremendous effect on me. I immediately saw that here was something new. Perhaps I can explain the reason for this big effect from the fact that previously as a schoolboy I had been much interested in the relations of space and time. I had thought about them a great deal, and it had become apparent to me that time was very much like another dimension, and the possibility had occurred to me that perhaps there was some connection between space and time, and that we ought to consider them from a general four-dimensional point of view.

However, at that time the only geometry that I knew was Euclidean geometry, and, if space and time were to be coupled in any way, they would have to be coupled with a plus sign here, and it was very easy to see that that would not work, and led to nonsense as soon as one tried to make any big change in one's time axis.

I could perhaps explain at this point that I was always very much interested in geometry. That was the branch of mathematics which fascinated me. You can divide all mathematicians into two classes, those whose main interest is geometry, those whose main interest is algebra. This division into two classes is also largely a racial division. People with European training, European background, tend to be interested in geometry, following from the old Greek school. Those whose main interest is in algebra tend to be Asiatic people, following from the discovery of algebra by the Arabs.

Now, a good mathematician has to be a master both of geometry and of algebra, and he has to be able to pass from one to the other quite freely according to the nature of the problem that he is working on. But still he will always keep his preference for one kind of thought, and my preference was strongly on the side of geometry, and has always remained so.

Well, this formula that BROAD wrote on the board of course gave me a new insight into geometry. I remember that previously, when I was at school, one of my mathematics masters had told me that I would probably be very interested in non-Euclidean geometry, and suggested books which I should read on the subject. However, I was not interested in it. The reason for that is that I was interested in the real physical world, and it seemed to me to be obvious that the real physical world was based on Euclidean geometry. There was therefore no need to consider any other kind of geometry. I was not interested just in logical developments, or in seeing what the possibilities are when one follows from a different set of axioms. That again is a trend which has stayed with me all through my life. I have been interested in the real physical world, and not in questions just of logic. This being interested in the real physical world was of course just what was supported by the engineering training which I received.

When I got this new outlook from the formula which BROAD wrote on the board, I was soon able to figure out by myself the basic relations of special relativity.

I completed my course in engineering, and I would like to try to explain the effect of this engineering training on me. I did not make any further use of the detailed applications of this work, but it did change my whole outlook to a very large extent. Previously, I was interested only in exact equations. It seemed to me that if one worked with approximations there was an intolerable ugliness in one's work, and I very much wanted to preserve mathematical beauty. Well, the engineering training which I received did teach me to tolerate approximations, and I was able to see that even theories based on approximations could sometimes have a considerable amount of beauty in them. A problem like arranging the windings in the rotor of a dynamo involved some mathematics. It was a mathematics of whole numbers, but there was quite a bit of beauty in it.

There was this whole change of outlook, and also another change of outlook which was perhaps brought on by the theory of relativity. I had started off believing that there were some exact laws of Nature and that all that we had to do was to work out the consequences of these exact laws. Typical of the exact laws were Newton's laws of motion. Now we learned that Newton's laws of motion were not exact, only approximations, and I began to infer from that that maybe all the laws of Nature are only approximations. I was quite prepared at that time to consider all our equations as only approximations representing our present state of knowledge, and to take it as a task to try to improve on them.

I think that if I had not had this engineering training, I should not have had any success with the kind of work that I did later on, because it was really necessary to get away from the point of view that one should deal only with exact equations, and that one should deal only with results which could be deduced logically from known exact laws which one accepted, in which one had implicit faith. Engineers were concerned only with getting equations which were useful for describing nature. They did not very much mind how the equations were obtained. Once they got them, they proceeded to use them with their slide rules, and get results which were necessary for their work.

And that led me of course to the view that this outlook was really the best outlook to have. We wanted a description of Nature. We wanted to find equations which would describe Nature, and the best we could hope for was usually approximate equations, and we would have to reconcile ourselves to an absence of strict logic and confine ourselves to trying to get the equations which really succeeded in describing Nature.

After my engineering course was finished, I continued at Bristol University for another two years studying mathematics. During this mathematical training, the man who influenced me most was FRASER. FRASER was a mathematician who never did any research, never published anything, but he was a wonderful teacher, able to inspire his students with real excitement about basic ideas in mathematics. I think he's pretty well unheard-of now, except that when he died HODGE wrote an obituary notice of him, *Journal London Math. Soc.* **34**, 111 (1959). He paid tribute to FRASER as being a really great teacher.

There were two things that I learnt from FRASER. One of them was rigorous mathematics. Previously I had been using only the nonrigorous mathematics which engineers were satisfied with. They just wanted to get practical results. They were not concerned with the exact definition of a limit, or how long to sum a series, and things like that. Well, FRASER taught that it was sometimes necessary to have strict logical ideas for dealing with such subjects.

However, I continued in my later work to use mostly the nonrigorous mathematics of the engineers, and I think that you will find that most of my later writings do involve nonrigorous mathematics. When I introduce a function, I do not stop to say whether it is continuous or differentiable or put down all the other conditions which a pure mathematician wants before he can make any statements about it. I just take the point of view that this is the sort of function that a physicist is interested in.

However, this kind of nonrigorous mathematics does not always work. There were a few occasions when I have been sharply stopped by the need to consider more accurately definitions, and possible sources of error, which the nonrigorous mathematics might lead one into.

The second thing I learned from FRASER was projective geometry. Now, that had a profound influence on me because of the mathematical beauty involved in it. There was also very great power in the methods employed. I think probably most physicists know very little about projective geometry. That I would say is a failing in their education. Projective geometry always deals with flat space, but it is a most powerful tool for dealing with flat space, and it provides you with methods, such as the method of one-to-one correspondences, which give you results apparently by magic; theorems in Euclidean geometry which you have been worrying about for a long time drop out by the simplest possible means when one uses the arguments of projective geometry.

Now, 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.

You might think that projective geometry is not of much interest to a physicist, but that is not so. Physicists nowadays are concerned very largely with Minkowski space. Now, if you want to picture relationships in Minkowski space, relationships between vectors and tensors, often the very best way to do it is by using the notions of projective geometry. I was continually using these ideas of projective geometry in my research work. When you want to discover how a particular quantity transforms under a Lorentz transformation, very often the best way of handling the problem is in terms of projective geometry.

It was 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 was an argument which any physicist would be able to understand without having had this special training.

However, for the purposes of research, when one is entering into a new field and one does not know what lies in front of one, one wants very much to visualize the things which one is dealing with, and projective geometry does provide the best tool for this.

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.

Well, I spent two years studying mathematics at Bristol, and then proceeded to Cambridge as a research student. Research students in Cambridge were each provided with a supervisor, someone who had to look over their work, suggest problems to them, and in general take an interest in their work and help them along with it.

My supervisor was R. H. FOWLER, another man who had a great influence on me. At first I was slightly disappointed when FOWLER was appointed my supervisor. The reason for that was that my main interest was on the geometrical side, and especially on relativity. Now, FOWLER was not concerned with relativity. There was CUNNINGHAM, who was an expert in special relativity. He had written a book about it in 1910. But he did not want to take on any more research students, and so I was passed on to FOWLER.

I soon found out that this shade of disappointment that I first felt was quite unjustified. FOWLER introduced me to quite a new field of interest, namely the atom of RUTHERFORD, BOHR and SOMMERFELD. Previously I had heard nothing about the Bohr theory. It was quite an eyeopener to me. I was very much surprised to see that one could make use of the equations of classical electrodynamics in the atom. The atoms were always considered as very hypothetical things by me, and here were people actually dealing with equations concerned with the structure of the atom.

I was very quickly plunged into the center of the problems concerning the explanation of atoms. The most profound problem of all was why are the electron orbits stable? Why do the electrons not just fall down into the nucleus like they should, according to classical mechanics?

I proceeded to think over these problems as hard as I could, and really spent my whole time on that kind of work, and also other mathematical work.

I kept up my interest in relativity. I studied Eddington's classical book, *Mathematical Theory of Relativity*, and found it rather tough at first but eventually mastered it. I was very happy to have EDDINGTON actually present, and sometimes I met him and had some discussion with him on the question of kinematic and dynamical velocity, which led to a little note of mine published in the *Philosophical Magazine*. It was really a wonderful thing for me to meet the man who was the fountainhead of relativity so far as England was concerned. EINSTEIN was just too remote to count.

Another mathematical activity that I had was provided by BAKER, the professor of geometry at Cambridge at that time. I did not go to any of Baker's lectures. I went to other lectures, by CUNNINGHAM and FOWLER, but I was not sufficiently concerned with geometry to go to Baker's lectures. However, on Saturday afternoons BAKER held a tea party to which I was invited, and at the end of the tea party someone would give a talk on some subject of geometry. It was always the geometry of flat space. It was always handled by the methods of projective geometry. Everyone considered then that projective geometry was the only kind of geometry worth studying. It was so much more powerful than the geometry in which one was confined to Euclid's axioms. They worked very often in a number of dimensions more than three, four, five or six, and studied the various figures which one could construct in these spaces of a higher number of dimensions, and I was much impressed by the power of their methods. By studying these figures in spaces of higher dimensions, one was often able to get quick proofs of results in the ordinary three-dimensional Euclidean space, results which would be very tedious to get by other methods.

These tea parties did very much to stimulate my interest in the beauty of mathematics. The all-important thing there was to strive to express the relationships in a beautiful form, and they were very successful. I did some work on projective geometry myself and gave one of the talks at one of the tea parties. This was the first lecture I ever gave, and so of course I remember it very well. I dealt with a new method for handling these projective problems.

Well, those were the influences which I had as my background. I was, effectively, very much in the world center for the development of atomic theory. I should say that BOHR was very friendly to RUTHERFORD all his life. He continually came to Cambridge and gave us lectures. Also FOWLER often went to Copenhagen and learned about the latest situation from there and of course kept me informed about it. In fact, FOWLER spent the winter of 1925, three months, in Copenhagen.

I also had the benefit of hearing the workers in the Cavendish speaking about their experimental work. There were RUTHERFORD, ASTON, WILSON, and so on, and I learnt to appreciate something of the problems of experimentalists.

I would like to say something about BOHR, when he came to lecture in Cambridge. He impressed everyone very much with his deep style of thinking. When he gave a lecture, he would always begin at the beginning, with his work leading to the explanation of the Balmer formula, and would proceed to develop his ideas on that basis. He spoke slowly, and of course it took him a long time to get to a more modern outlook which was the goal of his lecture. The result was that his lectures lasted usually for two hours and maybe more, but everybody listened with the greatest attention. People were pretty well spellbound by what BOHR said.

Of course, it was necessary to pay the greatest attention because he spoke in a quiet voice. Microphones were not in use at that time. One just had to strain one's hearing in order to find out what he said.

While I was very much impressed by what BOHR said, his arguments were mainly of a qualitative nature, and I was not able to really pinpoint the facts behind them. What I wanted was statements which could be expressed in terms of equations, and Bohr's work very seldom provided such statements. I am really not sure how much my later work was influenced by these lectures of Bohr's. That's something I cannot answer. He certainly did not have a direct influence, because he did not stimulate one to think of new equations.

At that time, I was just a research student with no duties apart from research, and I concentrated all my energy in trying to get a better understanding of the problems facing physicists at that time. I was not interested at all in politics, like most students nowadays. I confined myself entirely to the scientific work, and continued at it pretty well day after day, except on Sundays when I relaxed and, if the weather was fine, Itook a long solitary walk out in the country. The intention was to have a rest from the intense studies of the week, and perhaps to try and get a new outlook with which to approach the problem the following Monday. But the intention of these walks was mainly to relax, and I just had the problems maybe floating about in the back of my mind without consciously bringing them up.

That was the kind of life that I was leading. There was excitement produced from time to time. There was the excitement of the Bohr-Kramers-Slater theory. That provided a new outlook, and it seemed to me to be a very reasonable outlook. With the backing of BOHR behind it, it seemed to me that here was a theory that was certainly worth considering. It meant giving up detailed conservation of energy, but I did not especially mind that. Conservation of energy had only been proved statistically. Here was a way which did seem to provide an escape from some of the fundamental difficulties concerned with understanding radiation. Radiation was emitted continuously in waves, absorbed suddenly in the form of quanta—a picture which people had not previously thought of, but which did account for all the experimental evidence available at the time.

The satisfaction which we had with the theory of Bohr-Kramers-Slater was short lived, because within a year, GEIGER and BOTHE had made precise experiments with the scattering of X-rays by electrons, and had checked that conservation of energy did hold in detail. So that was just a passing interest which faded out.

I might mention that the Bohr-Kramer-Slater idea was revived in 1936 by SHANKLAND, who did experiments like the Geiger-Bothe experiments using gamma-rays instead of X-rays, and SHANKLAND reported that, in the case of gamma-rays, there was no detailed conservation of energy.

Well, at that time I had a great respect for experimenters and when they asserted something confidently, I had a tendency to believe them, and I rather accepted Shankland's point of view. I tried to think how it could be that conservation of energy broke down in the case of high energies, even though it held for lower energies, and I was even sufficiently interested in it to write an article in *Nature* about it.

However, within a year SHANKLAND repeated his experiments and he reported that his earlier results were inaccurate and there was detailed conservation of energy, so we were back again to the point of view of the precise quantum theory preserving accurately conservation of energy.

Another of these early ideas was provided by DE BROGLIE. He put forward a theory connecting particles with waves. Now, this was a very beautiful theory because the connection was relativistic, and the beauty appealed to me immediately. The connection is such that, when you make the rest mass of the particles go to zero, you get the relationship between light quanta and electromagnetic waves.

Although I appreciated very much the beauty of de Broglie's work, I could not take his waves seriously. I was so much imbedded in the Bohr theory, I took the Bohr orbits very literally—we had the electrons as real particles, and the de Broglie waves seemed to me to be just a mathematical fiction of no importance to physicists.

Of course, I was very wrong in this point of view. SCHRÖDINGER also read de Broglie's work. He had a different outlook. He also had a different training. He had a training involving very much eigenvalues and eigenvectors, which I knew nothing about. And SCHRÖDINGER with his different outlook was able to develop de Broglie's ideas and get to a brilliant result. He was able to extend the primitive ideas of DE BROGLIE, which applied only to free particles, to particles moving in an electromagnetic field, and that led to wave mechanics.

Well, that was one of the examples where I was seriously at fault.

You might wonder, what was the sort of work I was doing on my own trying to solve problems of physics? Well, I was very much concerned with understanding Hamiltonian dynamics. I had read Sommerfeld's book *Atomic Structure and Spectral Lines*. There was an English translation available, which was rather fortunate because I was not very fluent in German at that time, and that provided the basis for getting a working knowledge of atomic theory. There was an appendix to Sommerfeld's book dealing with Hamiltonian dynamics and its applications to quantum theory. I studied this Hamiltonian theory very closely, and I also did some other reading about Hamiltonian dynamics. I read up about the advanced transformation theory connected with Hamiltonian dynamics, and became familiar, or at any rate acquainted, with the general ideas of this theory.

There were many meetings among the students in Cambridge to discuss scientific problems, and among those there was the Kapitza Club. KAPITZA was a young physicist who had come from Russia. He was very talented and RUTHERFORD appreciated his talents and helped him to get established in Cambridge. KAPITZA also had a very dynamic personality and he established a club of physicists, theoretical as well as experimental. We would meet on Tuesday evenings after dinner when someone would read a paper on some recent development in physics.

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.

In the Summer of 1925, HEISENBERG came to Cambridge, and he gave a talk to the Kapitza Club. The main subject of his talk was « Anomalies in the Zeeman Effect », and I followed most of it. Towards the end, though, he spoke about some new ideas of his. By that time I was just too exhausted to be able to follow what he said, and I just did not take it in. He was talking about the origins of his ideas of the new mechanics. But I completely failed to realize that he was really introducing something quite revolutionary. Later on I completely forgot what he had said concerning his new theory. I even felt rather convinced that he had not spoken about it at all, but other people who were present at this meeting of the Kapitza Club assured me that he had spoken about it. In particular FOWLER was quite positive, and I just have to accept that he really did speak about it and that I had failed to respond to it at all, and so missed a great opportunity of getting started on it.

It was a little later when I really got started on the new Heisenberg theory. Toward the end of August I returned to Bristol to be with my parents for part of the vacation, and HEISENBERG sent to FOWLER the proofs of his first paper on the new mechanics. FOWLER sent it on to me with a query, « What do you think of this? »

I received it either the end of August or the beginning of September, I am not sure of the date, and of course I read it. At first I was not very much impressed by it. It seemed to me to be too complicated. I just did not see the main point of it, and in particular his derivation of quantum conditions seemed to me to be too far fetched, so I just put it aside as being of no interest. However, a week or ten days later I returned to this paper of Heisenberg's and studied it more closely. And then I suddenly realized that it did provide the key to the whole solution of the difficulties which we were concerned with.

My previous work had been all concerned with studying individual states. If you want to know about the false trails of work that I had been engaged on, one of them was that I had examined the Hamiltonian theory of planetary perturbations and wondered whether such a theory could not be applied to the interaction of the various electrons in a Bohr atom. That work gave me a mastery of Hamiltonian methods, but of course it did not lead anywhere.

HEISENBERG brought out the quite new idea that one had to consider quantities associated with two stationary states rather than just one.

I think I had better stop at this point, and take it up later.

II.

I told you yesterday about the background and training which I had for approaching the problems of the new mechanics. The background was essentially an intense interest in mathematical beauty which had been especially built up through studying projective geometry. Also I was very much interested in relativity, which was a new subject which had just recently appeared and was absorbing everyone's interest. And then there was quantum mechanics, which was full of problems and difficulties, which I had thought about intensively without making any real progress. I do not think I would ever have made any progress in studying atomic theory if it had not been for HEISENBERG. I was so much attached to the Bohr orbits. It needed quite a different kind of intelligence to be able to break away from just building up theories in terms of Bohr orbits.

My work during the first two years at Cambridge, which was before Heisenberg's theory appeared, was very much concerned with relativity. I did not point this out sufficiently yesterday. There was a sort of general problem which one could take, whenever one saw a bit of physics expressed in a nonrelativistic form, to transcribe it to make it fit in with special relativity. It was rather like a game, which I indulged in at every opportunity, and sometimes the result was sufficiently interesting for me to be able to write up a little paper about it.

That was the situation up to September 1925, when I had the opportunity of studying Heisenberg's first paper. As I said yesterday, my first reaction was unfavorable, and it needed about ten days or so before I was really able to master it. And I suddenly became convinced that this would provide the key to understanding atoms.

Now, what did I do under those conditions? You will perhaps be able to guess. I was dissatisfied with the nonrelativistic form of Heisenberg's work, and I wondered whether I could transcribe it so as to fit it in with special relativity by the same sort of arguments which I had used on various occasions previously.

The special feature of Heisenberg's work that jarred against relativity was that he had a number of matrix elements forming the building bricks of his theory, and each of them was associated with two energies. It was associated with two states, referring to two energy levels.

The obvious step to take from that was to suppose that, if these matrix elements each referred to two energies, they would each refer to two momentum values. Now, if you had these matrix elements referring quite generally to two energies and two momenta, that gave you a situation where you could not combine them very reasonably. The whole structure was too loose. So it occurred to me that you would have to put some restriction on the two momentum values. The natural restriction to take was that the difference between the two momentum values would be equal to the difference between the two energy values, divided by c, the velocity of light, and that the direction of this momentum difference would be the same for all the matrix elements.

That would mean that these matrix elements were all connected with light moving in one particular direction. This was a rather artificial situation, but still, from the relativistic point of view, it was less artificial than having all the matrix elements referring just to different energies in one particular Lorentz frame of reference and all referring to no momentum change.

Well, there was a definite idea which I could start to work on, and I proceeded to write it up, but I never got very far with it. I suppose I soon saw that this was not the essential problem, and this work I dropped.

These ideas did reappear about a year later in some work that I wrote on relativistic quantum mechanics with an application to Compton scattering. This contained the essence of these early ideas of matrix elements each referring to two energy levels and two momentum values, with a momentum difference equal to the energy difference divided by the velocity of light.

I was in Bristol at the time I started on Heisenberg's theory. I had returned home for the last part of the summer vacation, and I went back to Cambridge at the beginning of October, 1925, and resumed my previous style of life, intense thinking about these problems during the week and relaxing on Sunday, going for a long walk in the country alone. The main purpose of these long walks was to have a rest so that I would start refreshed on the following Monday.

The question that was bothering me was of course the noncommutation of the dynamical variables. HEISENBERG had set up a theory where the dynamical variables correspond to matrices, and they are such that, for two variables u and v, if you multiply them in that order to get uv, the result is not the same as if you multiply them in the reverse order to get vu. There is a difference $uv - vu$ which was very hard to understand.

I heard later on that HEISENBERG himself was extremely worried when he first noticed that uv was not the same as vu. He must have found it out pretty soon, and he was naturally very disturbed by it because it is a result that is completely foreign to physicists. They had been brought up to deal with physical problems on the basis of Newton's laws, and everything that followed from that, always assuming that the product of dynamical variables was commutative. When HEISENBERG first noticed this noncommutation, he felt afraid that this was a fatal objection to his theory and that he would have to abandon it. I think he needed quite a bit of support from his professor BORN to carry on in spite of this really frightening disturbance, coming essentially from the revolutionary character of the new ideas which he was bringing forward.

Of course, I did not have this fear of the whole theory collapsing which HEISENBERG must have had to begin with, so I was able to approach the whole question more boldly, and I realized pretty soon that the noncommutation was really the most important feature of the new theory. Just what disturbed HEISENBERG so much was really the main new feature of the theory, and it is what one had to try to understand. In trying to understand this, of course, I had as background the Hamiltonian form of dynamics with which I was familiar.

It was during one of the Sunday walks in October, 1925, when I was thinking very much about this $uv-vu$, in spite of my intention to relax, that I thought about Poisson brackets. I remembered something which I had read up previously in advanced books of dynamics about these strange quantities, Poisson brackets, and from what I could remember, there seemed to be a close similarity between a Poisson bracket of two quantities, u and v, and the commutator $uv-vu$.

The idea first came in a flash, I suppose, and provided of course some excitement, and then of course came the reaction « No, this is probably wrong. »

I did not remember very well what a Poisson bracket was. I did not remember the precise formula for a Poisson bracket, and only had some vague recollections. But there were exciting possibilities there, and I thought that I might be getting on to some big new idea. It was really a very disturbing situation, and it became imperative for me to brush up my knowledge of Poisson brackets and in particular to find out just what is the definition of a Poisson bracket.

Of course, I could not do that when I was right out in the country. I just had to hurry home and see what I could then find about Poisson brackets. I looked through my notes, the notes that I had taken at various lectures, and there was no reference there anywhere to Poisson brackets. The textbooks which I had at home were all too elementary to mention them. There was just nothing I could do, because it was a Sunday evening then and the libraries were all closed. I just had to wait impatiently through that night without knowing whether this idea was really any good or not, but still I think that my confidence gradually grew during the course of the night. The next morning I hurried along to one of the libraries as soon as it was open, and then I looked up Poisson brackets in Whittaker's *Analytical Dynamics*, and I found that they were just what I needed. They provided the perfect analogy to the commutator.

The precise formula for a Poisson bracket is the following:

$$[u,v]=\sum_r\left(\frac{\partial u}{\partial q_r}\frac{\partial v}{\partial p_r}-\frac{\partial u}{\partial p_r}\frac{\partial v}{\partial q_r}\right). \tag{2}$$

The q's and the p's there are a set of Hamiltonian variables describing the dynamical system, and the sum is taken over all the degrees of freedom. You see that it is a rather complicated formula, and I think I can be excused for not having it clearly in my mind during my walk, because there was no reason to believe that it was of any importance. It was just something which had appeared in the books when I was reading about the transformation theory of dynamics, and I had not bothered to learn the details by heart.

The situation was really rather confused by the fact that there was another kind of bracket expression also occurring in this theory, the Lagrange bracket. The Lagrange bracket is very similar to the Poisson bracket in general appearance, but has of course a different significance in the details. The way things have turned out, the Poisson bracket is of very great importance and the Lagrange bracket is of no importance at all, from the point of view of people studying quantum theory.

This idea of connecting Poisson brackets with commutators formed the beginning of my work on the new quantum mechanics. The connection between these two things, which look very different, is very close when one examines their properties. It is so close that one finds that one only needs to put in a suitable numerical coefficient, $ih/2\pi$, and then one can say this quantity $uv - vu$ is the analog in quantum mechanics for the quantity

$$\frac{ih}{2\pi}\sum_r\left(\frac{\partial u}{\partial q_r}\frac{\partial v}{\partial p_r}-\frac{\partial u}{\partial p_r}\frac{\partial v}{\partial q_r}\right) \tag{3}$$

in classical mechanics.

The importance of this step is that it provides us with some way of handling the dynamical variables in the quantum theory which takes the place of the partial differentiations which we have in classical mechanics. In classical mechanics we have our dynamical variables which we can add and multiply; we can do the same with our quantum variables. In classical mechanics we have differentiation with respect to the time t. We can take this process over directly into the quantum theory by supposing that the quantum variables like u and v are functions of a time parameter t. But there is no immediate way of taking over processes of partial differentiation into the quantum theory until one sets up this analogy. Then, whenever one has to do a partial differentiation with respect to some variable in classical mechanics, there is a corresponding process which one can do in the quantum theory of taking the commutator of the quantity with a certain other dynamical variable.

It was this result—that we have a process of differentiation of variables applicable to quantum mechanics—which appeared to me to be the main consequence of the work, and so I started to consider the problem again from the general point of view of just looking for a process of differentiation which we can apply to quantum variables.

Let us forget this formula connecting the commutator and the Poisson bracket and start again, and look for a process of differentiation which can be applied to quantum variables.

We have a quantum variable x. Let v be some other quantum variable. These quantum variables are both represented by matrices in accordance with Heisenberg's ideas. What can one do to give a meaning to dx/dv?

Well, first of all one should require dx/dv to be a linear function of x. This requires that the matrix elements of dx/dv shall be linear functions of the matrix elements of x. Thus

$$(dx/dv)(nm) = \sum_{n'm'} a(nm;\, n'm')x(n'm'), \tag{4}$$

where the a's are unknown coefficients, but they have to be independent of x.

Then we can go on to put down the requirement that we differentiate a product xy according to the law

$$\mathrm{d}(xy)/\mathrm{d}v = (\mathrm{d}x/\mathrm{d}v)y + x(\mathrm{d}y/\mathrm{d}v)\,, \tag{5}$$

y being another quantum variable, represented by a matrix.

We find that the conditions which are needed for this to hold are that $\mathrm{d}x/\mathrm{d}v$ must be of the form

$$\mathrm{d}x/\mathrm{d}v = xa - ax\,, \tag{6}$$

where a is another quantum variable represented by a matrix. I then proceeded to examine this equation to see what it corresponds to in the case when we are dealing with large quantum numbers, and was led to the formula connecting the commutator and the Poisson bracket.

I wrote up my first paper on quantum mechanics following those lines. Most of the papers that I wrote followed the lines of presenting the ideas in the order in which they had occurred to me, but here I made an exception. I did not want to write up the paper based primarily on the idea of connecting the commutator and the Poisson bracket, which had come to me rather out of the blue (I could not really say how it came) and I preferred to set up the theory on this basis where there was some kind of logical justification for the various steps which one made. One had the need for getting a process of differentiation into the quantum theory, and this process of differentiation had to satisfy the linearity condition and also the product law (5). On that basis, one could set up an argument which leads to this formula (6), and connect it with the Poisson bracket.

I wrote up that paper. It was communicated by FOWLER to the Royal Society. The Royal Society appreciated the importance of this work and published it very quickly, much more quickly than papers usually took.

I also sent a copy of the paper to HEISENBERG, and I got a fairly prompt answer from him. I would like to tell you about this letter from HEISENBERG and some later letters. That is information that is not available from published work. HEISENBERG wrote to me in German. I knew enough German to be able to follow pretty well what he said. If one makes a rough translation of Heisenberg's letter, it would read like this (*):

« I read your beautiful work with great interest. There can be no doubt that all your results are correct, insofar as one believes in the new theory. » I think that phrase in Heisenberg's letter is a remarkable one, because it shows that HEISENBERG did not really have very much confidence in his theory, or at any rate professed to feel that there still might be doubts about it—he brings in this phrase, « insofar as one believes in the new theory ».

(*) HEISENBERG to DIRAC, 20 November 1925, 2 pages, in German.

« Your representation of the frequency condition and energy law are simpler and more beautiful than the proof of the equation $\nu = \partial H/\partial J$ in the classical theory »—that refers to the connection between frequency and energy which one has in classical Hamiltonian theory.

HEISENBERG goes on: « I hope you are not disturbed by the fact that part of your results have already been found here some time ago and are being published independently in two papers, one by BORN and JORDAN the other by BORN, JORDAN and me. Your results, in particular the general definition of differentiation and the connection of the quantum conditions with the Poisson bracket, go considerably further. I would like to refer to one point which is not important ». He refers now to the quantum condition which I gave in my paper. For a system of one degree of freedom the only quantum condition is

$$2\pi m(q\dot{q} - \dot{q}q) = ih \, . \tag{7}$$

Equating the constant part of the left-hand side to ih, one gets Heisenberg's quantum condition, but I said in my paper that, equating the remaining components of the left-hand side to zero, one gets further conditions which are not given by Heisenberg's theory.

That was what HEISENBERG objected to. He said that those further conditions are

$$\frac{\mathrm{d}}{\mathrm{d}t}(pq - qp) = 0 \, . \tag{8}$$

In special cases, this follows from the equations of motion, namely in those cases when H is a function of q plus a function of p. He gives details of that calculation, and he says that he believes this result can be proved generally provided H is real.

He says again that this is not an important point. The letter goes on to say « In the work of BORN, JORDAN and me, we tried to give a fairly complete representation of the whole theory, with perturbation theory, degenerate cases, etc. » One can then deduce the Kramers-Heisenberg dispersion formula. He did not put it that way, he said « one can then deduce the Kramers dispersion formula », although everybody calls it the Kramers-Heisenberg dispersion formula.

He said finally that « PAULI has succeeded in getting the theory of the hydrogen atom and the Balmer formula on quantum mechanics. I would willingly send you proofs of this paper and would be glad to hear of your further progress ».

This was really a very kind letter that HEISENBERG wrote to me. I suppose I was a stranger to him at that time. I had seen him when he came to Cambridge and spoke at the Kapitza Club, but I was just a member of the audience. I do not know whether I was actually introduced to him. A lecturer would not normally pay any attention to just one member of the audience unless there was some special reason for it, and at that time there was no reason why I should be specially singled out to be introduced to HEISENBERG.

It was a very kind letter, because he did not want me to be disturbed by the fact that there were other people simultaneously working on these problems who had anticipated my results to some extent, and he was also full of praise for my own contributions.

I got a second letter from HEISENBERG just three days later (*). This second letter was sent off of course before he could have received any answer to his first letter. In his second letter he says « Since my last letter I have had considerable discussion of your work with Jordan, and some questions have come up which I would like you to explain. As a specially satisfying result, I find your general formula for differentiation—that is this formula (6)—and while I have no doubt about the correctness of the result, your proof is not clear to us. You conclude from the linearity condition this equation ». Equation (4). He says, « At first sight, this seems to imply that one could use the same argument for ordinary functions, which would lead to

$$\mathrm{d}x/\mathrm{d}v = ax\,, \qquad x = e^{av}\,, \tag{9}$$

which is not in general true. I do not write this as an objection, but I would like you to give your deduction more explicitly. I also have a wish with respect to a second point. It seems to me that the physical content of the theory is not sufficiently characterized when one says that the mathematical operations which are used to deduce physical results are different from the classical theory. I would rather believe that we really have to do with a change in the kinematics. This brings with it that there can be no question of the validity of classical mechanics. In spite of this, it is possible on the basis of the new kinematics to build up a mechanics which is largely analogous to the classical one, and which allows the energy law and frequency condition to be proved. I do not know if this difference appears to you unimportant, but it seems to me that an important physical point lies in it. »

Well, I cannot remember just what I wrote in answer to these letters of HEISENBERG. It was nearly 50 years ago. I suppose I answered something like this, that there was quite a considerable difference between his argument that led to $x = e^{av}$ and the argument in my paper. With regard to the second point I probably answered that I did think that the argument he gave was rather unimportant because it did not affect the equations.

The letters which I wrote to HEISENBERG on this and later occasions were kept by HEISENBERG until the end of the war, 1945. Then he had all of his important papers put together and they were removed by the American military authorities, and HEISENBERG has been unable to get them back. Presumably these papers are now lying somewhere among the secret files of the American Atomic Energy Commission. Still we must count them as lost for the present. Maybe at some future date they will be unearthed and historians of science

(*) HEISENBERG to DIRAC, 23 November 1925, 2 pages, in German.

will be able to get at them. But for the time being, we must do the best we can without them. I can only imagine what I would have answered to these letters, but of course, with the basis of much greater knowledge at the present time, the answers I imagine might be different from what I actually did answer then.

HEISENBERG wrote to me a third letter on the lst of December, 1925, and he says (*) « Many thanks for your interesting letter. Unfortunately in my last letter I expressed myself unclearly in some points, and I would like to ask you again about it. My thoughts against the derivation of the formula (4) were intended so. If this formula follows generally from the condition of linearity

$$\frac{\mathrm{d}}{\mathrm{d}v}(x+y)=\frac{\mathrm{d}x}{\mathrm{d}v}+\frac{\mathrm{d}y}{\mathrm{d}v}, \tag{10}$$

then it must apply also when one has only a single stationary state. In fact, nowhere is the assumption made that the number of stationary states must be infinite. Thus for a single stationary state, when n and m and n' and m' are all equal to one, say, one would get the equations

$$\frac{\mathrm{d}x(11)}{\mathrm{d}v}=a(11,11)x(11)\,,\qquad \frac{\mathrm{d}y(11)}{\mathrm{d}v}=a(11,11)y(11)\,. \tag{4'}$$

Now let $x(11)=v^n$, $y(11)=v^m$. Your relations (10) (see above) and (5) are certainly fulfilled, but not (4′) which gives in the first case $a(11,11)=1/nv$ and in the second $a(11,11)=1/mv$, which seems to lead to a contradiction. Please do not think of this as a criticism, but only as a proof that it is difficult to understand your equations without further explanation.

A further point has given me much to think about in the course of the discussions. You write in particular the energy will be the same function of the action variables as in the classical theory. I find it hard to believe that this result is true in general. » And then he proceeds to give a counter-example of the anharmonic oscillator.

Well, the answer to this letter of Heisenberg's I suppose would run something like this: « In the case when there's only one stationary state, noncommutation is impossible, so the process of differentiation breaks down, and that is why the argument, trying to follow through with only one stationary state, leads to a contradiction. »

The second point which HEISENBERG raised in that letter was quite correct. I had been careless in saying that the energy is the same function of J in the classical theory and in the quantum theory, and it was quite correct of HEISENBERG to point out this mistake to me. HEISENBERG actually said that one would not be happy if my statement were true, because then one would have no

(*) HEISENBERG to DIRAC, 1 December 1925, 3 pages, in German.

hope to understand the complicated spectra for which the classical calculation of the energy completely fails.

He says at the end of this letter « Please do not take these questions that I write to you as criticisms of your wonderful work. I must now write an article on the state of the theory for the *Mathematischen Annalen*, and still wonder about the mathematical simplicity with which you have overcome this problem. »

This letter was followed by a postcard written on the same day, and he says (*) « In my last letter which I sent to you this afternoon, I forgot to mention a considerable difficulty which has come up in connection with your equation (11). » That is the equation

$$xy - yx = \frac{ih}{2\pi}[x, y]\,. \tag{11}$$

« Let us take the case of one degree of freedom, and take the case when x is equal to p^2 and y is equal to q^2. Then your equation gives

$$xy - yx = p^2q^2 - q^2p^2 = \frac{ih}{2\pi}[p^2, q^2] = -\frac{ih}{2\pi}4qp\,. \tag{12}$$

But a simple calculation gives

$$p^2q^2 - q^2p^2 = p(pq^2) - (pq^2)p + p(q^2p) - (q^2p)p = \frac{h}{2\pi i}(2pq + 2qp)\,. \text{ »} \tag{13}$$

This considerable difficulty that HEISENBERG refers to here is just the fact that the Poisson bracket as worked out in the quantum theory is equal to the classical Poisson bracket only in simple cases; in the more complicated cases, one has to use directly the result that one gets from the commutator rather than taking over the classical formula. In effect, that was the answer that I gave to HEISENBERG with regard to that difficulty.

That was the beginning of quantum mechanics so far as I was concerned. I might mention that, in writing up my paper, I gave careful consideration to the question of notation. I feel that people who are writing up work on a new subject should pay considerable attention to this question of notation, because they are starting something which will very likely get perpetuated, and if they perpetuate a bad notation they are really doing harm to the future development of the subject.

One of the questions of notation which I had to face was with regard to the Poisson bracket. I had got the information about Poisson brackets from Whittaker's *Analytical Dynamics*, and Whittaker's notation had round brackets for them. He used the square notation for the Lagrange brackets. Now, in quantum theory we do not want the Lagrange brackets at all, we only want

(*) HEISENBERG to DIRAC, 1 December 1925, in German.

the Poisson brackets, and it seemed to me that this notation was unsuitable. It makes one think of the scalar product which one has in vector analysis, and the scalar product is symmetrical between the two terms that are mentioned in it, while with the Poisson bracket we have something which is antisymmetrical between the two terms mentioned in it. So I boldly used the other type of bracket for the Poisson bracket, departing from Whittaker's notation. Everyone has copied me since then, and it is really quite suitable to have square brackets always referring to a quantity which is antisymmetrical between the two terms mentioned in it.

Another question of notation—it often happens that $uv = vu$ is a special case. When that is so, the mathematicians who had been handling noncommutative algebra said that u permutes with v. It seemed to me that the word « permute » was not really very appropriate. One thinks of permutations as rearranging the order of several quantities, and here we are concerned only with two quantities. So I invented the word « commute. » I do not think it had been previously used in mathematics. I said that, when $uv = vu$, u and v commute with each other. That again is a notation which everyone has accepted since then.

Well, the situation which I had got into now was that I was dealing with these new variables, the quantum variables, and they seemed to me to be some very mysterious physical quantities, and I invented a new word to describe them. I called them q-numbers, and the ordinary variables of mathematics I called c-numbers, to distinguish them. The letter q stands for quantum, or maybe queer, and the letter c stands for classical or maybe commuting. Then I proceeded to build up a theory of these q-numbers; c-numbers can be looked on just as a special case of q-numbers, having the property that they commute with everything.

Now, I did not know anything about the real nature of q-numbers. Heisenberg's matrices I thought were just an example of q-numbers; maybe q-numbers were really something more general. All that one knew about q-numbers was that they obeyed an algebra satisfying the ordinary axioms except for the commutative axiom of multiplication.

I proceeded to develop a theory in which I felt free to make any assumptions I wanted to, unless they led immediately to an inconsistency. I did not bother at all about finding a precise mathematical nature for q-numbers, or about any kind of precision in dealing with them.

I think you can see here the effects of an engineering training. I just wanted to get results quickly, results which I felt one could have some confidence in, even though they did not follow from strict logic, and I was using the mathematics of engineers, rather than the rigorous mathematics which had been taught to me by FRASER.

It was perhaps the most suitable attitude to take for a quick development of the theory, but it did lead me to make mistakes. One of the mistakes that

I made was to assume that every q-number has a reciprocal. Another mistake that I made was to assume that if the product of two things, A times B, is equal to zero, then one of the factors must be zero.

I got a letter from BRILLOUIN pointing out these mistakes to me. He wrote to me in March 1926, and he pointed out that the assumptions that I had made about q-numbers were not valid for matrices. It took me quite some time to get reconciled to the view that my q-numbers were not really more general than matrices, and had to have the same limitations that one could prove mathematically in the case of matrices.

Another assumption that I made was the following one.

I assumed that, if we had any two q-numbers, u and v, one could always find a q-number b such that $v = bub^{-1}$. On the basis of that assumption, I was able to set up a general theory of functions of q-numbers, which worked very nicely mathematically; but this assumption of course is not true. We have learned now that this assumption can be true only in the special case when u and v have the same eigenvalues.

However, these mathematical points did not bother me at the time, and I proceeded to develop my equations. I soon wrote a second paper in which I applied the methods of just handling these q-numbers in accordance with the algebraic rules to get a theory of the hydrogen spectrum and to deduce in particular the Balmer formula.

The method was just to take the equations of motion for the electron and count the dynamical variables as q-numbers, and then proceed to solve the equations. I worked in two dimensions only and that was adequate to get the result that I wanted. I had already heard from HEISENBERG that PAULI had succeeded in getting an application of quantum mechanics to the hydrogen atom, and I was really competing with him at this time.

I should mention that, while I was doing this work on q-numbers, a paper appeared by LANCZOS that involved transforming the Heisenberg matrices into functions of two continuous variables. I was not very much impressed by this paper because it seemed to me to be just a mathematical development which would not help forward the physics at all. I was really perfectly satisfied with my own method. But I was wrong in attaching so little importance to this work of LANCZOS which was really quite an important development, and paved the way for the later connection that was given between Heisenberg's matrices and the Schrödinger form of quantum mechanics.

When I wrote up my second paper containing the application to the hydrogen atom, I again sent a copy of it to HEISENBERG, and I got Heisenberg's answer, where he states the following (*):

« Since some days I am back in the world of physics, and I found your last work on the hydrogen atom. I congratulate you. I was quite excited as I

(*) HEISENBERG to DIRAC, 9 April 1926, 2 pages, in German.

read the work. Your division of the problem into two parts, calculation with q-numbers on the one side, physical interpretation of q-numbers on the other side, seems to me completely to correspond to the reality of the mathematical problem. With your treatment of the hydrogen atom, there seems to me a small step towards the calculation of the transition probabilities, to which you have certainly approached in the meantime. Now one can hope that everything is in the best order, and, if THOMAS is correct with the factor 2, one will soon be able to deal with all atom models. »

This reference to the Thomas factor 2 refers to the newly proposed spin of the electron. The idea of the spin of the electron had been introduced by GOUDSMIT and UHLENBECK and they applied it to describe the doublets which occur in the spectra of the alkali elements. The electron spin did explain the existence of the doublets, but it gave for the doublet separation twice the observed value. THOMAS showed that the factor 2 came from an error in the calculation, involving the use of a formula for the precession of the spin in a frame of reference in which the electron is at rest, whereas one should take into account the motion of the electron.

Heisenberg's letter continues: « The real reason for my letter is naturally that I want to ask you a few questions. A few weeks ago an article by SCHRÖDINGER appeared in the *Annalen der Physik* (vol. 79, p. 301), whose contents according to my opinion are closely connected with quantum mechanics. Have you considered how far this Schrödinger's treatment of the hydrogen atom is connected with quantum mechanics? These mathematical questions interest me especially because I believe that one can win a great deal for the physical significance of the theory. »

Well, my answer to that was that I had not considered Schrödinger's theory. I felt at first a bit hostile towards it. The reason was that I felt that we had already a perfectly good quantum mechanics, which I believed could be developed for handling all the problems of atomic theory. Why should one go back to the pre-Heisenberg stage when we did not have a quantum mechanics and try to build it up anew? I rather resented this idea of having to go back and perhaps give up all the progress that had been made recently on the basis of the new mechanics and start afresh. I definitely had a hostility to Schrödinger's ideas to begin with, which persisted for quite a while. I do not know in detail just what I answered to HEISENBERG about this, but I got his answer again, which was dated the 26th of May (*).

HEISENBERG begins with a detailed exposition of the connection between the Schrödinger theory and matrix mechanics. He went to the trouble of writing out in two or three pages the details of this connection. It was very helpful to me.

HEISENBERG goes on to say that he agrees with my criticism of Schrödinger's

(*) HEISENBERG to DIRAC, 26 May 1926, 4 pages, in English.

paper, that the wave theory of matter must be inconsistent just like the wave theory of light, but the real progress of Schrödinger's theory is that the same mathematical equations can be interpreted as point mechanics in a nonclassical kinematics, and as wave theory according to SCHRÖDINGER. HEISENBERG hoped that the solution of the paradoxes in the quantum theory would be found in this way. HEISENBERG asked for more information about what I had done with the Compton effect. He said that « People in Copenhagen have discussed the problem so much and are very interested in it ».

I should say that I had been developing my theory of q-numbers and had found a way of making the theory to some extent relativistic, following the lines of the very first ideas which I had when I read Heisenberg's paper in September 1925, and which I spoke to you about yesterday.

In March of 1926, SOMMERFELD visited Cambridge and I had a chance of meeting him. I was invited by EDDINGTON to a tea on March the 13th, where SOMMERFELD was also present. I was very happy to meet SOMMERFELD because I had learned so much from his book, and, during the course of the talk which we had then, I mentioned that I had worked out the problem of the Compton effect according to quantum mechanics. SOMMERFELD rather flared up at this point and said « Now, why haven't I heard about this? ». FOWLER, who was present also at the tea party, said that I had only just done this work, and soothed SOMMERFELD down.

I had worked out this theory of the Compton effect and published it soon afterwards, and this is what HEISENBERG was referring to in his letter. I wrote up all this work for my doctor's dissertation. I wrote it up in the Spring of 1926. At that time, there was a general strike going on in England. Everyone who was willing was called upon to do public service, like driving a train or a bus or something, to try to keep the essential services going, and a great many of my fellow students did leave their studies and go over to this kind of work. But I was too absorbed in writing up my thesis, and just stuck to it, and completed it in June 1926.

III.

I wrote my doctor's dissertation in the Spring of 1926. I continued working at it steadily for some time, unperturbed by a general strike which was occurring in England at the time and which had disrupted very many people's activities. In this dissertation there were still some mistakes in my general ideas of q-numbers. Also there was no reference to Schrödinger's theory. I think I told you last time that when Schrödinger's theory first appeared, I rather resented it. I felt that we had, as a result of Heisenberg's work, quite a satisfactory foundation for quantum mechanics, and we could continue to develop that quite happily without there being any need for a further revision of the foundations.

However, one of the letters that I received from HEISENBERG explained in detail the connection between Schrödinger's theory and the matrix mechanics, and I saw from that that Schrödinger's theory would not require us to unlearn anything that we had learned from matrix mechanics; Schrödinger's theory, quite to the contrary, just supplemented the matrix mechanics and provided very powerful mathematical developments which fitted in perfectly with the ideas of matrix mechanics.

Of course, after that my ideas of the Schrödinger theory changed; maybe not immediately, it took a little while. Then I took up Schrödinger's theory with enthusiasm, learning all I could about it. I had to learn a new technique, the technique of eigenvalues and eigenvectors. It was a technique that SCHRÖDINGER had learned in his early training, but it was very little known in Cambridge.

After I had mastered this new technique, I considered how it could be made use of, and I was led to study the problem of an atomic system with many similar particles. I thought of the possibility of having a wave function which is symmetrical with reference to all the particles, or alternatively one that is antisymmetrical. These symmetry questions brought in the possibility of new laws of Nature. Examining their consequences I found that with the symmetrical wave functions we had the particles obeying a statistics which was precisely that which had been originally proposed by BOSE and somewhat corrected by EINSTEIN. This statistics was known as the Einstein-Bose statistics. It applied to photons and gave an explanation for Planck's law.

Then there were the antisymmetrical wave functions, which gave a new statistics. I worked out the basic relations for this new statistics, and I published this work.

Soon after publication I got a letter from FERMI pointing out that this statistics was not really a new one; he had proposed it some time earlier. He gave me a reference to where he had published this work. I looked up the reference and found that it was indeed as FERMI had said in his letter. He had considered the statistics which had the characteristic that there could not be more than one particle in any state.

When I looked through Fermi's paper, I remembered that I had seen it previously, but I had completely forgotten it. I am afraid it is a failing of mine that my memory is not very good and something is likely to slip out of my mind completely, if at the time I do not see its importance. At the time that I read Fermi's paper, I did not see how it could be important for any of the basic problems of quantum theory; it was so much a detached piece of work. It had completely slipped out of my mind, and when I wrote up my work on the antisymmetrical wave functions, I had no recollection of it at all.

I then wrote an apologetic letter to FERMI. I felt that FERMI had reason to be angry with me and that I should placate him. FERMI must have forgiven me, because he never wrote any further letter to me on the subject, and when we met in later life, he was most friendly. We never had any discussion about who was the author of the statistics. The statistics is now often connected with both our names. But the published records show quite clearly that it was first proposed by FERMI, and my later work showed how it could be fitted in with quantum mechanics, and is in fact a consequence of quantum mechanics, when one makes the further assumption that the wave functions have to be antisymmetrical.

After I had obtained my PhD degree, I was no longer confined to Cambridge, and I felt I would like to travel. The place which was most attractive to me was of course Göttingen, the birthplace of quantum mechanics. That was where HEISENBERG lived, and it contained also BORN and JORDAN, who had been extremely active in starting off matrix mechanics. However, when I talked about it with FOWLER, he suggested that I should go to Copenhagen. FOWLER himself had very close connections with Copenhagen. He had been there frequently. He told me how friendly the Institute in Copenhagen was, how BOHR was so friendly with everyone who visited his Institute, and I was therefore undecided over the question whether I should go to Copenhagen or to Göttingen. I decided eventually to divide my time during the coming year between these two places, going first to Copenhagen.

I went to Copenhagen in September of 1926, and I was very glad that I did so because I found, as FOWLER had said, that it was an extremely friendly place and that BOHR was especially friendly to me. I learned to become closely acquainted with BOHR, and we had long talks together, long talks in which BOHR did practically all of the talking.

BOHR had a habit, it seemed, of thinking aloud, doing all his very deep thinking aloud, and he liked to have an audience, maybe the audience of a lecture room or else the audience of just one or two people. Very often I was just his audience during this process of thinking aloud. I admired BOHR very much. He seemed to be the deepest thinker that I ever met. His thoughts were of a kind which were, I would say, rather philosophical. I did not understand them completely, although I struggled as hard as I could to understand them. My own line of thinking was really to put emphasis on thoughts which could be expressed in the form of equations, and much of Bohr's thoughts were of a more general character and rather remote from mathematics. But still I was very happy to have this close connection with BOHR and, as I mentioned once before, I am not sure to what extent hearing all these thoughts of BOHR influenced my own work.

Another person whom I met in Copenhagen, who had a very profound influence on everyone whom he came into contact with, was EHRENFEST. EHRENFEST would insist on absolute clarity in every detail in a discussion. He would never let a speaker get away with some fuzziness in his explanation.

He would go back and just stick to that point to get it absolutely clear before allowing the discussion to go on to further development. EHRENFEST was a most useful person to have in the audience whenever there was a lecture or a colloquium or anything like that. Not only would he jump up and insist on further clarification when the speaker had not expressed himself sufficiently clearly, he had other most valuable qualities.

Suppose the speaker was going into very great detail, elaborating some point, and the audience was getting a bit bored. Well, then ERHENFEST would get up, interrupt the speaker, but do it in a very polite and diplomatic way, so that the speaker would not be offended. He would say that « I'm quite certain this work is very important, but we would like to read about the details of this work later and we don't want to hear all the details now. Would the speaker please go on to discuss his conclusions and results? ». The speaker was pacified by this very diplomatic interruption, and would go on to the results, and everyone in the room would be grateful to EHRENFEST.

Then there were other occasions when the speaker was perhaps assuming too much, assuming something that many people in the audience did not know about. EHRENFEST would again interrupt and ask for further explanation of this matter. Again, many people would be grateful to EHRENFEST for doing this. Probably many people in the room also wanted further explanation of this point, but did not want to expose their ignorance by asking for it.

EHRENFEST would say on these occasions that he did not mind being laughed at. Occasionally he was laughed at when he had asked for an explanation of some point that needed only a quite elementary explanation. But EHRENFEST was not in the least perturbed by being laughed at. I never knew anyone who was so unperturbed at being laughed at. He would say « It doesn't matter in the least if I'm laughed at. The only important thing is that I should get to understand this point. »

If EHRENFEST was present in any audience, then of course one could be assured that the lecture would be a good one, that we should not waste time on unnecessary things, and the speaker would be confined to telling the audience just what they really wanted to hear.

There is another thing I might say about BOHR. During the course of the discussions that I had with him, he told me of the disagreement which he had much earlier with THOMSON. He said that he was a great admirer of THOMSON, and the last thing he wanted to do was to criticize THOMSON or upset him in any way. However, BOHR did want further explanation of some of the features of Thomson's atomic models, and he did not then know English very well and was not able to express himself in such a polite form as he would like to have done, and THOMSON did take his questions badly. THOMSON assumed that he was being criticized and got angry.

This distressed BOHR very much, and I think this incident made a lasting impression on him. It continued to distress him all his life, I should think.

He was very careful in later life that this sort of thing should not happen again. Whenever he was questioning any author about his work, he would keep on saying « This is not to criticize you, it is only to learn ». That became rather a stock phrase in Copenhagen: « This is not to criticize but only to learn ». It was often said in German « Nicht um zu kritisieren, nur um zu lernen ».

I believe HEISENBERG must have been affected by this phrase, because in the letters which he wrote to me, which I have been telling you about in my earlier talks, he is continually saying « I have no doubt that your results are right, but I would like to have further explanation of this point ». He was very careful not to say something which I might take as direct criticism and get offended with. It was really unnecessary for HEISENBERG to be so diplomatic. I was very honored in having these letters from HEISENBERG, and I would not have been offended if he had openly criticized me. But he was very careful not to do that.

Another person in Copenhagen who very much influenced the proceedings there was GAMOW. GAMOW was rather childlike, always wanting to play, and introducing a sort of light humor into all occasions. He was very fond of drawing pictures of Mickey Mouse. He added a lot to the entertainment that we had. He had some good ideas, applications which led to important developments in quantum theory, but I do not think he did any work which was very deep.

While I am on this question of giving my opinions of other physicists, I should also mention SCHRÖDINGER. I do not think I ever saw SCHRÖDINGER in Copenhagen. I do not remember any such occasion. But I met him frequently in later life, and, of all the physicists that I met, I think SCHRÖDINGER was the one that I felt to be most closely similar to myself. I found myself getting into agreement with SCHRÖDINGER more readily than with anyone else. I believe the reason for this is that SCHRÖDINGER and I both had a very strong appreciation of mathematical beauty, and this appreciation of mathematical beauty dominated all our work. It was a sort of act of faith with us that any equations which describe fundamental laws of Nature must have great mathematical beauty in them. It was like a religion with us. It was a very profitable religion to hold, and can be considered as the basis of much of our success.

Now, there is one point that you might wonder about when you read of Schrödinger's work. SCHRÖDINGER developed his quantum mechanics from de Broglie's wave equation. De Broglie's wave equation was relativistic, and SCHRÖDINGER of course was profoundly influenced by the beauty of relativity, and you may wonder why it is that his work, where he introduces the wave equation, is nonrelativistic. There is a contradiction there.

SCHRÖDINGER explained this matter to me many years later, I do not remember just when, around about 1940, when I had got to know him well. He said that he was working from the relativistic point of view inspired by DE BROGLIE, and he was led to a relativistic wave equation, which was a generalization of de Broglie's equation, bringing in the electromagnetic potentials. When he got this relativistic equation, his first concern was to apply it to the hydrogen atom to see what results it would give. The calculation gave results that were not in agreement with observation.

SCHRÖDINGER was extremely disappointed by that and thought that his wave equation was no good at all, and abandoned it. He gave it up for some months, then went back to it, and taking a second look at it, he noted that, if he used the equation with less accuracy in nonrelativistic approximation, the results that he got were in agreement with the experimental results, again with neglect of relativistic effects. So he was able to publish his wave equation in a nonrelativistic form, and in agreement with experiment.

Of course, the reason why Schrödinger's original equation, the relativistic one, did not agree with experiment was because it did not take into account the spin of the electron. The spin of the electron was a very new idea at the time, and possibly SCHRÖDINGER had never even heard of it. And SCHRÖDINGER then did not have the necessary boldness to publish an equation which definitely gave results in disagreement with observation.

The relativistic equation of SCHRÖDINGER was later on resurrected by KLEIN and GORDON and was published by them, and is known nowadays as the Klein-Gordon equation. It is considered as a good equation for use in a relativistic manner for a charged particle which does not have any spin. There was no such charged particle known at the time, and KLEIN and GORDON published this work purely as a mathematical development without any direct physical application. They had the boldness to publish an equation which was not connected with experimental results, while SCHRÖDINGER did not have that boldness.

Well, to return to my period in Copenhagen, in spite of meeting so many eminent physicists and having these discussions with them, I continued to work mainly on my own, following up my own ideas, and the problem that concerned me mainly was to get a general physical interpretation for quantum mechanics. One had the equations based on noncommuting things, q-numbers, as I called them, but one could use these equations to get results comparable with observation only by following various special rules. There was a great need for putting these rules together and getting some general method for physical interpretation. I worked on that for some time, and wrote a paper incorporating my results.

I would like to say that this work gave me more pleasure in carrying it through than any of the other papers which I have written on quantum mechanics either before or after. You may wonder why this is so. Many of my papers just were consequences of an idea that had come to me rather accidentally. The early work on Poisson brackets, for instance, and my later work on the relativistic wave equation were very definitely of this nature. They were consequences of an idea which had just come out of the blue. I could not very well say just how it had occurred to me. And I felt that work of this kind was a rather undeserved success. On the other hand, my work on the physical interpretation of quantum mechanics was a deserved success. There I was tackling a problem which was not too difficult to be solved by a direct approach. There were various stages in this problem which had to be disposed of one by one.

During the course of the work I was continually faced with the question of getting a suitable notation for writing down the equations that I was then dealing with. There was a frequent modification of notation. Everything proceeded step by step in a rather logical way, and led to a piece of work which laid the foundation of the general transformation theory of quantum mechanics, and also provided the essential features of a suitable notation.

With regard to this question of notation, I had to face the problem of writing down symbols which would contain an explicit reference to those factors which it was important to mention explicitly, and which left understood those quantities which it was safe to leave understood, to keep at the back of one's mind and not to write down explicitly. Well, this led to the notation which, with some slight modifications, has become the standard notation for use in quantum mechanics at the present time.

I had a very successful time in Copenhagen, because I developed this general physical interpretation which gave me so much pleasure, and I also started the quantum theory of radiation, and showed how it could be connected with the Bose-Einstein statistics, which follows from the use of wave functions which are symmetrical in the particles that they refer to.

While doing this work, I got one of those ideas out of the blue, namely to take the Schrödinger wave equation and apply a process of quantization to the wave function itself. The wave function was previously always considered as expressed by ordinary numbers, c-numbers. What would happen if you turned them into q-numbers, and assumed that they are noncommuting with their conjugates?

That led to a theory which was equivalent to the theory of radiation which I had been setting up, and provided an alternative way of introducing the subject. It gave rise to a process which has become known as second quantization.

It was towards the end of my stay in Copenhagen, probably in January 1927, that PAULI visited Copenhagen. I explained to him my work on the physical interpretation and transformation theory of quantum mechanics. We discussed how these ideas would apply to the spin of the electron. We were led to the introduction of the three σ-variables to describe the three components of spin. I believe I got these variables independently of PAULI, and possibly PAULI also got them independently of me.

Soon after PAULI left Copenhagen he wrote a paper (*Zeits. f. Physik*, **43**, p. 601) in which he incorporated the spin of the electron into the wave equation, in a nonrelativistic manner. SOMMERFELD in his book *Atombau und Spektrallinien II* refers to Pauli's paper (page 226) and says « The discovery of the

Pauli equation was an important step leading to the recognition of the true nature of the electron, *i.e.* the Dirac equation ».

This statement is not true, so far as I was concerned. I was not interested in bringing the spin of the electron into the wave equation, did not consider the question at all and did not make any use of Pauli's work. The reason for this is that my dominating interest was to get a relativistic theory agreeing with my general physical interpretation and transformation theory. I thought that this problem should first be solved in the simplest possible case, which was presumably the spinless particle, and only after that should one go on to consider how to bring in the spin. It was a great surprise to me when I later on discovered that the simplest possible case did involve a spin.

While on this subject of the relativistic wave equation I might mention that KRAMERS told me (some years after my equation had appeared) that he had independently obtained a second-order equation equivalent to my first-order equation. It is possible that KRAMERS worked from the Pauli equation. KRAMERS did not publish his work because it was superseded by mine.

In the beginning of February 1927, I moved from Copenhagen to Göttingen. I passed through Hamburg. At the time there was a meeting of the German Physical Society in Hamburg, which I attended for a few days. The work at this Physical Society meeting was largely concerned with a discussion of experimental results about spectra. Still, I did get to appreciate the way the German physicists worked. It seemed to me that they were very hard working, had long hours of lectures, and they did not seem to get tired from them. They had enormous energy.

I traveled from Hamburg to Göttingen in a fourth-class compartment of a train with a number of other physicists who had attended this Hamburg meeting and were returning to Göttingen. Among them was ROBERTSON, whom I got to know pretty well later in Princeton and who was concerned with cosmology, and I got my first interest in cosmological models of the universe from him.

I arrived in Göttingen and spent some months there. It was a rather more formal atmosphere than we had in Copenhagen. I increased my mathematical knowledge. I went to a course of lectures by WEYL on group theory. I met HEISENBERG and BORN on various occasions. I also met OPPENHEIMER and became a close friend of his, because we lived in the same pension and of course saw very much of each other.

I should say that, after leaving Cambridge, I continued my general mode of life, studying and calculating hard during the week and relaxing on Sundays and going for long walks in the country. In Copenhagen the walks were often not solitary walks. Sometimes I was accompanied by BOHR. He was also fond of walking and we had many enjoyable long walks together. Sometimes there was a whole party from the Institute all going together, providing a sort of excursion which refreshed us all.

I continued with these Sunday walks also in Göttingen. Sometimes I went with OPPENHEIMER. I remember in particular one long walk we had together on Easter Sunday in 1927, where we covered a great deal of ground.

I had received an invitation from EHRENFEST to visit his institute in Leyden. OPPENHEIMER was also invited, and we traveled together from Göttingen to Leyden in June of 1927. We spent some days with EHRENFEST at his institute, and we also visited KRAMERS in Utrecht for one day.

I went to the Solvay conference in Brussels in October of 1927, which was a great experience for me, meeting so many eminent physicists, among them EINSTEIN and LORENTZ. There are a few things that I remember clearly about this meeting. I had given a talk about my second-quantization method, and after this talk someone announced that there was a similar second-quantization method applicable with the Fermi statistics, a method which had been given by JORDAN and WIGNER.

At first I did not like this work of JORDAN and WIGNER, and I think I can attribute this dislike to my mind being essentially a geometrical one and not an algebraic one. In the case of the Bose statistics and the second quantization which was connected with it, one had a definite picture underlying the basic equations, namely the picture that the theory could be applied to an assembly of oscillators. There was no such picture available with the Fermi statistics, and I felt that that was a serious drawback. I did not appreciate therefore the importance of this other kind of second quantization.

Actually, of course, the importance lies in the very close connection that we have between these two kinds of second quantization, when looked at from a purely algebraic point of view. I will just put down the basic equations

$$\psi_n \psi_m - \psi_m \psi_n = 0\,, \qquad \bar{\psi}_n \psi_m - \psi_m \bar{\psi}_n = \delta_{nm}\,. \tag{14}$$

One gets equations like this when one quantizes the ordinary Schrödinger wave function, and these equations can be connected with those that describe harmonic oscillators, there being one oscillator for each state ψ_n.

With the other kind of second quantization, we have

$$\psi_n \psi_m + \psi_m \psi_n = 0\,, \qquad \bar{\psi}_n \psi_m + \psi_m \bar{\psi}_n = \delta_{nm}\,, \tag{15}$$

the same equations, except for a plus sign instead of a minus sign. There is this extremely close similarity between the two processes of second quantization when you look at them algebraically. If you try to get some pictures of the relations, then we have a picture in the Bose case and no picture in the Fermi case. But it is the algebraic connection which is important, and which has the effect that the second quantization for Fermi statistics is really just as important as the one for Bose statistics.

Another important question at this 1927 Solvay conference was the physical interpretation of quantum mechanics. Of course, there was a lot of discussion between those people who saw the need for indeterminacy in the results of quantum mechanics, and those who objected to any kind of indeterminacy appearing in fundamental natural processes. I gave my own point of view, which had been based on work on the general interpretation of quantum mechanics. This work led very directly to our being able to interpret the square of the modulus of the wave function as giving the probability of there being definite results for any observation applied to an atomic system. I should say that BORN had independently obtained the same result for use in connection with his scattering theory. With this probability coming into the interpretation, one had to accept that the results were not deterministic when one made an observation, and I expressed this situation by saying that, under these conditions, "Nature makes a choice". I think that that is perhaps still the best way of expressing the kind of indeterminacy which we have in atomic theory. There are occasions when we just have to admit that Nature makes a choice, and we cannot predict what this choice will be.

There is one incident that I remember about this Solvay conference. During the period before the lecture started on one occasion, BOHR came up to me and asked me « What are you working on now? ».

I said « I'm 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 interrupted our conversation, and I had to leave the question rather in the air.

This was a problem that was very much dominating me at the time: how could one get a satisfactory relativistic theory of the electron? I had the general physical interpretation for quantum mechanics which I felt sure was right, but it required one to work with a wave equation which was linear in the operator $\mathrm{d}/\mathrm{d}t$, giving $\mathrm{d}\psi/\mathrm{d}t$ equal to some definite function of ψ. Now, the Klein-Gordon equation involves $\mathrm{d}\psi^2/\mathrm{d}t^2$. This would not fit in with my general interpretation. If one tried to fit it in, one was led to a probability which could be sometimes negative, and that of course is physically nonsense.

KLEIN and GORDON had tried to get over this difficulty by saying that the quantity which I considered ought to be the probability was really the charge density. The equation should be applied to an assembly of particles, and an expression was proposed which gave the charge density, and of course the charge density could be either positive or negative, if one allowed the possibility of particles with negative charge as well as positive charge.

However, this was not good enough for me. It was not much use having a theory of several particles if one did not have first of all a theory of one particle. It could not be considered as a logical theory if it could not be applied to one particle, and so long as one was considering just one particle, it was necessary to be able to find probabilities for this particle, and the probabilities had to be positive, and that required that the wave equation should involve only $d\psi/dt$.

This was quite a problem for some months, and the solution came rather, I would say, out of the blue, one of my undeserved successes. It came from playing about with mathematics. I was playing about with the three components σ_1, σ_2, σ_3, which I had used to describe the spin of an electron, and I noticed that if you formed the expression $\sigma_1 p_1 + \sigma_2 p_2 + \sigma_3 p_3$ and squared it, p_1, p_2 and p_3 being the three components of momentum, you got just $p_1^2 + p_2^2 + p_3^2$, the square of the momentum. This was a very pretty mathematical result. I was quite excited over it. It seemed that it must be of importance. But it did not immediately answer the question of how one could get a satisfactory relativistic equation for the electron.

It provided effectively a method of taking the square root of the sum of three squares and getting it in a linear form. Now, if we are to have a relativistic theory of a particle, we would need to have the square root of the sum of four squares, and it was just impossible to use this method to get a square root for the sum of four squares. So it seemed that it was an interesting bit of mathematics, but just was not good enough to provide an answer to the problem.

It took me quite a while, studying over this dilemma, before I suddenly realized that there was no need to stick to quantities σ, which can be represented by matrices with just two rows and columns. Why not go to four rows and columns? Mathematically there was no objection to it at all. Replacing the σ-matrices by four-row-and-column matrices, one could easily take the square root of the sum of four squares, or even five squares if one wanted to.

Well, that led to a new wave equation for the electron, a wave equation which is linear in the four components of the relativistic four-vector of momentum and energy. It provided us with this wave equation

$$(p_0 - \alpha_1 p_1 - \alpha_2 p_2 - \alpha_3 p_3 - \alpha_4 mc)\psi = 0 \tag{16}$$

(I expect you are all familiar with it), a wave equation in which we have a wave function with four components, corresponding to the four rows and columns of the matrices, and every one of those components just satisfies the de Broglie equation.

Well, that was just an equation for one particle in the absence of any field of force. To get something interesting one had to bring in an electromagnetic field. There was the general problem of how to bring in an electromagnetic field when we had a theory for the particle in the absence of any field. I had met that problem some time previously. I think it was first in my work on the Compton effect. It was necessary there to bring the electromagnetic potentials into the description of the motion of the particle, and still to keep the equations in the Hamiltonian form.

When I first met this problem, I proceeded to solve it without bothering to look up the literature to see whether it had been solved previously. It was a problem of classical mechanics, to put the equations of motion of a charged particle, expressed relativistically, into the Hamiltonian form. I expect it was solved probably some time near the beginning of this century, but I never bothered to look up who first did it. That is a question for the historians of science. I proceeded to work it out for myself, which did not involve much difficulty, and I think was really simpler than looking up references.

Well, I used this same method again for the new wave equation, linear in the four p^2's. It just involved replacing each of the four p's by $p + (e/c)A$, A being the corresponding electromagnetic potential.

Then I noticed that this was really a very successful equation. It led to the electron automatically having a spin of half a quantum, which is just what experiments required. It led also to the electron having a magnetic moment, and I applied it to the hydrogen atom in the first approximation, and got results in agreement with observation.

I wrote up this work and published it, keeping to the first approximation in my treatment of the hydrogen atom. You may wonder 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.

The originator of a new idea is always rather scared that some development may happen which will kill it, while an independent person can proceed without this fear, and can venture more boldly into new domains.

There was then a wave equation for the electron which was satisfactory in many respects, but also had a serious failing, namely we had the matrices describing an internal motion, containing four rows and columns, whereas we need only matrices with two rows and columns to describe the two states of spin of the electron as it is observed. The result is that this equation gives you twice as many states as you want for describing the experimental situation. If you look into it more closely, you soon see that half of the states refer to negative energies for the electron, so you can say, well, just exclude these negative-energy states, which are unobservable. Let us confine our attention to the positive-energy states, and then we just have a theory giving us things which can be observed.

However, it is not so simple just to do that, because of the transitions which may occur between positive-energy states and negative-energy states. We have the negative-energy states occurring also in classical theory, but with the classical theory they can be ignored, because we do not then have any transitions from positive-energy states to negative-energy states. In the quantum theory these transitions cannot be ignored.

They occur rather seldom, if one is dealing with radiation which does not involve very high frequencies, and so one can get an approximate theory just by ignoring them. That is what we had to do for a time.

SCHRÖDINGER proposed a modification in which the transitions between positive- and negative-energy states were excluded, but he had to bring in a change in the wave equation which spoiled its relativistic character and spoiled its beauty, so it was not a satisfactory explanation.

The problem of the negative-energy states puzzled me for quite a while. The main method of attack to begin with was to try to find some way of avoiding the transitions to the negative-energy states, but then I approached the question from a different point of view. I was reconciled to the fact that the negative-energy states could not be excluded from the mathematical theory, and so I thought, let us try and find a physical explanation for them.

And that was not so difficult, when one remembered that electrons satisfy the Fermi statistics which does not allow more than one electron to be in any state. I was led to a picture in which we have a world with all the negative-energy states occupied, so that an electron in a positive-energy state cannot make a transition to a negative-energy state. Then, of course, we have to consider the possibility that some of the negative-energy states are not occupied; there are holes and these holes will appear as particles also but having a positive energy.

It was not really so hard to get this idea, once one had the proper understanding of what one needed, because there was a very close analogy provided by the chemical theory of valency. In this theory we have the inert gases, where all the electrons fill up closed shells. Then we get the alkali elements, where there are one or possibly two electrons outside the closed shells, and these are the chemically active electrons, and they are also the most active in producing spectra. Then we have to consider the possibility of there being a hole in a closed shell, which gives the halogen gases. The relationship between the holes and the electrons, which one gets from this chemical theory of atoms, could be taken over directly to the positive- and negative-energy states, so it did not need any great stretch of the imagination to set up this theory where we have nearly all the negative-energy states occupied.

Of course, as soon as I got this idea, it seemed to me that the negative-energy states would have to correspond to particles having a positive charge instead of the negative charge of the electron, and also having the same mass as the electron. Now, that was a serious difficulty. At that time, we had the electrons carrying negative charge, and we had the protons carrying positive charge, and everyone felt pretty sure that the electrons and the protons were the only elementary particles in Nature. It is true that RUTHERFORD had sometimes considered the possibility of there being a third particle, the neutron. He proposed 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.

Well, what was I to do with these holes? The best I could think of was that maybe the mass was not the same as the mass of the electron. After all, my primitive theory did ignore the Coulomb forces between the electrons. I did not know how to bring those into the picture, and it could be that in some obscure way these Coulomb forces would give rise to a difference in the masses.

Of course, it is very hard to understand how this difference could be so big. We wanted the mass of the proton to be nearly 2000 times the mass of the electron, an enormous difference, and it was very hard to understand how it could be connected with just a sort of perturbation effect coming from Coulomb forces between the electrons.

However, I did not want to abandon my theory altogether, and so I put it forward as a theory of electrons and protons. Of course I was very soon attacked on this question of the holes having different masses from the original electrons. I think the most definite attack came from WEYL, who pointed out that mathematically the holes would have to have the same mass as the electrons, and that came to be the accepted view.

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 some thing 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 the 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. The remark was that it had often been observed that there were tracks leading into the source. 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. What they thought were electrons going into the source were really positively charged particles with the same mass as the electron coming out from the source.

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.

Well, I expect you all know the history after that. The positively charged particle with the same mass as the electron was discovered a few years later. It was actually first observed by BLACKETT. The first example was obtained by BLACKETT, but he was rather cautious and did not want to publish his result without confirmation. ANDERSON, getting a similar result, was bolder and published it and scooped the credit for being the first to observe a positron.

Well, that marked the beginning of the discovery of a whole lot of new particles. The neutron was discovered, then various kinds of mesons and a whole lot of new particles, and people are still going on discovering more.

It is very strange how the whole climate of opinion with regard to new particles has changed so drastically, from the late 1920's, when it was considered practically self-evident that there could not be any particles other than electrons and protons. People have gone over to the diametrically opposite point of view, and are willing to postulate a new particle on the flimsiest experimental or theoretical evidence. The number of particles which are considered as rather fundamental has gone up to several hundred now, instead of just two.

Well, this takes me to the end of what I counted as the exciting era. It was an era in which there was a rapid development of theoretical ideas on the foundations of our knowledge of atoms. Since then, physics, of course, has continued to develop strongly, but on rather different lines. Since then the experimental people have had it pretty much all their own way. They make experiments and report on the results of their observations. The theorists are not in a strong enough position to contradict those observations. They have to accept what the experimentalists say and do their best to construct theories to fit in with the observations, and most of their work consists in setting up theories to account for this host of new particles.