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## Conference Manuscript

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CONFERENCE ON THE FOUNDATIONS OF QUANTUM MECHANICS

Xavier University  
Cincinnati 7, Ohio  
October 1-5, 1962

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Roster of Limited-Attendance Portion of Conference

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Xavier University

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Cambridge University (England)

Professor Nathan Rosen  
Technion, Haifa, Israel

Professor Wendell H. Furry  
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CONFERENCE ON THE FOUNDATIONS OF QUANTUM MECHANICS  
October 1-5, 1962

Public Relations Policy

In order that each participant may feel free to express himself spontaneously in the spirit of the limited attendance portion of this conference, the chairman has adopted the following policy regarding references. It is understood that each person present, before referring in publication to remarks made by a participant during these sessions is expected to check such material with the participant or participants concerned. Reports of general conclusions are to be checked with the chairman. This policy applies as well to the published report of the proceedings, which is to be edited by the chairman.

Dr. Podolsky: In the heat of an argument I can make a statement, and I probably will, which any freshman with a pencil and paper and five minutes can prove to be nonsense. Perhaps a few minutes later I might regret having made the statement. Now, we don't want such statements to get out. The principle reason for this is to make sure the participants won't stop to worry about whether or not what they're saying is really so, or whether it is nonsense. We want the participants to feel free to express themselves spontaneously, and afterwards, in more sober discussions, withdraw these statements without things getting out in the newspapers.

MON-A.M.

Conference - October 1-5, 1962

Part of Monday Morning Session after introductions:

Podolsky: You probably have seen in the newspapers some reference to quantum-mechanical action at a distance. The idea of that occurred to me and probably has occurred to many other people. I would like sometime to have a discussion on this subject. Aharonov and Bohm suggested an experiment which was, as you know, performed by Mölllenstedt, and which can be interpreted in two ways. One is that the vector potential has physical significance. That was the way in which they presented it. But if you consider the fact that the observed phase shift in the wave function actually turns out to be proportional to the flux, which is gauge invariant, one might interpret the result of the experiment in a different way, namely, that the reason we have the shift is quantum-mechanical action at a distance. The fact that flux through a loop formed by the two electron beams actually affects the wave function of these two electron beams and produces the observed phase shift at the place where the flux is not, will be an example of an action at a distance. Now if this experiment was alone it would not convince me that there is such an action at a distance, because then it would be simplest to say that a vector potential has a direct effect. But there is this old question, sometimes referred to as the Einstein-Podolsky-Rosen paradox, where also we have a kind of action at a distance. You are all probably familiar with that. I have discussed this question of quantum-mechanical action at a distance with several people and one of them said "Well, of course, in all the quantum-mechanical effects there is action at a distance." For example: when you take the so-called reduction of a wave packet and the wave function suddenly changes from one thing to something quite different, there is again a kind of action at a distance, I would like expressions of opinion about this question of action at a distance.

Aharonov: It is clear that this action at a distance is never observable in the usual sense. The usual statement about observation involves probabilistic statements, and it is clearly the case that in all the other examples that you mention about action at a distance there will be a change in a particular case, but not a probabilistic change in an ensemble of cases.

Podolsky: But what about the Aharonov-Bohm experiment?

Aharonov: Well, in that case there is a change in probabilities too, of course, because that is what we observe in the experiment. But one can in that particular case discuss a local action which involves an interaction with potentials. You wanted to strengthen the idea of action at a distance by discussing other examples, but I am not aware of any other example of action at a distance that will involve change in probabilities and not only in information. In one particular case, namely, in the case of ensembles, there is no change in probabilities after so-called action at a distance.

Podolsky: What about the experiment of Wu and Shalnov?

Aharonov: Well, this case is exactly similar to any other example of the Einstein-Podolsky-Rosen paradox. That is to say: in any individual case, when you make an experiment on one of the particles that are involved in the setup of the paradox, you learn something about the state of the other particle. In that sense, you have made a change in the wave function of the total system, and also of the particle that is far away. So you can predict something about the probabilities of an experiment that will be made on the other particle. But if you consider an ensemble of similar experiments, you ask whether your measurements of the first particle in each pair of particles that appear in this ensemble will cause changes in probability of the second particle in the ensemble. This means

that the observer that makes experiments on the second particle (all the members that are called second particle in the ensemble of Einstein-Podolsky-Rosen pairs) will never be able to discover that the experiment was done on the first particle. On the other hand, this action at a distance cannot send any information, or any change of probabilities to the faraway members. In other words, there is a transformation of knowledge but not of probabilities from one side to the other.

Podolsky: That makes the wave function purely a subjective entity. That this isn't a subjective thing is shown by the fact that in a measurement when there is a transformation of knowledge, the wave function changes completely, while no other change occurs.

(A brief discussion here continues about the question whether the change is complete or if only a partial change occurs in the wave function).

Wigner: Well, it is true that under certain conditions the change in the wave function is complete. Anyway, it does not matter whether it is a

complete change or a partial change.

Rosen: Do you accept, then, <sup>the idea</sup> that the change in the wave function is <sup>due to</sup> through the process of measurement, rather than <sup>to</sup> by a change in our

knowledge?

Wigner: No.

Furry: By introduction of means of measurement, one could introduce a statistical situation which, from the point of view of the coordinates of the particle to be measured, is a mixed state. This mixed state is just the same thing as the classical Gibbs ensemble. I did not say it

well, perhaps, but I will try to say it better this afternoon. When one makes the measurement one chooses from among those <sup>possibilities</sup> Now, this has the

difficulty, as in the case of Einstein-Podolsky-Rosen, that the ensemble

can be interpreted from many points of view. When I said the Gibbs ensemble, it implies a realistic interpretation. This realistic interpretation is valid only in the usual laboratory set-up, when one is interested in measuring something that has non-uniform distribution over the

various possible results of the measurement. It is typical of the Einstein-Podolsky-Rosen distribution that <sup>it</sup> is uniform, therefore it is

possible to interpret it in many different ways. This is, in a sense, an artificial situation, but the theory has to deal also with artificial situations. Well, it just seems to me that the problem is perhaps the wholeness of the quantum state, and the quantum state may have this character as a whole extending over a very large distance. In a certain sense it is like the Wu experiment, and this may conflict with old-fashioned ideas. But probably we just have to accept these new properties. London suggested that this property of wholeness may extend over a very large distance in a many bodied system.

Wigner: I do not think though, if I may put my two-bits in, that I fully agree with what Professor Furry says: that the consequences of quantum theory are such that there is no way out of what he has mentioned. There are suspicious elements, though, since under some conditions it is very, very difficult to confirm the consequences of quantum theory. This point has been emphasized, the first time in my knowledge, in the book of Bohm. The fact that it is so difficult to verify it makes one suspicious that perhaps all that is quantum theory cannot be applied to these very difficult situations. I would like to hear about what the people that probably have thought very deeply and greatly about it think.

Aharonov: I wonder if you are familiar with an article that was written on this subject by Professor Bohm and myself, in which we analyze an



experiment that was done by Yu. (He pronounced it like "you".)

Wigner, wholly astonished asks: By who?

Aharonov: By Wu. (referring to Wu and Shaknov) The experiment involves a case that is similar to the paradox in which two photons emerge from annihilation of electron and positron, and there is correlation between the photons polarization of a type similar to the correlation discussed in the products of Einstein, Podolsky, and Rosen. The correlation is complete in the sense that whenever you measure the polarization of one of the photons in an arbitrary direction, you find the polarization of the other photon in the same direction well defined. This means that by different measurements on the first photon, one could put the second photon in eigen states corresponding to a non-commuting operator, namely, polarization in different directions. The purpose of our article was to show that no semi-classical description for this situation would suffice. If one assumes that the photons in each particular case are in a well-defined state of polarization, and one just gets the correlations in different directions as an average over different cases (that is, in different cases the polarization is well defined in different directions) one finds that the correlation between the results of measurement on one side and those on the other side are not enough. The only possibility to account for the experimental results would be to assume that really the complete correlations of a quantum type, described by the products of E.P.R., are necessary. But one should add that this experiment is not conclusive as far as the question of signal velocities is considered, because the experiment was not done quickly enough to insure that there was no possibility of a light signal going from one photon to another photon. The only clear way to insure that no hidden "interaction"

between the two photons can account for this kind of correlation would be to send the photons so far away from each other that two measurements could be made on each one of them outside the light cone connecting the measurements. Then there would be no possibility of sending information, from one photon to the other, about the type of experiment that is done. In that case, we could insure that as long as the hidden "interaction" behaves according to relativistic laws there is no other possibility to account for this type of correlation except by taking the quantum theory fully into account.

Wigner: I agree to this kind of a discussion completely, though I did not have this experiment in mind. But I do think there is a contradiction. I agree that in all the actual cases one discusses about quantum theory, namely, the case of two electrons that one might like to quote, or the cases of two light quanta that you have discussed, quantum theory is probably valid and one may also discuss experimental verification in a way that you have pointed out. In this connection, when the information cannot possibly be transmitted, it is certainly a fact of life. But if we go to systems which are complicated, where, for example, even a photographic plate helps in a case similar to the Einstein-Podolsky-Rosen paradox; there the question whether quantum mechanics applies is not certainly clear. It is not clear because it is virtually impossible to verify it due to the experimental difficulties, and because of the complexity of the system. It may still be proved where there is a basis for doubt. Evidently it should be discussed, if at all, after we all held very clear discussions of the program, because clear discussion will add to it. It also creates a common ground for the discussion during which we could approach things better instead of going again and again over preliminaries. Maybe it will be a good thing to discuss it. It

certainly would be one thing about which I would like to hear the views of some of those present. Would the program allow us to take this question up even though it is somewhat speculative?

Podolsky: I think it will be possible to arrange it.

Furry: I think we should devote a considerable amount of time to this question.

Podolsky: Are there any other opinions related to this?

Rosen: I would like to hear some other opinions before I ~~can~~ present

my ideas.

Aharonov: I would like to add a remark that will help to see the fact that no observable information can be sent using this type of correlation in a more picturesque way. In order to do this, let me first emphasize that there is no way to distinguish, in quantum theory, between a box full of particles all spread over a passible eigenstate of position and another box with particles all in eigenstates of momentum spread over all the possible eigenvalues. Put more exactly, we take one box where all the particles are in eigenstates of position with equal probability for each eigenvalue; then in the other box all the particles are in eigenstates of momentum. The number of particles in each eigenvalue of momentum is equal to the number of particles in any other eigenvalue of momentum. There will certainly be a difference in the mathematical way that one should describe the two boxes, since the description of particles in one box will involve delta functions of position and pure plane waves in momentum, while in the other case one will have plane waves in position and delta functions in momentum space. It is an interesting observation that if there was any observable way to distinguish between these two boxes corresponding to the difference in the mathematical way that one describes them, then quantum theory and

special relativity would not be able to be brought together in a consistent way. To see this better, come back to the example of the Einstein-Podolsky-Rosen case in which we have a pair of particles having their relative position and their total momentum both well defined. This means that by measuring the position of one of these particles we can put the other particle in an eigenstate of position, or instead, by measuring the momentum of the first particle we could put the other particle in an eigenstate of momentum. Now let's suppose that instead of having just one pair we have an ensemble of such pairs, all of them in the same state of relative position and total momentum but the two members of each pair are very far away from each other. Let's say all of the first members of each pair are on the earth and all of the second members of each pair are on the moon. Now let's suppose that we make a measurement of position on each one of the first members, which are on the earth. In this way we put all of the second members, which are on the moon, in eigenstates of position. In other words, we have prepared all the particles on the moon in an ensemble of eigenstates of position spread over all the possibilities, because the position was not well defined to begin with. But all of them are in eigenstates of position. We could, on the other hand, have chosen to make a measurement of momentum on the first members on the earth, and in this way put all the particles on the moon in eigenstates of momentum. So in other words, we could prepare either a box of particles all in eigenstates of position, or a box of particles all in eigenstates of momentum, on the moon. And this preparation would have gone on with arbitrary velocity, namely, instantaneously after the measurement was over on the earth all the particles on the moon would either be in eigenstates of momentum or in eigenstates of position, which, as I mentioned before,

are distinguished by the mathematics. If there was any way to distinguish between these two cases by observation, it would mean that we could instantaneously send information from the earth to the moon by deciding either to make a measurement of position or to make a measurement of momentum. So in this way we see more clearly why we say that this type of correlation causes a kind of action at a distance, quantum mechanical action at a distance. It affects only the mathematics and not the physically observed state, because there is no way to distinguish observationally between one kind of measurement and another kind of measurement. Oh, Wigner just mentioned that old-fashioned people remember that in his first article on the problem of mixture he discussed a similar example. I was not aware of the fact that I re-discovered this instead of invented it.

Wigner: Well anyhow it is an important point, but it's a little bit distinct from the problem we would like to discuss now. Would anybody from the audience like to defend the idea that quantum theory really describes correctly the question of the wave-function collapse? One finds again and again articles in which it is stated that the problem of the reduction of the wave packet is unnecessary, and that this reduction is an old-fashioned idea. If we could hear a little more on that it would be very useful. But we should, of course, hear about it from somebody who is convinced about it.

Furry: Well, if there is somebody here who believes in this, he should know exactly what you mean. I take it that what you mean is that there is no need to go outside the present organized formalism in order to understand the reduction of the wave function.

Wigner: Do you believe that?

Furry: I don't believe in that. Quantum theory certainly describes

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changes that are different from the changes that are necessary in measurement theory. Now if one could find a way to describe measurement theory consistently also, so that there will not be this difficulty in a description of the ordinary kinds of dependent states and measurement cases, it would be very interesting. But I know that quantum theory is not that theory.

Aharonov: I would like to describe one kind of such an attempt which says that the universe is taken to be infinite and therefore includes infinite degrees of freedom. All of them in some sense take part in every measurement process and therefore it is never possible to discuss all of them in a closed or complete theory which is finite. Therefore one always has to discuss an open system in which one has a mathematics that is different from the mathematics of the closed system, namely, that time dependent evolution is not given just by canonical formalism. One has to discuss a more general case of density matrices that have non-canonical equations of motion. The reason for these non-canonical kinds of time displacing operators comes from the fact that you have to integrate over all of those degrees of freedom that you have to neglect, namely, the infinite number of them that you don't consider in your equations. In that case you find that the equations of motion, for the rest of the degrees of freedom that you care to discuss, are non-canonical, and density matrices can introduce either a spread over its diagonal or the opposite case that corresponds to a collapse to one eigenstate of the measured quantity. So in that sense people some-times say that one can get a consistent description of measurement theory, if one agrees on what it means to say that it is impossible to discuss any process by a closed system because there is always an infinite number of degrees of freedom involved, and therefore one has

always to discuss an open system. I would like to add that this is not my own point of view. I am just trying to give an argument that is quite common among people who try to say there is no difficulty in measurement, that it is only a question of mathematical difficulties in handling infinite systems. Somehow I feel that one really avoids the main problem because it seems to me disturbing that one needs to fall back into the difficulties of infinities that one gets into in order to solve a problem that might also be formulated for finite number of degrees of freedom for non-relativistic systems that haven't necessarily infinite number of degrees of freedom.

Wigner: Well, I would like it very much if you could show that when one has an open system one could really avoid the problem of the reduction of the wave packet.

Furry: I would very much like Professor Aharonov to discuss this problem.

Aharonov: Well, I'm not prepared to do it now, since I have to try to organize my thoughts about this problem. I would like just to mention again that when one discusses an open system, one says that one has to integrate over degrees of freedom that in principle cannot be measured. Then one can get results that are different from the usual cases which are discussed, namely, cases of a closed system. These results might look arbitrary in a sense, but the mathematics permits them, and therefore they should be discussed carefully. If I think about it a little bit more, I hope I will be able to present it in a more systematic way.

Wigner: I think if one looked more closely into the mathematics one may find that this leads to a contradiction, but I may be wrong.

Aharonov: Maybe we don't understand exactly the point of view of each other.

Wigner: That's very likely. I feel that it might be useful to continue these discussions after you have thought about it, because this will provide us with some common notation and starting point, and so on.

Podolsky: Dr. Aharonov, how soon do you think you could discuss this question more fully?

Aharonov: Well, maybe next year.

Wigner: When everyone put down the notion of reduction of wave function

one gets letters pointing out that it looks unnecessary. For example, Margenau says it is an unnecessary assumption.

(Aharonov mentions again that he would like to say that he is not, he

believes, exactly of the opposite view, namely, that the reduction is a

necessary assumption.

Wigner points out: therefore, he is not a good candidate to have votes from the other point of view).

(A short discussion followed about the possibility of inviting Everett to discuss his point of view about the reduction of the wave packet.

Podolsky asks Rosen if there is something he could say about Everett's

ideas. He explains that according to Everett, it ~~isn't~~ <sup>is not</sup> necessary to ~~worry about~~ <sup>worry about</sup> have the problem of the reduction of the wave packet, because all the different possibilities after measurement are on equal footing.

<sup>an</sup> The various possible results of a measurement correspond to a ~~Some~~ kind of branching ~~happens after a measurement~~ so that if you get

one result it means that you are just on one of the branches.

But since all of the other branches exist on the same footing, one

describes all of the possible measurements as one huge tree. Each time after a given result is found, one simply ~~belongs to~~ <sup>goes along</sup> one of the branches



and from this branch one continues into further branching by making another measurement, and so on. We all seem to feel that the measurement does something decisive. For example.

when we cast a die and get a given result, we have in some way *singled out* forced this result out of all the possible results, ~~at their~~ ~~expense.~~

Podolsky: Oh yes, I remember now what it is about - it's a picture about parallel times, parallel universes, and each time one gets a given result he chooses which one of the universes he belongs to, but the other universes continue to exist.

Aharonov: Perhaps Professor Rosen will be willing to introduce the idea a little bit more fully with perhaps a little bit more on the mathematical side.

Rosen: I just have some recollection of the paper. It's not a question of mathematics, it seems to me, but rather a question of interpretation. The mathematics involved is very simple you expand a wave function as a linear combination of eigenfunctions of the observed quantity. In other words, if you have two systems interacting, one of them being the measured system and the other the measurer, ~~then~~ you can use what

Professor Furry will talk more about in the afternoon, namely, correlation between the measurer and the measured system. Then

you ~~ve got~~ *get* an expansion involving eigenfunctions of the system,

multiplied by eigenfunctions of the measuring instrument. The usual belief is that when the measurement is over, one of these

terms is singled out and the others are thrown away. That ~~is~~ *is* what is referred to as the reduction of the wave packet. The other point of view <sup>*that of Everett,*</sup> is to keep all the terms ~~in spite of the fact that all one gets out of the measurement is experience.~~

Aharonov: There seems to be a problem here. It raises the questions Is time reversible? If you look on the process of branching you see that it has a definitely preferred direction of time. You never experience any collection of past branching connected together with one observer in the present. So the observer described by this method is always going in one direction of time, namely, more and more branching toward the future and not vice-versa. In other words, it seems that the idea of the unique direction of time is basic for this theory, and one should therefore explain why a reversible equation for a closed system somehow irreversibly measures in this idea of branching.

Professor Podolsky suggested we should at least very briefly in the conference discuss the general question of what basic problems in physics have not been solved yet.

Professor Wigner remarks that he is not aware of any basic problem that has been solved yet, but then he corrects himself

and says "Well, perhaps one basic problem was solved and that is the question of the behavior of inanimate matter in the question of practical applications of physics, which at least in principle has been solved."

Professor Podolsky answers that "the question what is practical or not seems to be entirely a matter of time. For example, there was this case of a quite well known physicist in England who was not drafted during World War II. Therefore they decided that they should let him work only on problems that have no practical significance at all, and put him to work on the question of atomic energy. Now this goes to show, of course, that the question of what's practical or not is not necessarily settled at any given period." Then he proceeds to discuss one of the questions that he feels is of basic importance and has not yet been solved in physics? the question of why all particles in the world have the same charge, plus or minus  $e$  or zero times  $e$ , while they might have different masses?

Professor Wigner then explains that what he meant to say was "the problems of atomic scale and so on, are solved as far as their practical application is concerned, at least in principle. But certainly they are not solved as far as understanding why these laws apply and not other laws. This is something not clear, and as is probably always the case, we

understand how to apply the laws but we never understand the reason for this kind of law."

Professor Podolsky: It is well known that we work with manifestly inconsistent theories in which we seem to get perfectly good experimental results, but which involve procedures like subtracting infinities from infinities. The question is: Are we going to be satisfied with such a theory?

Wigner answers: "No!" (There is laughter from everybody)

Podolsky: Well, therefore there are important questions.

Wigner: Oh yes, excuse me. I did not want to say there are not any important questions left.

Dirac: Well, I think that the value of  $e^2$  over  $hc$  is an important question.

Podolsky: Yes, that is something I feel is of very great importance. We have quantum mechanics and we have relativity theory. Relativity theory is based on the concept of an event. Events cannot be experimentally determined. We can't measure position with arbitrary accuracy. It is not only that we are limited in quantum theory, but also when we start using light of very short wave length, instead of having a position measurement we get a shower of particles, and the old concept of position is lost. So our concept of event in quantum theory does not correspond to relativity theory.

(Somebody from the audience asks whether Professor Podolsky is aware of an article by Wigner in which he points out limitations in measurements of position and time so that all concept of space and time in quantum theory may have quantum limitations and uncertainties. He answers, "Yes, that's right.")

Podolsky continues: We have a fine structure constant which connects  $e$ ,  $h$ , and  $c$ ;  $h$  represents quantum theory,  $c$  represents relativity, and from those two concepts we expect to derive  $e$ . We then will have a theoretical explanation for the fine structure constant.

(Somebody from the audience asks whether it will be possible to discuss quantization of an event in space-time, in such a way that he will get quantization of minimum lengths and minimum time.)

Podolsky: Yes, Heisenberg was trying to do something similar to that. He got quite interesting results but he gave it up later. Is there anything more somebody would like to say before I close this session?

Aharonov: May I just mention one more point which is related to the question of unifying special relativity and quantum theory? It is quite clear that quantum theory has states in which the momentum is well defined at a given instant of time. Not only that, the general operators of coherence, wave

functions that are in different regions of space, also are defined at a given instant of time. Now it is interesting to point out that all these operator-observables cannot be checked out, or measured, in arbitrarily short periods of time. This is because if one wants to get an interference of two of non-overlapping contributions to the wave function, one must wait at least a time period that is equal to the distance between these two wave packets divided by the velocity of light. All velocities are restricted to being smaller or equal to the velocity of light. One can see also the reflection of this limitation in the fact that if one wants to measure the momentum up to an uncertainty  $\Delta p$ , one must introduce an uncertainty of position which is equal to  $h/\Delta p$ , and let's call it  $\Delta x$ . Therefore the time that it will take to introduce such an uncertainty will be at least  $\Delta x/c$ . Otherwise we would send information faster than the velocity of light. Now it seems to me that such a limitation has no direct counterpart in the mathematical formalism, since we can write down any arbitrary states which include all the interference properties in regions that have time extension smaller than their spatial extension divided by c. Since there is no indication of such a limitation, it seems to me that one could perhaps formulate a more satisfactory

theory in which these limitations will appear directly in the formalism and not just indirectly in measurement discussions. I wonder whether anybody has some remarks about this problem.

Wigner: Well, I might talk about something related to it, although I am not sure it will have any substantial relevance. However, the point you make is a very important one. The Doctor to the left of Dr. Podolsky (referring to Dirac) once tried to make a theory in which the initial conditions are given not in a space-like surface, well, on the light cone surface, converging to a point, and I never heard actually what happened to that.

Aharonov: Well, it probably ran away with light.

Dirac: Well, as far as I know, it is equivalent to the usual theory.

Conference on the Foundations of Quantum Mechanics

Monday Afternoon - October 1, 1962

THE QUANTUM MECHANICAL DESCRIPTION OF STATES AND MEASUREMENTS

W. H. Furry

Professor Aharonov apologized because most of his talk has been published. I think practically all of mine has too, and any of you who have recently read von Neumann's book on the Foundations of Quantum Mechanics can just go to sleep.

My talk will be concerned essentially with what I suppose no one will object to my calling orthodox quantum theory. Some people object to that, but I simply mean standard quantum mechanics. I shall describe the regular formalism of the theory of measurement in quantum mechanics - thus, I hope, providing a background for various further discussions.

This will bring out, of course, several points along the way - among which is the quantum mechanical view of microscopic systems as having a certain quality of wholeness of their basic states. In fact, this quality comes out in a particularly pronounced way in the sort of example that was given by Einstein, Podolsky, and Rosen.

Now in setting up this formal theory, I shall use four main assumptions: First (a) there's the assumption of the discrete spectrum, and we all know what this is for. This is just to make things easy, and has no real bearing on the main



problems. One could get greater elegance of a certain sort, and some prestige, by generalizing the theory so as not to have this assumption. But by using this assumption we get more of the sort of elegance that makes it possible to give a lecture in 30 or 40 minutes, or 50, or 60, or 70.

Having given that relatively innocuous assumption, I'll hit you with the bad one. Assume that (b) every Hermitian operator is, in principle, observable. These two little letters, h.m., just show off that I recently reread von Neumann, and they stand for "hypermaximal". Occasionally there is a trick sort of operator, which, although Hermitian, cannot be regarded as observable, even in the mathematical theory. But this is the sort of thing which physicists would never, or rarely at least, think of using anyway. It's not hard to avoid. So essentially every Hermitian operator one is likely to think of using is here assumed to be observable. Now this is a mean assumption and it is possible to take strong objection to it, as Pauli did. Pauli objected very strongly to the idea of quantum mechanics based on this. But on the other hand, if you're going to make a formal mathematical theory and include the whole sweep of the subject, you need a strong assumption. Of course, if you just let me assume the theory, I won't need this assumption. But if one is to derive the theory, then one needs a strong assumption. This assumption has been used in

many treatments, in Dirac's as well as von Neumann's, and in many others.

The third assumption is a very famous early one of quantum theory: the possible values that may occur in measurements of an operator are its eigenvalues.

Now here's the assumption for which one has to ring a bit of a bell, because it's really something to accept and it's fundamental to the theory. If, say, A and B are such observables, then (d) any real multiple of A (so as to keep it Hermitian) is

also an observable, <sup>and</sup> ~~as is~~ A plus B <sup>is</sup> also an observable. This will be <sup>obviously</sup> ~~deviously~~ true, of course, if you could measure both

A and B at once, and clearly, the sum of the values is the value of the sum. It is a natural thing to take as the value of the sum.

But this is true even if A and B cannot be both measured at once. This is a basic assumption and is used all the time in quantum mechanics. For example, the kinetic energy, a function of the momentum, and the potential energy, in a simple Schrödinger case a function of the coordinate, are not simultaneously measurable. But we assume that the sum of the two, that is, the Hamiltonian function, is measurable. There is an addendum to this: (dd) the expectation value that we get for these observables, the expectation value of the multiple of

A, is that same multiple <sup>of</sup> ~~as~~ the expectation value of A;

$$\mathcal{E}(cA) = c\mathcal{E}(A) \quad (1)$$

and the expectation value – this is the strong point – the

expectation value of ~~the~~ A plus B is the sum of the expectation values of A and B.

$$\mathcal{E}(A+B) = \mathcal{E}(A) + \mathcal{E}(B) \quad (2)$$

That is, the expectation value is a linear function of the observables you're using.

This, of course, is familiar from the recipe that is usually used for the expectation value:  $\psi^*$ , times operator, times  $\psi$ , and then integrated.

$$\mathcal{E}(A) = \int \psi^* A \psi \, d\tau \quad (3)$$

There is a lot of talk about the most general statistical situation we can have in quantum mechanics, and that situation is not a situation described by a wave function. This is the thing that I want to remind most of you of, and perhaps inform a few people of, so it gets clearly in our minds early in this series of discussions.

So let's begin on the mathematics. I found, interestingly enough, that von Neumann doesn't begin back at quite so

abstract a level, and never have ~~I~~. But it's possible to do

it, so I will. We note that if we have any observable, such as A, we can tell what observable it is, we can characterize

it, by giving its matrix elements,  $A_{mn}$ . That is, if we have any

complete orthonormal set of functions,  $\psi_m$ , we can give the matrix elements  $A_{mn}$ , which are the products of  $A$  times  $\psi_n$ , and then the inner product of that with  $\psi_m$ , by which, of course, I mean simply, the integral of  $\psi_m^* A \psi_n$  integrated

(use all  $\psi$ 's)

over whatever coordinates there are.

$$A \text{ characterized by } A_{mn} = (\psi_m, A \psi_n) \quad (4)$$

orthonormal  $\psi_n$

So let's start using this way of characterizing what an observable is.

This, of course, is a rather artificial looking way,

because we have to pick some particular set of  $\psi_n$  to use.

But that soon drops out of the argument. Now from this assumption (dd), that the expectation value has meaning for sums, and is, in fact, a linear function of the observable, we see that the expectation value must be a linear function of the matrix elements, since it can be characterized by these matrix elements which themselves are linear functions of the observable.

$$E(A) = \sum_{m,n} R_{mm} A_{mn} = \text{Tr} RA \quad (5)$$

This is the most general form an expectation value can have.

It's the most general sort of statistical situation that these assumptions will allow. And, of course, it works out to be what von Neumann developed as the theory of a

statistical situation given by a density matrix or statistical matrix. We know at once that this expectation value must be a linear function of these A's. That is, it must just be a linear combination or sum over  $m$  and  $n$ , and there must be some coefficients,  $R$ . These coefficients will depend on what the state of the system is, on the way it was prepared, on our information about it, and they will also depend on  $m$  and  $n$  so I have put those on, let's say subscripts, on this coefficient. I put them on in this order,  $n m$ , and then here  $(R_{nm})$  is a two index quantity that we can think of as a matrix. We multiply these matrices together when we sum over  $m$ . Then when we sum over  $n$  we take the trace of the product. So this is the trace of the product  $RA$  and, as a trace, it has ceased to have any

dependence on what particular sort of  $\phi$ 's we took. So that  $\phi$

we have now rid ourselves, for the moment at least, of any dependence on a particular representation, on a particular

set of  $\psi$ 's.

Now one can quickly prove the rest algebraically, but I shall not go through the algebra. You can easily find by a little algebra the fact that  $R$  is Hermitian itself, that is, that  $R_{nm}^*$  is equal to  $R_{nm}$ . Then you use both assumptions (b) and (c) and you use the fact that the possible values are eigenvalues. Then you pick yourself some special operators that have only a few non-vanishing eigenvalues, say only one apiece and that one positive. Then you can easily convince

yourself that all the diagonal elements of  $R$  have to be positive. Because if they weren't, you could get a negative expectation value for something that could have only positive measured values, which is silly, because the expectation value is the average of the measured values. The average of positive quantities couldn't be negative. So one concludes from this that the diagonal elements of  $R$ , in any representation in fact, must be either positive or zero, and this just says that  $R$  is positive semi-definite.

Now take the particular representation where  $R$  is diagonal.

Take this particular representation, choose the  $\phi_m$

which are eigenfunctions of  $R$ .  $R$  has to have the form then in that representations

$$R_{mm} = W_m \delta_{mm} \quad W_m \geq 0 \quad (6)$$

$R_{mn}$  will have a factor  $\delta_{mn}$ , and then it will have some

positive coefficients,  $w_n$  or  $w_m$ , that is, with  $w_n$  greater than or equal to zero. Then we consider the expectation value of one — one is a very simple observable, whenever you measure you get the value one. If you turn me loose in a laboratory this would be the only observable I would know how to measure. This expectation value is one, it must be one.

$$\mathcal{E}(1) = 1 \quad (7)$$

That means that if you take the trace of this – just the sum of these diagonal elements – that the sum of the  $w_n$ 's must be equal to 1.

$$\sum_n w_m = 1 \quad (8)$$

The right member of that equation is 1. This particularly brings us right back to the discrete spectrum case. Well, I've used (a) all the time, really; the fact that this is a discrete spectrum case. I wrote all these sums. And you could not make a trace equal to one if you did not have that situation.

Now let's continue to use this special representation a moment longer, and consider again the expectation value of a particular: other observable A. That will be the trace of RA. And if we write it out, then we will have the expectation value of A. That will mean that we must take the sum over m and n, and we will have  $w_n d_{mn}$ . Now we multiply the R here by the A, and then, of course, we want to take the trace, so we will sum over n and sum over m also, so this is the trace. But, of course, this sum is very easy to do with a delta function. Let's do the sum over m. That means we replace m by n, and so we have  $w_n$  times  $A_{nn}$ .

$$\begin{aligned} E(A) &= \sum_{m,n} w_m \delta_{mnm} A_{mnm} = \sum_n w_n A_{nn} \quad (9) \\ &= \sum_n w_n (\phi_n, A \phi_n) \end{aligned}$$

And now you see in this formula, the diagonal element  $A$ , is just the formula for the expectation value of  $A$  when we know that the wave function is  $f_n$ . So we see that this expectation value is the sum over  $n$  of  $w_n$  times the expectation value of  $A$  for the state with a wave function  $f_n$ .

$$E(A) = \sum_n w_n [E(A)]_{\phi_n} \quad (10)$$

So here we have the most general statistical situation that quantum mechanics offers, and we see that it has what I'll call a realistic interpretation. In fact, this is the

formula with which one usually starts the introduction to *the*

density matrix. Perhaps I should write one or two more lines of my formalism before I explain the ideas of realistic interpretation.

I'll mention how this occurs in Dirac's book on quantum mechanics. They're called not precisely this, but I think recognizably the same thing. You probably didn't use the letter  $W$  for probability (speaking to Dirac), which you know is a Teutonism picked up from von Neumann. If I wrote  $p$ , it might be momentum. I forget what Dirac wrote. The thing looks like this, if you look at the proper section in the book.

$$R = \sum_m |m\rangle w_m \langle m| \quad \text{Gibbs ensemble} \quad (11)$$



It is called Gibbs ensemble in Dirac's book, which is a very good name for it.

The realistic interpretation is simply that maybe the system is in one of these states, and maybe it is in another, and so on. It is not in a state given by a particular one of these wave functions. It may be in a state given by another one of these wave functions, and so on. We do know which wave function we should give to the system, but we do know probabilities with which we might assign one or the other of these wave functions. So we take the average of the expectation values that the various wave functions would give it, weighted with the probabilities for the system to have such a wave function. And of course, this operator – well, I shall not go into the technical details of how this wonderful formula does exactly that same thing. But it's called Gibbs ensemble. You see, Gibbs ensemble does not necessarily mean anything with  $e$  to the minus something over  $kt$ . That is a Gibbs canonical ensemble.

The Gibbs ensemble basically is the idea that we could think of many systems, some prepared one way, some prepared another way, and the experiment consists of measuring on a system drawn from this ensemble. Then, you see, the fraction

$w_n$  of the systems was given the wave function  $\phi_n$  and so one

gets this result.

So one has this realistic interpretation. One can think of a lot of boxes, each box containing a system. If  $w_n$  is equal to 15%, well then 15% of the boxes were prepared with a function  $\Phi_n$ . Another 7% if another  $w_\ell$  is 7%. Well, that means that 7% of them were prepared with a wave function  $\Phi_\ell$  and so on.

Now you see that there are two possible situations here. We may have what is called a mixed state. That's with several of the  $w_n$ 's (more than one, at least, of them) greater than zero, and the rest, of course, zero. Or you may have what's called a pure state. In a pure state only one  $w_n$  is different from zero. And of course, that one is 1, since the sum of them is 1.

This means that  $w_n$  is 1 if  $n$  is a particular value, say  $n_0$  and is zero otherwise. For the pure state  $w_n$  is  $d_{nn_0}$ .

Now it must be emphasized that this mixed state does not mean – very definitely does not mean – a state which has a wave function which is a linear combination of some other wave functions. You find the expression used this way a mixture of s and d wave functions in some nuclear level, or something. That is not what is meant here, because there one takes a linear combination of the two wave functions and makes a definite wave function for the system. This is not that. A mixed state here does not have any wave function at all. It has instead, a list of probabilities for different wave functions.

It is not a list of coefficients where you can multiply them and add them up to get a wave function, but just a list of probabilities. So that to solve a problem, say find an expectation value, you just first solve it with one of the wave functions and then solve it with another, and so on, and finally average your answers after you're through.

Now this is exactly what you do classically when you don't know what sort of thing you have. If some of your boxes contain one thing and some of them contain another and you don't know which box contains which, you do know that a certain percentage of them contain each thing. And you can calculate an average like that by taking the averages for what the different possible contents would give and multiplying by the probabilities for the box to contain a particular thing. So in this realistic interpretation of the situation, we simply say that this gives us a way also to ascribe this density matrix with  $w$ 's in it to any such situation. We simply make it with the wave functions which the sort of preparation this had might allow it to have, and then assign probabilities in accordance with what you know about the situation. If you know, for instance, that the beam of particles came out of the furnace — just came out, there was no particular field on where the furnace was, and no particular deflecting arrangement, it's just coming out through some collimating slits — then you will

assign a mixed state to it, in which you give equal probabilities to all possible values of the spin component, because there is no reason to give anything but equal probabilities to them. Here one appeals to the principle of insufficient reason in precisely the same way that one does in classical probability theory. And, in fact, all the reasoning about these  $w_n$ 's is precisely the reasoning of the classical probability theory.

But here we have two different things coming in something which is just classical theory, just the classical theory of the Gibbs ensemble; and something else which is not at all classical theory. We have two sources of dispersion, two sources of what the statisticians call variance, but what the physicists call dispersion.

The dispersion in the values— that is, the spread in the values of a variable— can come from the mixing of the state, from this Gibbs ensemble situation. It comes from the fact that the various  $w_n$ 's give various contributions, that we have not prepared all the systems alike, or that we don't know exactly how to say in just what way the system was prepared. This has a classical analog. In fact, it's precisely like the classical case in every respect. All the calculations are just the classical ones. The analogy is extremely close.

In fact, it's identical in the way you calculate.

Then it has another source of dispersion. It comes from the dispersion in the individual state or in the pure state, the

various pure states. Each of those certainly gives dispersion to certain quantities. If for instance, I measure the momentum pretty carefully in each of these pure states  $\Phi_n$  ' then I'll have a sizable dispersion for the coordinate.

Thus there is another source of dispersion in quantum mechanics and it has no classical analog. In the early days of quantum mechanics some people, who were struggling to understand what in the world this statistical theory could be about, comforted themselves by saying, "Well, it's just a sort of Gibbs ensemble". It isn't! It's something entirely different. When you work in the usual way that elementary quantum mechanics does work with a wave function, you are working with something that has nothing whatever to do with the Gibbs ensemble. But it is true, that if you work in the most general possible way, you can build the Gibbs ensemble on top of the quantum mechanical situation. And for some purposes, in discussing some situations, it's quite important to do that.

Let's note one more thing. It's a famous result and somebody might, in the next few days even, find it useful to use in an argument. It takes only a moment to mention. If I have a pure state of this situation – a pure state, where only one of the  $w$ 's is different from zero – then you see  $R$  (always working in the representation where  $R$  is diagonal), then  $R_{mn}$  is  $d_{mn}$  because  $R$  is diagonal, and then it has to be

multiplied by  $w_n$ , and  $w_n$  is  $d_{nn_0}$ .

$$R_{mm} = \delta_{mm} w_m = \delta_{mm} \delta_{mm_0} \quad (12)$$

This is a neat little product of delta functions, you see, and you can put in another one,  $d_{mn}$

$$R_{mm} = \delta_{mm} \delta_{m_0} = \delta_{mm} \delta_{mm_0} \delta_{mm_0} \quad (13)$$

It doesn't cost you anything, but all three letters have got to be the same. If they are the same, it's 1, and now you can readily believe that when one works out the algebra for  $R^2_{mm}$  it will turn out in a line or two of writing that this is the same as  $R_{mm}$ . In other words, they just have to be equal, and if they are equal it's 1. Of course, when you square the matrix you have to use a summation. The summation drops down to one term because of all these delta functions. So  $R^2_{mm}$  is the same as  $R_{mm}$ .

$$(R^2)_{mm} = (R)_{mm} \quad (14)$$

2

This is now an algebraic relation between  $R^2$  and  $R$ . And it holds in this representation, so it holds in every representation. Algebraic relations between matrices have that property. The condition for a pure state is the so-called idempotent condition,  $R^2$  is equal to  $R$ .

I shall not go through any argument in which this comes up. I'll just mention a famous argument in which this criterion

for a pure state is used. That is, von Neumann's famous argument against hidden parameters, which has something to do with our thinking these days. Namely, this argument in which this criterion is used proves that if the formalism of quantum mechanics holds exactly — that is, within this formalism of quantum mechanics — it is not possible to ascribe this second form of dispersion to unknown but varying values of some sort of parameters which have not yet been discovered (which are, so to speak, hidden in the system). This is not a consistent way to describe the situation, provided one stays within the context of quantum mechanics. This, of course, doesn't mean that people who like — you know, it's been proved mathematically that when you prove something mathematically you always start with assumptions. For instance, I started with these assumptions (a), (b), etc., some of which are rather strong. And, of course, this proof of von Neumann's is based on the assumption that quantum mechanics is the exact and complete description of the situation. So if you don't choose to believe that, you can believe in hidden parameters. I don't say that I'm recommending this. I have normally been pretty orthodox in my own views, but I think it's only proper to say what the limitations are on a mathematical proof. In mathematics you prove something from assumptions. You don't prove it in the absolute.

Now I want to mention how this idea of mixed state comes in. This is the situation in the sort of thing which is really one of the key things with which we are confronted – the sort of problem that I conceive of us as undertaking to discuss this week – that's the problem of measuring some quantity. Now when you make a measurement in quantum mechanics, you do something. When you measure in quantum mechanics, the usual postulate is that when you measure a quantity you will get one of the eigenvalues as a result of the measurement. The probability that you will get a particular eigenvalue is the square of the absolute value of the inner product of the eigenfunction of that eigenvalue and the wave function. Of course, we now generalize it and say that the probability that you will get that eigenvalue is the square of the inner product multiplied by the  $w_n$  and summed – that is a loaded average of such calculated results. The important thing is the statement simply that when you measure, this is what you get. There is no statement made as to what happens in the actual measuring process. Two statements are made that you have these various probabilities of getting the various eigenvalues, and that after the measurement has been made – if it's what is called a predictive or preparative measurement – the system will be in a state which can be calculated in a suitable way from its previous state and from, the result of the measurement.



In particular, suppose its previous state was a pure state:

$$\psi = \phi_m \quad (15)$$

If the quantity measured has only one eigenfunction for the eigenvalue in question, then the state after measurement is a pure state with that function as the wave function. If it has many eigenfunctions, if the situation is degenerate, then you will also have to appeal to the previous state for evidence about the  $w$ 's in your new mixed state. At any rate, there is only a statement of these results. There is no statement as to what happens in the measuring.

This is what various people, Bohr, Aharonov, and Bohm, and other people called the "cut". It is where something happens which the theory does not describe mathematically.

Classical theory didn't have to describe how you measure things. That was self-evident to all. Why, you just looked and there it is. The moon goes around its orbit, the planets do their stuff, and we observe them. And we don't have to say what happens exactly when we observe them. If we do try to say what happens, let's say in a theory of the telescope, or a theory of the physiology of the retina, why we're just having some fun with more science. We are not really saying anything about what happens in the measuring process as such.

In quantum mechanics, however, we agree that the measurement can affect the state of the thing measured – we agree that

there *is* some sort of uncontrolled interaction between whatever we use to measure and the system measured. That's necessary because the measurement performed with a system prepared in precisely the same way may sometimes give a different result; and the system afterwards will then be in an eigenstate for the one result, or an entirely different eigenstate for the other result. So there was an interaction with the means used to make the measurement. So that in quantum theory we have something not really worse than we had in classical theory. In both theories you don't say what you do when you make a measurement, what the process is. But in quantum theory we have our attention focused on this situation. And we do become uncomfortable about it, because we have to talk about the effects of the measurement on the systems.

Now this discomfort can be allayed somewhat. In fact, this discomfort is most often <sup>completely allayed</sup> ~~displayed~~ among physicists because many people live long and fruitful lives without ever worrying about the problems that we are distressing ourselves with right now. But it can be allayed by noting that we can describe what is happening quantum mechanically, in principle, up to any particular point we please. We can change the position of this cut, this place where we suddenly say "Well, at this point we made the measurement and we applied the rules for what happens when you make a measurement, and we're not talking about how the measurement itself occurs."

I can do this, if I want to, if I have an object system which I'll call  $q$ , if we can distinguish that from zero. This object system  $q$  has coordinates,  $q$  say, and it has a wave function originally  $\Phi(q)$ . And if I want to, I can simply say, well, I measure the observable  $A$  on that, and I get the result, and let's take a case of a non-degenerate spectrum, so that the eigenvalue  $A_n$  has only one wave function, belonging to it. Then, of course, the probability of getting  $A_n$  will be the square of the absolute value of the inner product of  $\Phi_n$  with the original wave function

$$(\text{Probability of getting } A_n) = |(\Phi_n, \Phi)|^2 \quad (16)$$

After the measurement has been made, the wave function of the

observable will be that one of the  $\Phi_n$ 's that "belongs to the value we've got". We can simply say it that way, or we

can say, "I will not perform this mysterious and undescribed operation on this object. I will instead, couple to the object  $q$  another system, another quantum mechanical system which has coordinates  $x$  and which has a wave function before I start the game, of  $u(x)$ , and to which I've given the letter  $I$ , so that I mean it's the instrument. And I will couple this instrument to the object, let them interact a while, then I will de-couple them, and then anything mysterious and undescribed I do will be done to the instrument". All that happens to the

object will be described by the laws of quantum mechanics. Except that, of course, if I obtain incontrovertible information about the object, in the course of my perhaps obscene dealings with the instrument, I will, of course, make use of it, in future predictions about the object. This is all I have to do.

Of course, in making the general theory, we assume that the experimentalists are intelligent people. This is one of the assumptions for which we have excellent evidence. And we simply assume that they are able to devise — let's first note one more step before I say what they're able to devise. We have now the wave function of object and instrument before we begin our operations. The wave function for them is a function of both  $q$  and  $x$  and it is, of course, just the product of  $\Phi(q)$  and  $u(x)$ .

$$O: \phi(q); I: \mu(x); \psi(q, x) = \phi(q)\mu(x) \quad (17)$$

One readily verifies that this gives all the predictions about the separate systems that could be gotten from these wave functions. Now we assume that the experimentalist is intelligent enough, and ingenious enough, to provide an interaction Hamiltonian, that is, to provide a piece of apparatus whose use corresponds in the mathematics to the presence of an interaction Hamiltonian  $H_{\text{int}}$ , which is a function of  $q$  and  $x$ , which will be different from zero during

a certain period of time, namely from zero to  $T$ , and after that will subside again, so that there no longer will be any coupling between object and instrument.

$$H_{\text{int}}(q, x) \neq 0 \quad 0 < t < T \quad (18)$$

Almost anybody could get them coupled somehow, you know, and manage to shut it off. I might, if you gave me a few weeks to bone up in the laboratory. But now he must also pick this thing so that it does just the right thing. You see, during the presence of that interaction, of course, this wave function  $\Psi$  is at all times obeying this precise, and if you please, causal formula of quantum mechanics, wave mechanics.

$$i\hbar \frac{\partial \Psi}{\partial t} = H \Psi \quad (19)$$

During this time interval, from zero to  $T$ , the Hamiltonian includes not only the Hamiltonians for the separate systems, whatever they are, depending on their nature, but it also will

include, during that time interval, this trick interaction potential which our intelligent experimentalist has devised for us. Of course, we could probably devise it in a given case. Mr. Aharonov could devise it readily, and he and Bohm have done so. There are some very cute cases in their recent papers. But we assume that the experimentalist could actually build the thing in the laboratory.

During this time (18) the wave function changes according to this law (19) and, of course, because this involves both  $q$  and  $x$ , the  $q$  and  $x$  get all churned-up together. At the end we have a wave function  $\Psi$  which, of course, can be expanded in terms of any set of functions we please for the  $q$ 's. I can write the  $\Psi$ , which is a function of  $q$  and  $x$  still, I can write it as a superposition of the  $\Phi_n(q)$ , and the coefficients will be some functions of  $x$ . Now, I'm finally going to tell you how clever the experimentalist has been. He has chosen this interaction term so that the following is true.

$$\Psi(q, x, T) = \sum C_m \mu_m(x) \phi_m(q) \quad (20)$$

The coefficients are just certain functions  $u_m(x)$ , which are eigenfunctions of a variable  $P$ , the pointer reading on the instrument, which has the eigenvalues  $P_m$  and the eigenfunctions  $u_m(x)$ . And then, of course, constants times those functions, And furthermore, if this is to be precisely the kind of measurement I want, the values of the  $c_n$ 's must be suitable, because this is to give information about the potentialities present in the previous state.

Mr. Bohm introduced the fact that these are really potentialities. The system in this state did not really have these values of  $A$ . It had the potentialities of showing them if the measurements were made. So for the  $c_n$ 's still more remarkable properties are demanded of the  $H_{int}$ . It must have the properties that after it has served this way to determine the change of the wave function with time, according to the quantum mechanical formula, the  $c_n$  is to be equal to the inner product of  $\Phi_n$  and  $\Phi$ .

$$c_m = (\Phi_m, \Phi) \quad (21)$$

Now let's notice what the situation is. We have a wave function, a perfectly good wave function. It has arisen by the operation of the immutable and ineluctable laws of quantum mechanics from the initial state. And, corresponding to this wave function, what is the statistical situation about the object system? Well, we can work it out. Let's

say that B is an observable for the system  $q$ . Let's find the expectation value of B. Well, we have to integrate then the product  $\Psi^*$  by the result of applying B onto  $\Psi$ . That will have to be integrated over all the coordinates, that is, both  $x$  and  $q$ . Of course, each of these stands for a whole list, if we want them to. And so we write it out:

I'll put B in here,  $\mathcal{E}(B) = (\Psi, B\Psi)$  working on the

(22)

$$= \sum_n \sum_m c_m^* c_m \int \phi_m^*(q) B \phi_m(q) dq \int u_n^*(x) u_m(x) dx$$

$\Phi_m(q)$  because, of course, B belongs to the object system. It has nothing to do with these coordinates  $x$  and the instrument system. And then I simply have left the integral over  $x$  of  $u_n^* u_m$ . That, of course is  $d_{nm}$ , and that means that I can do the summation. And so I arrive at sum over  $m$  alone of  $|c_m|^2$  times the integral which is, of course, now the diagonal matrix element of B, because  $n$  and  $m$  are equal. It is then in fact the expectation value of B in the state  $\Phi_n$ . And we get a

mixed state, for the object, <sup>which</sup> has arisen through the action of

the laws of quantum mechanics on the total wave function. It is a pure state, of course, for the whole system of object and instrument we're considering. It is a mixed state of statistical information for the object. Now, first let's see what happens if we make some measurements. If we make a

measurement of both P and  $A_{\vec{p}}$  of course you see I'm not going

(dash)



to do that eventually. I'm not going to interfere with  $q$  at all. Let's just consider what would happen, what would inevitably happen, according to the quantum mechanical

doctrine if we <sup>were to</sup> measure both P and A. If we measured

both P and A then there would be a probability say  $|c_1|^2$  for me to get the value  $P_1$ , and the value  $A_1$ . There will be a probability  $|c_2|^2$  for me to get the values  $P_2$  and  $A_2$ , and so on. There is no probability whatever, there is zero probability in other words, of my getting  $P_1$  and  $A_2$  or  $P_2$  and  $A_1$ . And knowing the whole history of the situation, I know there is no chance of getting the wrong value of A, when I measure the value of P. I don't need to measure the value of A. I measure P. I look at the pointer and I know the value for A.

Now what happens when I look at the pointer? What happens to the system  $q$ ? The system  $q$  considered by itself was in a mixed state. A mixed state would be realistically described by saying that there is a probability  $|c_n|^2$  for it to be in the state with wave function  $\Phi_n$  and another probability  $|c_m|^2$  for it to be in the state with wave function  $\Phi_{m'}$ , and so on. Then I can take, as my realistic picture of what happened in the measuring process, that somehow or other in the coupling of the instrument, the object actually went into one of these states with these various probabilities for the

different ones. And then when I read the instrument I find out into which one it went. And, of course, once I know which one it's in, then I assign to  $q$  the wave function for that state. A perfectly reasonable procedure. If that were only all.

I see it's time for questions, but of course all the questions come from the fact that this is not all. This is an eminently satisfactory situation. But Einstein, Podolsky, and Rosen have rubbed our noses in the fact that this is not the whole story. In fact, it's impossible to maintain this nice realistic description that I just gave. They didn't say it this way— I said it this way. That's my merit in the case.

What they pointed out was that it is possible to have situations in which all of these  $c_n$ 's in this expression (20) are equal in absolute value over some wide range of states. Of course, there's an infinite number of states and we chop out a finite range of them — say two, in a very important example by Bohm, or Aharonov, or by Bohm in his book, or a thousand if you want, some finite number — and get equal values for the squares of the absolute values of these things here in (23), equal probabilities for the states.

$$E(B) = \sum_m |c_m|^2 B_{mmm} = \sum_m |c_m|^2 [E(B)]_{\phi_m} \quad (23)$$

As soon as you do that, then there is not just one way to write this  $\Psi(q, x, T)$  as the sum of the products of orthogonal functions, the so-called biorthogonal expansion, because it contains orthogonal functions in both places; there are an infinite number of ways to write it in that form. In fact, you will readily see that you could also write this wave function  $\Psi$  in this form, the sum over  $\ell$  of — let's see. I want the sum of the squares of them to be equal to one, because the squares of them are probabilities. So I'll say that there are  $N$  of the states, for which these coefficients

$c_m$  are equal, and they have the form

$$c_m = \frac{1}{\sqrt{N}} e^{i\gamma_m} \text{ for } N \text{ states} \quad (24)$$

This can also be written in the form of one over the square root of  $N$  times  $X_\ell$  of  $q$   $v_\ell$  of  $x$ ,

$$\Psi(q, x, T) = \sum_{\ell} \frac{1}{\sqrt{N}} v_{\ell}(x) X_{\ell}(q) \quad (25)$$

where the  $X_{\ell}$  can be any new wave function I please, any new

set of orthogonal wave functions connected with the  $\Phi_m$  by

some unitary matrix  $d_{\ell m}$ .

$$X_{\ell}(q) = \sum_m S_{\ell m} \Phi_m(q); S S^{\dagger} = 1 \quad (26)$$

S is a finite unitary matrix. Then (24) is equal to (20)

provided I also choose for  $v_\ell$  the new set of orthogonal functions given by this formulas

$$v_\ell(x) = \sum_m S_{\ell m}^+ e^{i\delta_m} \mu_m(x) \quad (27)$$

If you just substitute these two things in (25), you're back to (20). So you can make all the biorthogonal expansions you please, provided that the weights are equal in one of them.

This then is the trouble, because as Einstein, Podolsky, and Rosen said, I can set this (20) up for a position measurement. Then by measuring something about the instrument, I can find the position of the object exactly, or with extreme accuracy. But if this is the situation, then by just taking linear combinations for a transform for the position wave functions, I could just as well write the biorthogonal expansion the other way around, as in (25). I could make a momentum measurement, again without touching the object, again looking only at the instrument, and find out what the momentum of the object is. In neither case have I interfered with the object at all.

Now I cannot, in quantum mechanics, assert realistically that the particle made a transition to a state in which both its position and its momentum were accurately defined. There

is no such state in the theory. So my realistic interpretation has blown up in my face.

The realistic interpretation is perfectly good for laboratory situations, because, of course, the experimentalist is not interested in a measurement in which he knows already that all the probabilities for all the different answers are equal. He is interested in measuring to find out a particular probability distribution, of unequal probabilities for something, say to plot the momentum distribution for electrons and atoms. He is interested in inequalities. The experimentalist will always be free in the laboratory to interpret quantum mechanics as realistically as he wants to. We have here a situation which theorists cannot ignore, which you could easily concoct in the theory, and where the realistic interpretation fails completely. It's just not available.

Now the best example, I think, of this sort of thing is the example which Bohm, so far as I know, first put forward. That is the singlet state, say of a pair of spin one half particles. And this singlet state comes apart and particles fly off in opposite directions. Because it is a singlet state, I know if I measured the z component of the spin of the particle, I am bound to have the opposite value for the other one, and hence, I don't need to measure the other one, of course. I know that it is down if this one is up, and vice-versa. But of course, for this particle on this side of the

room, I can choose not to measure the z component but the y component, and again if I get "out", that one will be "in", or if I get "in" that one will be "out". I can do either one. But, of course, it's not possible for this to have made a transition into a state with both a definite value of the z component and a definite value of the y component. There is no such state.

Now, this is the hard thing to say. I'll make an attempt for one minute to say it and then be still, because I could only flounder if I tried longer. What this means is that there is a form of relation, a statistical relation, between these two particles, no matter how far apart they get; so that measurements on one will reveal things about the other; and so that one could make such a variety of measurements on this here, that it is not possible to say that one is merely finding out what state they're really in. One, in fact, in some sense creates the state of that other particle over there, when one makes the measurement on the instrument particle here, in just about as real a sense as one creates the state of a particle when one makes the measurements straight out without any of this argument about object and instrument.

So that it seems that the property of wholeness – the property of being something so that when you deal with it, you deal with it as a whole – the property of wholeness of

the quantum state can apply to systems in which the parts become widely separated, and in which one deals only with one part. This then indicates something which, if we are to regard the orthodox quantum mechanics as a final theory, we have to accept as one of the things that oblige us to take, as part of the doctrine, that this wholeness is typical of quantum systems in the small, let us say of the atom.

This property of wholeness is well known, that this wholeness extends into such cases as this, where two parts of of the system are very widely separated. Now I also think this is analogous to the wholeness of the quantum state which London has emphasized in the theory of superconductivity and superfluids. There, one again has over macroscopic systems, macroscopic distances— and in that case with a great many particles in them— one has this essential wholeness of the quantum state giving the properties to the macroscopic system.

CONFERENCE ON THE FOUNDATIONS OF QUANTUM MECHANICS

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Concept of Observation in Quantum Mechanics

by Eugene P. Wigner

Xavier University  
Cincinnati, Ohio



DR. EUGENE P. WIGNER

Thank you very much, it is a great pleasure to be here. I will talk about a subject which is not published, and it couldn't be published, because I would like to continue the ideas which Dr. Furry has told about. I shall try to continue what he did, and of course, I did not know very much ahead of time what he would say. He explained, with almost unbelievable conciseness and clarity, the process of measurement and what we know about it. But I would like to make one addition to it, and then explain in what way and how we are somewhat unhappy with it.

I do not mean to say that there is a logical flaw in the structure. Now there is no logical flaw and - I don't know whether I should say this three times over again - but there is no logical flaw in the structure, there is no logical flaw in the structure, the structure is free of logical flaws (audience laughter) - because it's very difficult to accept this if the man afterwards just the same says that he is not entirely happy with it. It is clear enough, well I said it three times and I think that should suffice.

Let me make now the single remark which I would like to add to Dr. Furry's talk. He explained to us how the quantum mechanical measurement can be described by considering it as an interaction - or nowadays people would say, as a collision - as a temporary interaction or collision between object and instrument, he called it. Now the result of this collision, he said, is a state of the joint system: object plus instrument, or object plus apparatus; in which neither of the two has a wave function, but only together do they have a wave function. Separately, they must be considered to be mixtures. That is the technical expression. They don't have separate wave functions.

But what the measurement accomplished was to give a statistical correlation between the properties of the instrument and the properties of the apparatus. As he explained to you, the correlation is such that if you now observe the instrument, it isn't necessary after that to observe the object, because you already know what the observation of the object would give. So this is the statistical correlation that has been established.

However, it is clear that the measurement is not completed because he said "If you now observe the instrument." He did not tell you how to observe the instrument. And the observation of the instrument, in some cases, may be even a very difficult task. But at any rate, it is again an observation. So that, as far as the description of the measurement by quantum mechanics is possible, it isn't a description of the full measurement but it is only the shifting of one of the measurements on the object to a measurement on what was called an instrument.

Now many people say, "Oh well, the instrument may be macroscopic. That's easy to observe". Well it is not so. Because the instrument, of course, may be macroscopic – but the instrument may be in a state which has no classical analog. And therefore, the observation on an instrument is fundamentally just as difficult and conceptually just as undescribed a problem as observation was to begin with. And I still quote exactly from the same source from which Professor Furry quoted, namely, the sixth chapter of von Neumann's book, where this is described.

What we can describe with quantum mechanics is the transmission of information from one to the other. But how we eventually get the information is not described and cannot be described clearly with quantum mechanics. One reason that it cannot be described was also mentioned by Dr. Furry. Namely, that the result of it is unpredictable, whereas

as long as quantum mechanical equations are valid everything is causal and predictable. So there is a final step in this: the cognition – or whatever more technical words are used – which cannot be described, of course. And we could not really expect quantum mechanics to describe it.

However, the fact is (and this is a point which has been brought out very often) that quantum mechanics does not permit objective reality. The wave function is only something that I use, and I use it to calculate probability connections between subsequent observations and that is all that I can calculate.

Now many people say that, "Oh well, that's not very spectacular. Classical mechanics can also be formulated as probability connections between subsequent observations." And that is true. But that means only that every theory can be formulated that way. Classical theory can also be formulated in terms of objective reality, but quantum mechanics cannot be formulated in terms of objective reality. This is a major difference between the two. And it is something with which we either have to come to equilibrium and accept, or we have to say, "Oh, we don't believe entirely what quantum mechanics tells us and we want to modify it." I don't know which one is the right procedure, but I think it is good to be clear about it, that one of the two things has to be accepted. Either we believe that quantum mechanics will have to be modified, and very fundamentally modified, by giving up the superposition principle, or else we have to acquiesce to the situation that the objective – well, what is usually called objective-reality, cannot be described and we have only probability connections between subsequent observations.

This is one of the two remarks which I wanted to make, still entirely within the spirit of Professor Furry's talk. Namely, to discuss the conceptual framework of quantum mechanics without any particular reference to its content. Professor Furry did not tell us that it is relativistic or not relativistic, that it describes a collision or doesn't describe a collision. He described only its language and not its content. Now this is one of the two points I would like to make which still refer only to the language.

Then I would like to make some remarks about how modern theory is compatible with it. And I will consider it from two points of view: from the point of view of relativistic invariance, which, as you know, plays a very fundamental role. Modern quantum mechanics is an attempt to reconcile relativity theory with quantum mechanics. And the other point of view from which I would like to discuss it, is the question, how realistic is it to consider this? Professor Furry said the experimental man makes an apparatus or instrument, he called it, which does this. Now, how does he do it?

But let me speak now about the other language problem which bothers me a great deal, and has bothered me since I learned these things many more years ago than I am happy to admit.

Professor Furry only mentioned an example of "What is the quantity which we measure?" He measured momentum, angular momentum, position, and so on. But if we look at the conceptual framework of quantum mechanics, "Oh" he said, "every self-adjoint operator can be measured" Well, why is it that we measure - as a rule - almost exclusively -positions?

If you ask a well-educated freshman how he measures the velocity, he wont tell you that he will measure it in the way Professor Furry

would want to measure it, namely with a grating to measure its momentum, and then divide by its mass. But if he measured it, "Oh", he will say, "I will measure its position at two times, take the difference, and divide by the time difference."

In other words, the position observable plays an entirely prominent role in all our measurements. Now why is that? If we think of it in an abstract way, we really can't explain this. And if there is such a very fundamental point here — that almost all our measurements are position measurements, whereas from the beginning all types of measurements are almost on a par — I feel terribly uneasy about it.

We come here to the question of measuring now the position, now the state of the apparatus, "Dr. Furry told us, "Oh, the apparatus has a pointer and we have to measure the position of the pointer." In that case he didn't talk about other measurements, but the measurement of the pointer. Wow why is that? To this I don't have any answer, and — well, I don't mean to repeat again what I repeated three times. I can't make a contradiction out of it, and it is not possible to make a contradiction, because the theory is logically consistent. But, there is something here which makes me at least, very uneasy.

Now, this brings me to the next question, which perhaps I still should classify as not in the content but in language. When we were — when one is young and one enters science, one has such a wonderful ideal how wonderful science is and what it will accomplish for us. One feels that it would be wonderful to be able to sit in a corner and have all our knowledge based on science. And — whether somebody will come in through the door — it would be wonderful to be able not just to say, "Oh yes, my girl friend is due just about this time", but somehow to

be able to calculate that scientifically. In other words, there is an ideal of what I might call "homo scientificus" – somebody who doesn't base his notions on everyday knowledge; on the properties of, well, the girl friend who keeps her appointments or not – but who would like to base his knowledge on scientific fact. Well, we don't necessarily want to have this; but this ideal, I think, exists in us when we enter science.

Now the fact that quantum mechanics gives us probability connections between subsequent observations reminds us very much of that, because it tells us, "Oh well, we have observed already this and that, from this we should be able to calculate this and that." Now, this again is, I think, a fallacy. And I want to point this out because I want to return to this question at the end. Because quantum mechanics brought it home to us that we cannot exist or cannot make science without being unscientific.

Professor Furry explained to us that the experimentalist uses certain apparatus to measure the position, let us say, or the momentum, or the angular momentum. Now, how does the experimentalist know that this apparatus will measure for him the position? "Oh", you say, "he observed that apparatus. He looked at it." Well that means that he carried out a measurement on it. How did he know that the apparatus with which he carried out that measurement will tell him the properties of the apparatus? Fundamentally, this is again a chain which has no beginning. And at the end we have to say, "We learned that as children how to judge what is around us." And there is no way to do this scientifically. The fact that in quantum mechanics we try to analyze the measurement process only brought this home to us that much sharply.

I mention this because at the end I would like to return to this very same question, which only teaches us a little humility in our science.

I would like now to enter a little more closely into the content of the theory, not only the language. In other words, to see where we stand. And there are two questions, as I mentioned, which I want to discuss in particular: namely, how relativistic the theory is, and how realistic the theory is. And, as I said, practically all my comments will be adverse comments on the theory of measurements. This is not surprising, because the favorable comments come naturally, and are made every day. The fact that we still have problems in physics is certainly not new, and the fact that these problems manifest themselves also in the theory of measurement is very natural and not at all surprising.

Now as to relativistic nature, the situation is, I think, this: What is it that we measure? We measure, according to Professor Furry -although he didn't use this word- the transition probabilities into a set of orthogonal states. Right? This is essentially what we measure. He called those orthogonal states  $\Psi_n$  and he said that we measure the quantity A. So for the operator A the equation would be  $A\Psi_n = A_n\Psi_n$ . You see, the eigenvalue A is only a label, What one really measures are the transition probabilities into the members of a complete orthogonal set.

Now, how is a complete orthogonal set defined? It is defined on a space-like cut in the universe. Right? It's not the universe in space-time. The  $\Psi_n$  is defined on a space-like cut in space-time, so that we measure the transition probabilities into something which is

defined on a space-like cut in space-time. Well, this clearly is not a relativistic concept. And, of course, what is a space-like cut in one coordinate system is tilted in time in a moving coordinate system, So that the question, as it's usually formulated, is not relativistic.

There are two ways to get out of this difficulty. The  $\Psi_n$  is a function which, let us say, is defined as a function of  $x$  at  $t$  equals zero. Now there are two ways to generalize this.

One way to generalize it is to say, "Oh, well, every measurement takes a certain length of time and therefore, what we really measure is not something that is defined as such a sharp cut but is defined somehow smeared out also in time." Well, possibly this is a useful and interesting way to do it. But this has never really been worked out or ever really even considered carefully. It is a difficult thing.

The other way to get out of the difficulty is to go to the other extreme and say, "We always measure something that is at a point. Namely, the field strength at this point or the density, or the current at this point."

Now, if you have something smeared out, and you make a coordinate transformation, it still will be smeared out. If it is a point, and you make a coordinate transformation, it still will be a point. What is not relativistically invariant is a "line parallel to this", because that will not be a "line parallel to this" after a coordinate transformation. But both the smeared-out thing and the point are.

The first way looks awfully difficult. So that one, in this way, is naturally led to the - Well, since the first one looks awf - Well, whether it's difficult or not, no one really did it seriously. It seems that one, in this way, is naturally led to consider field



quantities. This is done seriously and this is the quantum field theory in which the observables are localized, not only in time, but also in space. From that point of view, it is quite consistent and therefore, if one wants to relieve the non-relativistic nature of the observation concept, one must say that every real observable is something like a field strength at that point.

Now this sounds wonderful in principle. But if we think about whether it satisfies the other criterion, whether it is realistic, we come to a rather negative judgment. Bohr and Rosenfeld, as I am sure many of you know, analyzed this. And they came to the conclusion, "Yes, it is possible to make such a measurement provided we have an arbitrarily big charge in an extremely heavy point concentrated in an arbitrarily small space." Well, nobody has yet succeeded to do that! Well, it has other problems too.

So the situation is really this: If I try to satisfy the relativistic requirement – if I ask myself, "Is it relativistic?" – I can happily answer, "Yes". But if I ask myself, "Is it realistic?" Well, I'm afraid I must answer "No, it is not very realistic." The measurement of field strength at points, with the accuracy required to see quantum effects, not only has not yet been accomplished in practice, but evidently runs into very grave difficulties.

The last question which I would like to ask is, "Is it enough?" In other words, could I build up a theory only on this basis? And this is satisfied, and in fact it is done. So the quantum field theories operate only with the concept of field measurement, and they work. Well, many people say – and, I think, correctly – that they're not really terribly consistent in themselves. But, on the whole, the lack of consistency surely does not arise because one does not have enough

variables.

So, if one tries to satisfy the relativistic requirement, one is led to measure fields. It's really not quite right to say that one measures only fields – one measures also charges and currents – but what I mean by this is that one measures at space-time points, rather than either in an extended region or on a cut. So this is the situation.

I see that in my notes I put down far relativistic, not "yes", but "perhaps". This refers to the fact that we really do not have a consistent relativistic theory, so that whether that is a "yes" or a "perhaps" is really very difficult to tell.

The other question which impresses itself on one is, as I mentioned, whether the theory is realistic. Professor Furry's second postulate was that every hypermaximal Hermitian operator is measurable. Well, nobody really believes that. In fact, I am not sure that it is really necessary to put up this postulate, von Neumann put it up, and I have used it very often, because it's very convenient if one wants to prove something. It's much easier to prove something if you have many tools in your hand and if you can say, "Oh well, now I measure this and then I see that it can't be that way." But nobody really believes that everything is measurable. It's absurd to think of it.

As a matter of fact, if one analyzes carefully what has been measured in a quantum sense, it is a depressingly small number. I don't think the position can be measured. Isn't that right? How da you measure it? You have to be everywhere at the same time. This surely is not possible. In addition to that, clearly if I ask somebody to measure something like  $e$  to the  $x$ ,  $i$ ,  $d$ ,  $dx$ ,  $e$  to the  $x$ , plus one over one plus  $x^2$ , he will say "Don't make yourself ridiculous." Isn't

that it? This is really a very great conceptual difficulty. The conserved quantities can be measured. There's a great deal that can be said about it, but let me not go too much into it.

It is easy enough to say that there is a measurement. A really phenomenological theory, however, would not only say that there is such a measurement, but it would tell how you carry it out. It would say, "If you want to measure this quantity, order such and such screws from so and so, and put things together this and that way."

For this reason, Heisenberg in '43, I believe, proposed to base everything on the collision matrix. In other words, to admit that Hermitian operators are not really measurable, in general. In fact, they are not measurable. But what is measurable is only the momentum, and the character of a particle — whether it's a proton or electron or whatever it is. Well, not so many other particles do exist in this sense in which Heisenberg postulated it. The momentum is a conserved quantity, once the two systems separated, and therefore it is not necessary to measure it at one cut. You can measure it, so to say, at leisure. And the practical measurements, either with Professor Furry's grading or with the old fashioned systems, are measurements essentially of this nature — when it is smeared — well, when the measurement occupies a space time volume.

Let me put down, therefore, the second criticism and its elimination, namely "realistic". One wants to make the theory realistic and not to demand things which you evidently can't do.

Now this leads one to the idea of the collision matrix. You note that both these theories have been put forward by Heisenberg. This one was not put forward because he wanted a relativistic requirement to be satisfied for measurements, but this one was. You recognize here

the two great modern directions of quantum mechanics: the theory of the collision matrix and its direct calculation by means of dispersion relations, and the theory of the fields. We have to struggle along with them.

It happens also, that they relieve the two fundamental problems of the theory of measurement which come at once to mind. The unfortunate thing is, of course, that neither of them relieves all requirements entirely. If I go back to my three criteria - whether it is relativistically invariant and so on - well, the theory of collision matrix and of dispersion relations is relativistically invariant. The relativistic requirement is satisfied and there is no problem with it.

Well, it is also sufficiently realistic.

However, if we ask whether it is enough, whether it is possible to reduce every physical problem to a problem of collision - and calculate every physical problem by means of the collision matrix - I think we have to say that it is probably not the case. As a matter of fact, there is a good deal of discussion on this. And not very ago even I belonged to the school which hoped that it would be enough. It I think it was Källén who convinced me that it is not really enough.

Fundamentally it is not enough because the world is constantly in a collision with us, and there is a constant interaction between matter. Unless we make it the purpose of physics to describe only certain carefully made experiments, but not more than that, we can't get along entirely with just the collision matrix. It is not true that everything is only a collision. The world continues. For instance, a gas constantly exerts a pressure on the wall. There are many similar examples which show that it is not really possible to

reduce everything to a collision. And it is not true that the collision matrix really solves all problems. There are in this world other things of interest in addition to collisions.

So you see, these two eliminate many of the difficulties and, of course, that is why they are so attractive. But neither of them seems to eliminate all the difficulties together.

Now you probably also realize that there is a considerable discussion, let me call it, among the physicists, "Which is the more promising field?" It is almost true, unfortunately, that there is nobody who is entirely impartial between these two directions of work. Some of us believe that the field theories will give the solution of the problem—and I could point, even in this audience, to protagonists of that point of view. I could also find people who believe that the collision matrix approach will be the ultimately fruitful one. Perhaps it is good, for this reason, to emphasize that they are really working very closely together and the conflict between the two points of view is not so very strong. As a matter of fact, when it turned out that the collision matrix hypothesis was in gross conflict with the field theory hypothesis— you remember, with the Mandelstam representation — the collision matrix people, who swore up to that time by the Mandelstam representation, dropped it most underemoniously and returned to the field theory representation.

Now in one sense, I am practically through with what I wanted to say. But I would like to return to that question which I mentioned to you (and which, of course, is a little naive) about the "homo scientificus."

To what degree can we hope that our knowledge will also be ultimately supported in its details by science. I think we should realize that when we thought that this can be done for physics alone, we were a little too proud of our knowledge and of our discipline.

Surely the way knowledge is acquired in general, - and the working of the mind, - cannot be understood only by never having paid the slightest attention to the question, how the mind works and how, in particular, knowledge is acquired. I think a hope for a really integrated knowledge - and for an absence of these very unpleasant difficulties, or a reconciliation to this somewhat unpleasant fact of the absence of an absolute reality - this cannot come as long as we worry only how electrons, protons, and physical objects behave. It would be unreasonable to expect that, just as it was unreasonable to expect that we understand the behavior of protons and electrons only by studying macroscopic bodies.

Science has taught us that in order to understand something we must devote a great deal of careful thinking and detailed thinking to the subject in question.

This brings me to the last point which I want to make. Namely, that all this teaches us a great deal of humility as to the power of physics itself. It also gives us a good deal of interest in the other sciences, in particular to the general question, "How is it that knowledge and understanding is acquired either by ourselves, or - well, when we were children?" Or, "How is it acquired by other animals?"

It is perhaps not just a mere accident and coincidence that very great strides are made not by us, but by other sciences in these directions, and that surprising new results and new recognitions are gained in those fields. I think an integration of more than physics will be needed before we can arrive at a balanced and more encompassing view of the world, rather than the one which we derive from the ephemeral necessities of present day physics, which say that only probability connections between subsequent observations are meaningful, without really telling us at all anything about the character of observations.

Thank you very much.

Tuesday morning, October 2

One of the Observers:

Gentlemen, at the session we called before this meeting, we had a question session, and we wanted to ask a question very pertinent to this point. Shall we ask the question now?

Podolsky says: "Yes, let's have the question."

Carmi: Is it not true that a measurement will take a finite time and the measurement could influence previous possible results?

Dr. Aharonov has some ideas on this and maybe Dr. Rosen could fit right in here. If you make two instantaneous measurements, they may overlap because they take a finite time.

Rosen: ~~Perhaps I'll put it this way.~~ I think <sup>T</sup> there are some

measurements which could be carried out in a very short interval of time.

There are others which may require a long interval of

time. In the latter case you ~~don't~~ <sup>do not</sup> know the answer until <sup>i</sup> the end of that interval. Presumably, the state of the object ~~only~~ <sup>only</sup> becomes clear, <sup>only</sup> at the very end of this interval because in between

you have a period in which there is interaction taking place

between the instrument and the object and you ~~can't~~ <sup>cannot</sup> say anything

precise about the state of the object.

Aharonov says: Could I add something at this point? There was a time when I thought to solve this paradox in the case of measurement of position and momentum in the following way:

One of the difficulties of the Einstein-Podolsky-Rosen paradox is the fact that the collapse of the wave function of the far away particle occurs instantaneously (immediately when the measurement is done on the first particle). Now consider the case of the state where  $p_1 + p_2 = a_1$   $x_1 - x_2 = a_2$ . One finds that in relativistic theories it must take a period  $\Delta t$  in order to measure the momentum to the accuracy  $\Delta p = h/c\Delta t$ . But during this period  $x_1 - x_2$  becomes uncertain since  $v_1 - v_2 = (p_1 - p_2)/m$  is not certain. The hope was then that perhaps by the time a measurement of momentum is possible, a measurement of position will not be possible anymore. But it is clearly seen that the two periods of time are different and therefore the relativistic aspect of the paradox remains unchanged.



TUES:A.M.

Conference - October 1-5, 1962

Tuesday Morning, October 2

Dr. Rosen speaking. I want to make a few rather standard remarks about my ideas of measurement. I'm very glad that yesterday we heard the lectures of Professor Furry and Professor Wigner because the first one provided the basis for what I want to say, and the second one considered some difficulties which would otherwise take too long to discuss. Here I want to emphasize the following point, one which I

will ~~make~~<sup>refer to</sup> rather frequently. We saw yesterday that quantum

mechanics deals with probabilities, and when we talk about

probabilities we have to ~~talk~~<sup>think</sup> about a large number of systems

or a large number of measurements. It seems to me that the only satisfactory way to define the probability of something

is to say that if we ~~have~~<sup>make</sup> many measurements of the same kind,

then in such and such percent of the cases we get such and

such a result. Whenever we are ~~dealing with~~<sup>using</sup> the quantum mechanical formalism we ~~find not~~<sup>are dealing not with</sup> a single system but <sup>with</sup> a large

number of systems at the same time. In other words, we always deal with ensembles. Professor Furry discussed the idea of a Gibbs ensemble, but I want to go further and say that we have an ensemble in every case, whether we have a pure state or a mixture. Now this may be just a matter of words, but I'd like to use this idea and introduce names.

We ~~talked~~<sup>can talk</sup> about a coherent ensemble and an incoherent ensemble.

An incoherent ensemble is what Professor Furry called yesterday a mixed state, and a coherent ensemble is what he called a pure state. If we carry out a measurement on a single system, then in general, we don't know what the result of that measurement

will be, even when ~~a~~<sup>the</sup> system is in a given state. So we carry

out ~~a~~<sup>s</sup> measurement on many systems and get a frequency distribution

of the various results. There are exceptions of course. There may be a state which is an eigenstate of the observable, in which

case we are sure to get a single result. ~~We don't~~<sup>In this case we do not</sup> have to

distinguish between the single system and the ensemble, but in general we do have to. Perhaps again this is just a matter of words, but I'd like to put it this way. When we are dealing with

an ~~in~~coherent ensemble, we introduce probability amplitudes when

we write down equations. The idea of introducing probability amplitudes is, of course, strange from the classical point of

view, but ~~I'll~~<sup>I shall</sup> come back to this a little later when ~~I've~~<sup>I have</sup> said a few words about the classical interpretation of quantum mechanics.

Now I want to make several remarks about measurement. The whole question of measurement is a very complicated topic because

we ~~can~~<sup>should</sup> distinguish between a measurement which does not disturb the thing ~~measured~~<sup>being</sup> and a measurement which does disturb it. The authors of textbooks usually use the first one because ~~it's~~<sup>it is</sup> much

easier to discuss, but sometimes somebody really should go into

the whole question. One also ~~really~~ should consider the question of the accuracy with which the measurement is carried out. Within the formalism one usually thinks, <sup>we usually assume</sup> that when we measure a certain quantity we carry out an exact measurement, because that makes such things so simple. Unless we are taking a quantity which is a constant of the motion of the system, we ~~know~~ <sup>we believe</sup> that if immediately <sup>after a measurement</sup> afterward we make another measurement <sup>of the same quantity</sup> on the same system we are sure to get the same result. ~~so we conclude~~ <sup>then</sup> we know that the system is in a pure state, <sup>the eigenstate with the eigenvalue obtained in the measurement</sup> Well, of course, I'm ~~talking about measurement of the same quantity.~~ <sup>more precisely, we should</sup> We're really talking about the measurement of the maximum number of indepen-

dent observables whose operators commute with one another, and in this case, if the measurement has been carried out exactly,

we have a <sup>pure</sup> definite state. Now, what happens if you carry out an

approximate measurement, a measurement which has some error in

it? That is a more difficult question, which again seems to me

to require analysis. We should distinguish, of course, between

~~two things~~ the state of the system before the measurement, or

the ensemble to which the system belongs before the measurement,

and the new state of the system after the measurement, or the new

ensemble into which it has gone. This brings up the question of

reduction of the wave packet, which is the great mystery in this

whole discussion. ~~Now, one can put the thing as I just described~~

~~it, if one carries out an exact measurement on a system so~~

*It is possible to say as a result of the measurement*  
~~that we know the new state. Then we can say that we transferred~~

the one system that we are dealing with from the old ensemble to  
 the new. But I am not sure how to interpret *this.* ~~that exactly.~~ I

would like to think of it as something objective and not

~~subjective, although that's something that we can argue about.~~

~~Yesterday the question was raised about this instantaneous change~~

~~that takes place. This is the case of the reduction of the wave~~

~~packet.~~ Now if we think of this as a real physical change, as

something objective, then *the con* *the* ~~people~~ raise a question: When you have  
*correlation*  
 this ~~combination~~ between two systems that we talked about so

much yesterday and today, and if you carry out a measurement on one  
 system here and get a certain result which implies a definite

result for the other system, ~~then~~ it appears as though the result

of the measurement here is transmitted instantaneously to the

other system no matter how far away it is. *In that case* ~~Of course,~~ one can

also raise questions about consistency with relativity theory

and so on. ~~Now,~~ *I* if we take a subjective point of view, there is

no difficulty because Eddington once remarked; "We can transmit

thought as fast as we please, greater than *with* the speed of light."

But *would like to* I think that the world has an objective reality independent

of whether there are people *present* ~~around~~ to observe it or not. ~~I~~

~~would like to put my attention on a physically objective point of~~

~~view.~~ In that case, it seems to me that we have to put the matter

*I will present*  
 in a different way. ~~I'll put it in~~ *approaches* two different ~~forms~~ and  
 you can take one or the other, or neither.

First of all, one can say that one ~~doesn't~~ *does not* carry out a  
 measurement of position, for example, in a small region of space,  
 because if ~~you~~ *one* did ~~you~~ *he* would run into ~~all kinds of~~ difficulties.  
 Suppose you carry out *such* a measurement and you happen to find *by chance* that  
 the particle is within the ~~container~~ *small region in* which you are measuring.

You have no assurance that this is the particle that you were

interested in in the first place. ~~In fact, maybe~~ *T* *may be* there ~~are~~ other  
 particles of the same kind just outside your region, and perhaps  
~~you're~~ *you are* confusing the particles because they are identical.

To ~~be really precise here~~ and get unambiguous results we will  
 have to make a measurement, in principle, over all space. If we  
 find that there ~~was~~ *is* no particle anywhere else except at a ~~given~~ *one*  
 place, then ~~you~~ *we* know that the particle *is* there. *in question* ~~So~~ *I* *this* *therefor* in that case,  
~~it's~~ *it is* not a question of ~~having made~~ *making* a measurement in one region  
 and then ~~somehow~~ *having* the result affecting something at a distance,  
 because ~~you~~ *we* have made a measurement that included all space,  
 including the region in which the other system might be. ~~That's~~ *That is*

one way of looking at it.

Another way of looking at it is to talk about the state of the  
 system in the same way as one talks about the state of the system  
 when one is considering quantized fields, namely, as a state which is  
 not localized to any particular part

of space, ~~This is a state which~~ <sup>but rather one which</sup> is associated with the whole of space at a certain moment of time, (or with some kind of space-like surface, if <sup>one</sup> you want to generalize). ~~In that case,~~ any change brought about by a measurement changes the state ~~as~~ <sup>in this way,</sup> defined ~~it~~ and that means that it is a change which is

associated with all of space and does not require any transmission

of a signal from one place to another. ~~I'd like~~ <sup>I would</sup> in this case to refer to a contrary opinion, and ~~I hope it is useful for me to do this.~~ Now <sup>T</sup> there is a paper, ~~for example,~~ by D. R. Inglis, in the Review of Modern Physics, Volume 33, page 1, (1961), in which he discusses again what is referred to as ~~a~~ <sup>the</sup> paradox. If we have

the object system and the instrument interacting for a time and

then separating, so that the combined wave function is given by a sum of terms, such as Professor Furry ~~pointed out~~ <sup>discussed</sup> yesterday,

and if we then carry out a measurement on the instrument, giving

information about the object system, ~~in the case, let's say,~~

~~where you have the two piles of cards,~~ <sup>we have</sup> then ~~he would say~~ the

following: Before the measurement on the instrument is carried

out, ~~you have various~~ <sup>There are</sup> possibilities for getting <sup>various</sup> results if you

were to measure the object system itself. ~~But~~ <sup>A</sup> after you carry

out the measurement on the instrument and get a certain result, then

because of the correlation, you are certain to get one particular

result if you were to carry out the appropriate

measurement on the other system. Now, ~~he says,~~ <sup>according to Inglis,</sup> and I'm not

*insofar as I remember*  
~~quoting him exactly but this is~~ the sense of what he said, ~~that~~  
*systems*  
 if the two ~~are~~ far apart and you carry out a measurement on the  
 instrument, then before the signal associated with this measure-  
 ment reaches the object, you have the possibility of obtaining  
 various ~~possible~~ results on the other system; ~~But~~ as soon as  
 the signal arrives, the picture changes and you can ~~only~~ *only* get one  
 result, namely, the one that is associated with the result that

~~was~~ obtained from the instrument. Now it seems to me that ~~that~~ *this*  
 is not a satisfactory interpretation because we can see it ~~will~~ *that may*

lead to inconsistencies. We ~~get~~ *might* a contradiction in this case if  
 somebody ~~could be making~~ *made* a measurement on the object system ~~after the measurement on the~~ *instrument,*  
~~before the signal arrived,~~ *but* ~~Then it turns out the result is~~ *and obtained as*  
 different from what you would expect on the basis of the ~~measure-~~ *earlier*  
~~ment, carried out on the instrument before the measurement of the~~  
~~object, though the signal from this had not yet reached the~~  
~~instrument.~~ I believe that this is not a ~~correct~~ *satisfactory* point of view  
 and that, in order to have a consistent interpretation of quantum  
 theory, we have to say that as soon as ~~you~~ *one* carried out the

measurement on the instrument the result for the object has

been fixed instantaneously. ~~I think that's known to be under-~~ *This would be the case*  
~~stood~~ *assumes* if one ~~thinks,~~ as before, ~~either~~ that ~~somehow~~ the measure-  
 ment which is carried out is not something localized, but ~~which~~  
 is associated with all ~~of~~ *of* space, or else one takes the point of  
 view that the state is associated with all ~~of~~ *of* space and it changes  
 instantaneously ~~as the result of a measurement.~~ *as the result of a measurement.*

Conference - October 1-5, 1962

TUES: A.M.  
-10-

Tuesday Morning - October 2

Kaiser Kunz is speaking: So in a sense, it's not a signal sent at all.

Aharonov says: Yeah, we all agree that there is no way to send a signal by this kind of correlation.

Kunz is speaking again: <sup>This is</sup> what we have in classical probability

theory when we have more information.

Furry speaks: Yes, there are, of course, many, many ways to calculate velocities faster than that of light. The simplest one is that if I fire a bullet in this direction with three-fourths the speed of light and fire another bullet in the opposite direction with three-fourths the speed of light, and you ask me the relative speed of those two things, obviously it is for me three-halves the speed of light. There is no contradiction in relativity. What relativity tells us is that if either one of the people who are on the projectiles that are fired take the measurement, they will, of course, get less than  $c$ , but for me it's a matter of simple arithmetic.

Aharonov interjects: Yeah, but ---

Furry continues: there is no reason I can't use simple arithmetic and get three-halves  $c$ . Similarly here, if I make a measurement and from it I conclude right now, without sending signals, that the state of spin further away has a certain property, I can



make the assertion instantaneously of what will be found if one makes a measurement and doesn't state whether one has already made it, makes it right now, or makes it later. It doesn't matter. I make the assertion. This is not sending a signal and relativity theory limits only the sending of signals. This has been pointed out in connection with propagation of electromagnetic waves where the phase velocity may very well be faster than the speed of light, but the signal velocity is always not greater than the speed of light.

Podolsky says: The question of sending a signal arises this way. Supposing we have two photons with opposite angular momentum. We can measure the x component of one, and then we know what the x component of the other is going to be. If we measure the y component, we know what the y component of the other is going to be. The question was, by maneuvering the first measurement, can we tell something to the fellow at the other end who is going to make a measurement on the second photon? We can say, for instance, that the question is, "is it a boy or a girl?" and so all we have to do is transmit one bit of information. Can we do it by deliberately choosing the measurement one way or the other so the other fellow will find out what we have chosen by making a measurement on the other photon? It turns out that it can't be done.

Rosen speaks: Let us take the case of spins. Suppose we know that the two spins have to be opposite. We can have a measurement carried out on the first, on the instrument as we call it. We will find the spin is up, and so we conclude that the spin of the object is down. A person near the object which is correlated with the instrument may carry on a measurement on the object immediately after that and he will find that the spin

*However this does not tell him whether the measurement is down. He knows at the same time that, at a distance, the instrument has been carried out; he can only predict what the result would be if it were carried out, when he gets information about the system which is right near him, it's not a new thing. It's already something that's inherent in the object which is near him. There is really no new information transmitted.*

tion transmitted.

Aharonov says: Just let me add one more point. I think that your question stimulates further clarification. You said that if we project a light sending information classically, but here there is a difference. You see, classically, suppose we get here -

Kunz says: It's not information. <sup>as</sup> I said it's the same as <sup>in classical</sup> information ~~on~~ a probability experiment. What is the probability of a certain experiment? You may calculate <sup>on the basis of your information</sup> ~~and~~ someone else who <sup>each</sup>

<sup>has</sup> ~~knows~~ additional information will get a different probability.

Aharonov replies: The reason why one "feels" that the measurement of the first particle "does" something to the second particle in

the quantum case, contrary to the classical case, is the following; In classical theory every coordinate of a particle is fully determined. This means that when you get a signal from a far away star and you beam something about its orbit, you have learned about "something" that you believe was there all the time. In quantum theory, position and momentum do not "exist" together. Once you have chosen in this setup to measure position, you and everyone else have lost the opportunity to know the momentum. And even more, in a sense the far away particle was put in a state in which there is not a definite momentum at all. If, on the other hand, momentum was measured, the far away particle was put in a wave-like state and one can later perform an interference experiment on it. So the "feeling" is that by measuring

the first particle something is "done" to the second particle. It is either put in a particle-like state or in a wave-like state. Let me just add that this "feeling" is not necessarily correct, but it is there, and this is really the difference between the classical and quantum case.

Then Dr. Furry illustrated the Einstein-Podolsky-Rosen "paradox" with the following story:

First, you get two envelopes. Then some person, who becomes incommunicado or commits suicide immediately afterwards, takes one or the other of two playing cards, the red or the black, (we don't know which) and tears it in two, and puts half in each

envelope. One of the envelopes is sent to Chicago and at any time we can tell what the color of the half card in that envelope in Chicago is just by opening the envelope we have here. We can tell it instantaneously. It doesn't matter if they are opening the envelope in Chicago simultaneously with the one we have here, or before, or after. They will always correlate. This correlation was established in a way that didn't involve any violation of relativity, because they were both together at the time they were put into the envelopes.

Podolsky speaks: Yes, but there is a big difference here.

Furry replies: Oh, I know, because you used many decks.

Podolsky then says: No! (laughter) Not only that, but our opening one envelope to determine what the card is in Chicago does not in any way affect the possibilities in Chicago. While in this quantum mechanical experiment, it does, depending on whether we choose to open one envelope or the other.

Furry says: Well, I don't know whether Professor Rosen wants to yield long enough for me to describe my set of envelopes which corresponds more closely to your example.

Rosen says: Please go right ahead.

Furry continues: It's enough to use, say, two envelopes.

We enclose them in a slightly infernal box so that the removing of one of these envelopes from the box will promptly result in the complete obliteration of the other one. Now we have two of

these boxes, each with two envelopes. The person tears apart a card out of a deck and puts half in each of these two envelopes. For one of them he chooses a card which is either a black suit or a red suit. For the other one he chooses either a low card or a high card. He puts the black or red in the left-hand envelope, the low or high in the right. Then one box is sent to Chicago and the other is kept here. Now you see, there can never be any contradiction if we pull out the black or red and look at it. The other one is destroyed as soon as we pull it out by the infernal arrangement of the box. If we pull out black or red, we now know that if the corresponding envelope is pulled out in Chicago, we know what the answer will be. If the other envelope is pulled out in Chicago, we don't know anything. In any case, however, the sending of the box is perfectly well understood. There is no contradiction with relativity, and the attaining of information from one place or the other is just what it sounds like. The difference, of course, between the classical and the quantum picture is that the quantum mechanical state does not correspond to this because this nice classical picture of the box with two envelopes is the hidden parameter description and the hidden parameter description is denied in quantum mechanics. But this is the only difference between the two things and there is no difference at all about the questions of information and of distance and time.

Podolsky says: Thank you. I think that's a very good example.

Rosen continues: Well, in talking about measurements and the reduction of the wave packet we come upon this relevant points Just what does happen in the measurement? The fact is that at some stage we have to think of the measurement as making a decision among a number of different possibilities, singling out one result from a number of potential results. That is the essential feature in the final stage of the measurement. Simply calling one thing an object and the other an instrument in itself does not insure this, because one could treat both of them quantum-mechanically. As Professor Wigner pointed out, you have the same problem about carrying out the measurement on the

instrument that you had ~~for~~<sup>in</sup> carrying out a measurement on the object by using ~~an~~<sup>the</sup> instrument, so that, according to this line of thought, you can have ~~the measurement chained.~~<sup>an endless chain.</sup> ~~But~~<sup>yet</sup> somehow

or other, we are able to cut this chain and say that there are

certain instruments that ~~you want to~~<sup>we</sup> call classical ones which

have the property that they make a decision and give us one

answer out of many possibilities. When we get that answer, we <sup>have</sup> singled out one term in the expansion, which Professor Furry

wrote down, so that we get one term instead of the whole series.

This is ~~the process and we interpret it as~~<sup>what we call</sup> the reduction of the

wave packet. Now at this point I think it is appropriate to mention Dr. Everett's point of view, in which he does not accept

the idea of the reduction of the wave packet. I hope he will correct me if I say this incorrectly and I hope he will add something to what I say. As I understand it, he considers this whole series as continuing to exist even after the measurement has been carried out. He does not want to distinguish between the actual result as obtained in a given case and the other possible results which might have been obtained, so that even

after the measurement he has <sup>the</sup> ~~this~~ series of terms, instead of one

term. He thinks of the wave function as changing only in

accordance with the Schroedinger equation, <sup>in a continuous</sup> ~~in an integral~~ way,

without the possibility of this sudden change in the wave function, which we call the reduction of the wave packet. My own feeling

is that such a point of view is ~~again~~ tenable and consistent, but

should be interpreted as referring to <sup>not</sup> ~~not~~ what one observer finds

but what many observers <sup>Carrying out</sup> ~~doing~~ the same sort of <sup>measurement</sup> ~~thing~~ on the same

sort of system <sup>would</sup> ~~will~~ find. ~~But~~ <sup>Present</sup> If Dr. Everett does not agree with

me, I hope he will <sup>at</sup> ~~give~~ his point of view himself. Would you

care to say something ~~in~~ <sup>at</sup> this stage of our discussion?

Hugh Everett speaking: I think you said it essentially correctly.

My position is simply that I think you can make a tenable theory out of allowing the superpositions to continue forever, even for a single observer.

Shimony suggests: It seems to me that if this is the case, there are two possibilities. The two possibilities involve awareness.

*one possibility is that ordinary*  
~~The two possibilities involve awareness. Ordinary human~~

awareness is associated with one of these branches and not with

the others. Then the question comes, <sup>to</sup> how does one speak of *your*  
~~that?~~ *formalism permit this solution? The other possibility*  
*is that awareness is associated with each branch.*

Rosen interrupts: Wait just a moment. I think perhaps it would help the group if you (Everett) could give us a little bit

of background on this. I ~~sort of~~ *threw* you into the middle of *the*  
~~this and I'm not~~ *discussion, and I am not* sure that everybody in the audience is familiar  
 with your theory. Would you mind saying a few words?

Everett replies: Well, the picture that I have is *something like this:*  
~~that if you~~  
 I imagine an observer making a sequence of observations on a

number of, let's say, originally identical object systems. At the end of this sequence there is a large superposition of states, each element of which contains the observer as having

recorded a particular definite sequence *of results of observation.* I identify a single

element as what we think of as an experience, but still hold that it is tenable to assert that all of the elements simul-

taneously coexist. In any single element *of the* ~~in its~~ final

superposition after all these measurements, you have a state which describes the observer as having *observed* ~~had~~ a quite definite and apparently random sequence of events. Of course, it's *of events in each element of the superposition.*  
~~different for~~ a different sequence *or different states.* In

fact, if one takes a very large series of experiments, in a certain sense one can assert that for almost all of the

elements of the final superposition the frequencies of the *results of*  
 measurements will be in accord with what one *predicts* ~~gets~~ from the

ordinary picture of quantum mechanics. That is very briefly it.



Podolsky speaks: Perhaps it might be a little clearer to most people if you put it in a different way. Somehow or other we have here the parallel times or parallel worlds that science fiction likes to talk about so much. Every time a decision is made, the observer proceeds along one particular time while the other possibilities still exist and have physical reality.

Everett says: Yes, it's a consequence of the superposition principle that each separate element of the superposition will obey the same laws independent of the presence or absence of one another. Hence, why insist on having a certain selection of one of the elements as being real and all of the others somehow mysteriously vanishing.

Furry says: Actually, wouldn't you prefer to say that no decisions were made, but to the observer looking back it looks in retrospect as if the decisions were made. The observer also exists in all the other states, and in each of them as he looks back, it looks as if the appropriate decisions were made. This means that each of us, you see, exists on a great many sheets or versions and it's only on this one right here that you have any particular remembrance of the past. In some other ones we perhaps didn't come to Cincinnati.

Everett replies: ~~Somehow~~<sup>T</sup> the picture that it leads to I do

think is tenable, and I think it's the simplest one that can arise. We simply do away with the reduction of the wave packet.

Podolsky speaks: It's certainly consistent as far as we have heard of it. The question arises as to what happens if we have

a large number of observers and how these worlds of individual observers fit in together.

Everett replies: Well, again, all of the consistency of ordinary physics is preserved by the correlation structure of this state. You'll always find that an observer who repeats

the same measurement will always get the same answer, and <sup>even more will</sup> ~~be~~ <sup>always agree</sup> ~~be~~ <sup>when</sup> interacting with another observer measuring the same system. <sup>This</sup> consistency can be deduced from the structure of wave mechanics.

Podolsky speaks: It looks like we would have a non-denumerable infinity of worlds.

Everett: Yes.

Podolsky continues: Each proceeding with its own set of choices that have been made.

Furry says: To me, the hard thing about it is that one must picture the world, oneself, and everybody else as consisting not in just a countable number of copies but somehow or another in an undenumerable number of copies, and at this my imagination balks. I can think of various alternative Furrys doing different things, but I cannot think of a non-denumerable number of alternative Furrys.

(Podolsky chuckles)

Everett says: I'd like to make one final remark here. ~~The~~

(Correction made by Everett, bottom of page 20)

Imagine a very large series of experiments made by an observer. With each observation, the state of the observer splits into a number of states, one for each possible outcome, and correlated to the outcome. Thus the state of the observer is a constantly branching tree, each element of which describes a particular history of observations. Now, I would like to assert that, for a "typical" branch, the frequency of results will be precisely what is predicted by ordinary quantum mechanics. Even more strongly, I would like to assert that, as the number of observations goes to infinity, almost all branches will contain frequencies of results in accord with ordinary quantum theory predictions. To be able to make a statement like this requires that there be

~~almost all~~, some sort of a measure of states. <sup>on the superposition</sup> Now ~~I will do~~ <sup>what I need,</sup>  
~~this by saying what I need~~ <sup>therefore,</sup> is a measure that I can put on a

sum of orthogonal states. There is one consistency criteria which would be required for such a thing. Since my states are constantly branching, I must insist that the measure ~~is~~ <sup>is</sup> on a state originally equal to the sum of the measures on the separate branches after a branching process. Now this consistency criterion can be shown to lead directly to the squared amplitude of the coefficient, as the unique measure which satisfies this.

With this <sup>unique</sup> measure, <sup>deduced only from a consistency condition,</sup> I then can assert: indeed, for almost all <sup>(in the measure theoretic sense)</sup> elements of a very large superposition, the predictions of ordi-

nary quantum mechanics hold. Now I could draw a parallel here to statistical mechanics where the same sort of thing takes place. Here we like to make statements for almost all trajectories. They are ergodic and things like that. Here also you can only make such a statement if you have some under-

lying measure that you <sup>regard as fundamental, since any such statements</sup> ~~thought of before me.~~ ~~is~~ would be false if I take a measure that had only non-zero <sup>measure on the exceptional</sup> trajectories. In

statistical mechanics it turns out there is uniquely one measure of the phase space which you can use, the Lebesgue measure.

This is because it is preserved under the transformation of

phase space <sup>(by Liouville's theorem)</sup> being essentially <sup>the only measure giving the</sup> conservation of probability.

It is precisely this analogue that I use on the branching <sup>of the state function</sup> and I can <sup>therefore assert that the probabilistic interpretation of quantum mechanics</sup> Carmi speaks: (Some discussion of questions raised by

Dr. Gideon Carmi were incompletely recorded at this point in <sup>can be deduced quite as rigorously</sup> the session.)

*from pure wave mechanics as the deductions of statistical mechanics.*

Podolsky says to Shimony: Do you wish to comment on this?

Shimony: You eliminate one of the two alternatives I had in mind. You do associate awareness with each one of these.

Everett replies: Each individual branch looks like a perfectly respectable world where definite things have happened.

Shimony speaks: Then the question that I have about the alternatives that you have chosen is: what, from the standpoint of any one of these branches, is the difference within a branch, between your picture of the world and one in which there are stochastic elements?

Everett says: None whatever. The whole point of this viewpoint as that a deduction from it is that the standard interpretation will hold for all observers. *In addition, however,* ~~except~~ one can, within this viewpoint, get some hold on approximate measures and this type of thing.

Podolsky: Thank you, Dr. Everett.

Rosen: I am reminded of a story *by O. Henry last* I once ~~heard~~ *read* about a man who walked along ~~the~~ *a* road and came to a fork and ~~there were~~ *with*

several roads leading from it. He decided to follow one of them and certain things happened. Then the story went back to the same point and he decided to go along another road from the fork and something else happened to him, and so on for three or four versions, according to which ~~one~~ *road* he chose.

Aharonov: I think we should be happy because other parts of us are perhaps doing much nicer life because they have chosen different branches.

Rosen: Now ~~I~~ *I would* like to say a few words about this paradox

that has been referred to a number of times, which the literature has often referred to as the EPR paradox. The first point

I want to make is that I ~~don't~~<sup>do not</sup> believe ~~it's~~<sup>that it is</sup> a paradox. ~~in~~  
~~spite of the name.~~ I want to stress here the fact that it  
 implies no criticism of the correctness of quantum mechanics.

As we all know, from what Professor Wigner impressed upon us last night, in a certain domain quantum mechanics is correct and is self-consistent. The question that was raised in the

discussion ~~referred to perhaps~~<sup>regarded</sup> can be interpreted as a  
 philosophical <sup>or</sup> metaphysical one, ~~if one likes~~, because it used  
 such ~~things~~<sup>terms</sup> as reality and ~~I'm sure that not~~<sup>I am not sure that</sup> everybody will

agree on what this means. ~~As we have seen from the papers~~  
~~that have been published with their different points of view,~~  
~~if, by measuring things on one system we get information about~~  
~~different things on another system, some people would say~~  
~~that the various things that could be obtained by carrying~~  
~~through measurements in the first place are all elements of~~

reality. That perhaps could be called the classical point

~~is~~<sup>seems to be</sup> that if a system is in a state which does not  
 correspond to a precise value of a certain physical quantity,

then perhaps this physical quantity ~~doesn't~~<sup>does not</sup> exist somehow  
 or ~~doesn't~~<sup>does not</sup> have physical reality. These are things that

of view. On the other hand, the orthodox quantum mechanical cannot be verified by any measurement. We know that in recent years the attitude has been that only things which can be verified by measurement have any meaning, and that any

discussions about things which cannot be verified are meaningless

in a certain sense, Then we can ask, of course, 'Why bother raising this question in the first place?' I think the answer may be that if ~~one points out~~ <sup>we discuss</sup> these things, even though they do not have any bearing on the results of measurement within the framework of quantum mechanics as it exists at present, we point to the possibility of other theories, more complete ones, which would remove ~~what appears to be the insurmountable difficulty, bothering us.~~ Now there are two alternatives here. One is ~~a reformulation of~~ <sup>to reformulate</sup> quantum mechanics, ~~in its present stage.~~ <sup>present</sup> This is to say, ~~to put the same theory into a~~ <sup>one can try to put the existing</sup> different form, one which is more complete, which has elements

in it having a one to one correspondence with what one says in

classical theory is reality. The other one is, of course, to

~~have~~ <sup>look for</sup> a different theory. At the moment it is difficult to ~~see how we can find~~ <sup>imagine</sup> a different theory which, on the one hand, ~~will~~ <sup>would</sup> give ~~a~~ <sup>at</sup> least the same degree of agreement with experiment

as present quantum theory gives, and which, on the other hand, ~~would have~~ <sup>involve the</sup> essential changes, ~~Perhaps~~ <sup>sought.</sup> ~~such a theory will be~~ <sup>However, it is not excluded that</sup> found some day. I must say, ~~I'm~~ <sup>I am</sup> sometimes a little annoyed

at the attitude of some quantum mechanics because of a certain dogmatism that they display in these discussions.

There is an old saying that the revolutionary of yesterday is the conservative of today. Some people even refuse to

consider ~~that there can be any other~~ <sup>the possibility</sup> ~~view than that~~ <sup>valid point of</sup> which corresponds to ~~this~~ <sup>she</sup> orthodox interpretation. Of course,

nobody here in this discussion is considered to be guilty.

Furry says: There also are people angry that the word orthodox

is used. (Chuckles among the panel)

Wigner says: No, I don't think so. I think I started to use that word and if anyone's orthodox, I am orthodox.

Furry: Oh, there are orthodox people who are not angry at the word orthodox. There are also the orthodox people who do not want that <sup>expression</sup> ~~viewpoint~~ used.

Rosen: Now I would like to say a few more words about this so-called paradox. I think all the panelists are familiar

with these ideas so I <sup>do not</sup> ~~don't~~ need to go over them. <sup>However,</sup> ~~But~~ the essential point is that, <sup>one considers which,</sup> a measurement, at least from the

classical point of view, does not disturb the system about which you ultimately get information. Here I would like to

distinguish between two things in quantum theory: <sup>t</sup> ~~The~~ formalism

as it exists and the way in which it describes physical systems on the one hand and, on the other hand, the analysis of a

particular process of measurement. <sup>I would</sup> ~~I'd~~ like to call your

attention, for example, to the work of Heisenberg. In his little book on the physical foundations of quantum theory, he analyzes various conceivable measurements in detail, and shows

<sup>If we take,</sup> ~~Now in those measurements,~~ for example, the case of determining ~~of~~ the coordinate and momentum of the electron, the reader gets the impression that <sup>Heisenberg</sup> ~~he~~ starts out with the idea that the electron has a position and momentum and <sup>a</sup> ~~we're~~ trying to determine <sup>them</sup> ~~it~~. <sup>However,</sup> ~~But~~ because of the interaction between the

measuring instrument and the electron, that is to say, because the electron is disturbed in an uncontrollable way, ~~he tries~~

that in every case one arrives at the uncertainty relation.



~~one~~ and finds that he is not able to determine both these

quantities with complete accuracy. There is always a certain limitation, ~~and he gets the new principle expressed in the~~ <sup>described by what is now called</sup> Heisenberg principle. When we go over to the quantum formalism

we find that ~~the~~ <sup>this</sup> formalism is consistent with ~~these kind of~~ <sup>this limitation.</sup> ideas. It gives us a description of the electron by using a wave function, and the wave function ~~itself having~~ <sup>has</sup> the

property that it cannot give us information beyond the limits set by the uncertainty principle. So here we say <sup>that</sup> everything

is fine because the information that the wave function gives us is not any more precise than what we could have obtained by a measurement on the system, taking into account the disturbance produced on the system by the measurement. On

the other hand, in the examples <sup>Connected with the so-called paradox,</sup> ~~that we're speaking of,~~ we ~~do not~~ <sup>we are</sup> have this situation because ~~we're~~ <sup>we're</sup> making measurements on something else, not on the system <sup>about</sup> ~~from~~ which we want <sup>to get</sup>

information. The system is not being disturbed, and we do not have an explanation for the uncertainty principle in terms of the

disturbance on the system. Nevertheless, the uncertainty principle holds, and we get the situation that I have

described. It is this fact that has led <sup>some</sup> people to believe that the description given by quantum mechanics is incomplete,

~~because~~ <sup>since</sup> it gives a limited description of the system, which is not associated with the limitation on measurement, ~~for~~ <sup>due to</sup> instance, ~~because of~~ <sup>of</sup> the disturbance ~~on~~ the system. ~~I'm not~~ sure whether that comes through clearly, but I hope you get the idea. As a matter of fact, <sup>Let us</sup> ~~if one does not~~ consider ~~two~~ <sup>not</sup>

systems, but ~~instead considers one~~ <sup>a single</sup> system, a simple electron. We start out <sup>1</sup> by preparing the electron in such a way that we

know that it has an exact coordinate. That is to say, it is described by a wave function called a delta function, telling

us where it is. ~~Now~~ <sup>I</sup> if we <sup>now</sup> ~~then~~ measure <sup>its</sup> momentum precisely, ~~of course~~ <sup>then,</sup> as a result of the measurement, the position will be changed by an uncontrollable amount and we <sup>shall no longer</sup> ~~won't~~ know where

it is, but we will know its momentum. Now there are two ways

of looking at it. We can ~~either~~ say that the momentum we have

obtained is the momentum which the electron had just before the measurement, since we can make a momentum measurement which

does not disturb the momentum. This would be the ~~more or less~~ classical way of looking at it. <sup>on the other hand,</sup> ~~Also,~~ we can say that because the electron was <sup>previously</sup> <sup>1</sup> in a state corresponding to a wide range of

possible values of momentum, that is to say, it was in a state

~~that is not one~~ corresponding to a precise value of momentum, and ~~that~~ <sup>then</sup> <sup>1</sup> the electron ~~does~~ <sup>did</sup> not have any momentum <sup>before the measurement;</sup> <sup>1</sup> because it only acquires <sup>it</sup> momentum as a result of our measurement. That

is more or less, I think, the orthodox quantum view. In this

way of looking at it, then, we give <sup>to</sup> <sup>1</sup> the system a physical quantity when we measure <sup>the</sup> <sup>1</sup> a physical quantity. ~~I hope this is the correct way of putting it.~~

Merzbacher asks: Is this what Professor Furry refers to as realistic?

Furry comes in: Well, the point of view that it already had the momentum before we measured it would be a realistic point

of view. <sup>if it had</sup> ~~It has~~ the momentum ~~from~~ before we measured it would

be a realistic point of view. Of course, quantum mechanics does not allow that in this case. This is a point in which Bohm introduces the word potential or potentiality. When a system has a wave function which is not an eigenfunction of a given observable, then it does not have a value for that observable. It has only potentialities for having various values and when we make the measurement of one of these quantities, the quantity in question, one of these potentialities gets realized. This is, I think, the best way to put it. I don't think that anyone <sup>had</sup> has ever put it in just that way before Bohm wrote his book on quantum mechanics. I think that this is probably the best statement of the quantum mechanical view that we are venturing to call the orthodox view of the subject, the view which I think probably most of us hold. But like many orthodoxies, it is possible to hold to this orthodoxy without ever having examined terribly closely just all that it implies. Bohm, in his book, examined it far more closely than most of us have.

Aharonov says: I think that's a good way of putting it.

Rosen: I think we should stop now for some coffee.

TUESDAY MORNING AFTER BREAK.

Rosen says: ~~But~~ Let us continue. During the intermission Dr. Guth made a remark to me and I feel that everybody should hear it so would you please say the same thing that you said to me? (he says to Guth)

Guth: Professor Rosen referred to a book by Heisenberg in which he

mentioned two things. One, discussion of experiments. (Guth makes some remarks which were not recorded clearly)

Furry interrupting Guth: You think you should obtain exactly the factor  $h$  over four  $\pi$ ?

Guth: Exactly.

Furry: Instead of just approximately?

Guth: (continues) I would like to quote Pauli who did a somewhat similar derivation. He discusses the question whether, in a relativistic theory, one can measure  $\Delta x$  better than  $h/mc$  and then he discusses it in theory of relativity but then he adds...  $\Delta x$  up to  $h$  over  $mc$ . Where this result can be assigned fundamental physical significance, can be decided only when you have a consistent formalism, but I think there is a gap here in derivation of measurement theory...very interesting and very enlightening discussions and exact theory and these discussions come out with the right results...

Furry replies: There is one paper of this general sort in which a little more care was taken with the factors and which, as I remember it, it comes out precisely right. This is not the discussion of the cases which Heisenberg talked about. It is the paper that Ramsey and I wrote in connection with the Aharonov-Bohm effect, and I think that if you will look at that you will find that the

remember it, the paper was written with a slight variety. There

factor<sup>5</sup>  $2\pi$  and everything<sup>else</sup> are precisely in order. In fact, as I

were two cases discussed, the electrostatic and the magnetic. In one of them we made it all come out exactly and in the other one we left it a little sloppy, so that you have a sample of both sorts of discussions. That is, I think it probably can be done in all these elementary cases. Now the other thing, the one about the  $h$  over  $mc$ .

There presumably the better treatment is <sup>that in</sup> ~~all~~ the positron theory.

What really happens when you try to push this  $h$  over  $mc$  is going to be that you just don't have one particle any more, but you produce pairs.

Rosen: If there are no further comments, then <sup>I will</sup> ~~I'll~~ continue with the discussion that I started before the intermission. The point I had made there was that there are two ways of looking at this electron. One was that it had a coordinate and momentum just before the last measurement and that the wave function is not able to describe a state in which both of these have exact values. That is what is referred to as the realistic point of view. The other one is that before the measurement, since the electron is in a state which is an eigenstate of the position but not of the momentum, the electron has a position but does not have a momentum. If you accept ~~this~~ <sup>the</sup>

first point of view, then you say that quantum mechanics is incom-

plete. It is correct, but it is incomplete, <sup>so</sup> because the description

is not as detailed as you believe the reality itself to be. That

is the essential point of the so-called paradox. ~~At least the point of the discussion involving it is to bring out the idea that quantum mechanics is incomplete. If you accept the second point~~

*If you accept the second point*

of view, of course, then quantum mechanics is complete because reality is what is given by quantum mechanics, so that by definition there is a one to one correspondence...I purposely stressed in the beginning the idea of having to interpret the wave function or the state of a system in terms of an ensemble, because that could be used in the present discussion. You see, when we have a state of a system in which the electron is described as having an exact position

but an indefinite momentum, <sup>one can</sup> ~~that is, a delta function.~~ You could interpret this as saying that <sup>the</sup> ~~this~~ wave function describes an ensemble of many electrons, each of which has this particular position, but <sup>that the various particles have</sup> ~~has~~ different values of momentum. This is what I termed <sup>earlier</sup> ~~originally~~ as a coherent ensemble, or which Professor Furry and others call a pure state. Perhaps <sup>it is</sup> ~~it's~~ a weakness of us as human beings, <sup>but</sup> I think <sup>that</sup> most of us ~~nevertheless~~ picture the electron as having a position and momentum, even if we talk about it <sup>as</sup> not

having a momentum according to quantum mechanics.

Now this brings me to the question of hidden parameters, <sup>about</sup> ~~of~~ which

I would like to say a few words. Professor Wigner discussed it last night. He gave a very good proof of the nonexistence of hidden

parameters himself, and also referred to von Neumann's proof. <sup>However,</sup> ~~But~~

I think that one would have to specify exactly what one means by hidden parameters before one decides whether they are permissible

or not in the framework of the present quantum formalism, ~~because~~

<sup>The</sup> fact is, that, in spite of the proof that exists to the contrary, one can set up a picture which is consistent with quantum theory and

nevertheless contains hidden parameters. ~~This is the same thing which has been discussed quite a bit. I would like to consider this problem by referring~~ to something I did on this a long time ago. ~~Since it was not a very good piece of work, I think it doesn't add anything to my credit.~~ It is published in a rather obscure

journal so that it was never abstracted and very few people ever heard of it. It's in the Journal of the Elisha Mitchell Scientific Society, volume 61, page 67, (1945).

Merzbacher interrupts: ~~May I make a comment? I once had occasion to ask the editors of Science Abstracts to include this journal in their lists of journals from which they regularly make abstracts. They asked for some examination copies, and then declined this.~~

Rosen: I should explain that this is a journal published at the

University of North Carolina. ~~It~~ <sup>It</sup> consists mostly of articles on biological matters ~~and applications of~~ <sup>biology and</sup> mathematics, and once in a

great while, on physics. It so happens that the University of North Carolina was founded in 1795 and on the occasion of the sesquicentennial celebration I was asked to submit an article.

~~That's~~ <sup>That is</sup> why it appeared there. Subsequently David Bohm wrote some

very fine papers in which he did a much better job than I did and

~~maybe~~ I should refer to them. Physical Review, volume 85, pages

166 to 193, 1952. Of course, he knew nothing about what I had done at the time he wrote this. The idea involved is that when you take a time dependent Schrodinger equation for a particle, let us say,

Correction to equations on page 33

in a given potential

$$i\hbar \frac{\partial \Psi}{\partial t} = -\frac{\hbar^2}{2m} \nabla^2 \Psi + V\Psi$$

and you write

$$\Psi = R e^{i\hbar^{-1} S}, \quad \rho = R^2,$$

where  $S$  and  $R$  are real functions, you obtain two equations:

$$\frac{\partial S}{\partial t} + \frac{1}{2m} (\nabla S)^2 + V - \frac{\hbar^2}{2m} \frac{\nabla^2 R}{R} = 0,$$

$$\frac{\partial \rho}{\partial t} + \frac{1}{m} \nabla \cdot (\rho \nabla S) = 0$$



← Replaced by material on left-hand page

TUES: AM -33-

in a given potential field, so that you can say it is

$$i\hbar \frac{\partial \psi}{\partial t} = -\frac{\hbar^2}{2m} \nabla^2 \psi + V\psi; \quad \psi = R e^{(i/\hbar)S}; \quad \rho = R^2$$

I get two equations:  $\frac{\partial S}{\partial t} + \frac{1}{2m} (\nabla S)^2 + V - \frac{\hbar^2}{2m} \nabla^2 R = 0$

$$\frac{\partial \rho}{\partial t} + \frac{1}{m} \nabla \cdot (\rho \nabla S) = 0$$

And the first equation, except for the last term, looks like the

Hamilton-Jacobi equation of classical physics for the motion of a particle in a potential field. Quantum mechanics has introduced

the last term, and what one can say is, that if we are willing to

introduce a new potential energy, instead of  $V$  we ~~take~~ <sup>taking</sup>  $V + V_q$  as

potential energy where  $U$  is the sum of these two,  $V + V_q = U$

where  $V_q$  is defined as  $V_q = \frac{-\hbar^2}{2m} \frac{\nabla^2 R}{R}$  <sup>then</sup> And now the first equation

looks like a classical equation of motion with <sup>in</sup> the potential field  $U$ .

<sup>One</sup> And now you can think of the two equations together as

describing a classical ensemble of particles, each of which is

acted upon <sup>or</sup> by a force <sup>described</sup> provided by the potential,  $U$ . ~~The particle is~~

~~free to move.~~ The particles do not interact with one another except

through the fact that <sup>the</sup> ~~this~~ term  $V_q$  is present, <sup>depending on the distribution of all the</sup> The particles are <sup>particles</sup>

distributed with a density  $\rho$  and each one has a velocity at a

given point of  $1/m$  times the gradient of  $S$ , and <sup>that being,</sup> ~~the gradient of  $S$~~

~~is,~~ as in classical physics, the momentum of each particle, and ~~that,~~

~~divided by the mass, is the velocity.~~ And so you have a classical

picture. According to this point of view, you <sup>do not</sup> ~~don't~~ know without

some measurement where a particular particle is located. You have

only your distribution function  $\rho$  to describe the <sup>ensemble of</sup> particles. ~~BUT~~ <sup>However</sup>

if you carry out a measurement and find that the particle is at a certain place, then, provided you have already solved these equations

so that you know  $\psi$ , or that you know  $R$  and  $S$ , then you know that at that place, <sup>the</sup> particle <sup>had</sup> a definite momentum, <sup>just before the measurement,</sup> namely, the momentum given by the gradient of  $S$  at that ~~particular~~ point. <sup>To be sure,</sup> ~~now~~

this is just a way of visualizing things. I want to stress that

this is not changing anything <sup>essential</sup> in the formalism because ~~this pre-~~ <sup>supposes</sup> that the wave function  $\psi$  exists, <sup>and</sup> since from the  $\psi$  you get the  $R$  and the  $S$ , <sup>It is</sup> ~~its~~ just a way of picturing what happens in

a classical manner. Now this can be regarded as giving a more complete description than the usual quantum mechanics does, without

any change in the ~~formalism so different~~ <sup>predicted</sup> results, ~~will be obtained.~~ ~~But~~ <sup>For</sup> those who like to think classically, this is a way of reconciling <sup>this</sup> ~~their~~ desire on the one hand <sup>with</sup> ~~and~~ quantum mechanics, as it exists, on the other hand. ~~so~~ <sup>What</sup> I want to say is here we have hidden parameters, but there is nothing ~~here~~ <sup>that</sup> which is inconsistent with quantum mechanics because it is just quantum mechanics, ~~in the~~ <sup>It is</sup> ~~usual form.~~ ~~It's~~ just another way of writing it. Now if one asks

"How is this possible?"...

Wigner interrupts: The function is time-dependent. The potential is time-dependent.

Rosen: That depends on whether you're dealing with a stationary state or not.

Wigner: But in general, it's time-dependent, and also there is the

potential. Is this equation supposed to describe the motion of a single particle or is it suppose to describe... What is it supposed to describe?

Rosen: That's a very good point. Thank you. The way I would put it is, ~~I'm not sure whether I...~~

Wigner: Perhaps we shouldn't enter this discussion.

Rosen: Well, ~~I will~~ <sup>I will</sup> say a word here. The way I would like to put it is that this describes, as I said in the very beginning, an ensemble of particles. However, this is a coherent ensemble and there is somehow a certain correlation or interrelation among the different members of the ensemble. Of course, quantum mechanics agrees with experiment, ~~and it~~ <sup>but the present picture</sup> says that each particle in that ensemble behaves in such a way that the motion is described classically, provided you assume that there is some kind of force acting on it which is associated with the ensemble, namely, this additional term.

Wigner: So that it would not be valid for a single particle?

Merzbacher: You can shoot the particles in separately, can't you? In other words, the single particle knows that all the other members of the ensemble have come before or are coming later, somehow or other. Is that true?

Rosen: You see, you ~~can't~~ <sup>cannot</sup> get something for nothing. You have to pay a price and the price that you pay here is this. ~~if~~ <sup>i</sup> you want

to have a classical picture of the behavior of the electron, then

you have to say there is a certain force acting on it which, ~~of~~ <sup>when</sup> ~~course,~~ has no classical analogue. This contains  $h$ , and  $h$  is zero

this goes out in general.

Wigner: But the  $\hbar$  doesn't bother me. What bothers me is that proper...

Rosen speaks again: Perhaps let me say something and then ~~we'll~~ <sup>we will</sup> come back to this. According to this picture, ~~and~~ and again you can accept it or not, ~~it's~~ <sup>it is</sup> a matter of taste, ~~you say that if you're~~ <sup>you are</sup>

talking about a single particle which is in a certain state, according to quantum mechanics that means that it is in a certain ensemble.

With this state or ensemble are associated two real functions,  $R$  and  $S$  or  $\rho$  and  $s$ . And now if you want the particle to be described

classically you have to say that in addition to the classical force which acts on it, there is another force which is associated with the ensemble, in spite of the fact that you are looking at a

single particle. The ensemble exists ~~sometimes, perhaps~~ somehow...

Podolsky interrupts: Isn't it true that what is described by these equations is a set of surfaces and all that we know is that

the particle will be moving orthogonal to these surfaces? We ~~wouldn't~~ <sup>would not</sup>

know where on the surface the particle is unless that is specified.

Rosen: Or measured.

Podolsky: or measured.

Furry: You do not have a density specified.

Rosen: Density? ~~That's right,~~ <sup>yes</sup> this  $\rho$ . You have a distribution.

You have an ensemble which contains many particles, each behaving in a ~~slightly~~ different way. However, if you carry out a measurement and locate the particle in a certain place, then according to

this picture, you can also know what its momentum is, because of knowing  $\rho$  and  $s$  as functions of position and time.

Furry: What do you do after that measurement? You have not changed your  $R$  once you make a measurement, you must change your  $R$  to an  $R$  which is say a delta function around where the particle is?

Rosen: Of course, this presupposes that everything you talked about yesterday holds.

Furry: Yes, this is just quantum mechanics.

Rosen: This is the picture as it was just before the measurement. If you carry out the measurement, you change things, and then the picture is changed.

Aharonov: Can you introduce the measurement with some kind of potential and do this, perhaps, to collapse the wave packet? Since each wave is supposed to be classical, we don't believe that something really collapsed. It should be simply something like coupled waves and when you get more information, it's going to be changed. That's the way it looks.

Rosen: The process of measurement is something which quantum mechanics

itself ~~doesn't~~ <sup>does not</sup> try to describe. Why should classical mechanics try to describe it?

Aharonov: But you invent a certain environment just to solve this problem of measurement and now you tell us we are not supposed to discuss it. You invent a certain something or other just to solve this problem. You invent hidden variables just to solve this problem and now you tell us we are not supposed to discuss it.

Rosen: No, I ~~didn't~~ <sup>did not</sup> invent hidden variables to describe the process of measurement, but just to give one a picture...

Aharonov: Oh.

Rosen continues: ~~within~~ a classical picture of how a system looks within the framework of quantum theory.

Podolsky: In other words, is there such a thing as a hidden parameter possible in spite of von Neumann's proof?

Rosen: Yes.

Wigner: That is a proof that there are hidden parameters.

Aharonov: Yeah.

Wigner: What is a hidden parameter?

Aharonov: A hidden parameter is supposed to tell us what will be the result of the measurement in the theory of observables.

Rosen: Well, there is no need for hidden parameters, <sup>but they are</sup> ~~it~~ possible. I think, ~~perhaps, one could answer this perhaps not realistic question by saying that it seems to be possible because the parameters~~ <sup>that they are</sup>

themselves do not contribute to the state of the system, but are determined by the system. You see, for example, the potential

energy <sup>or</sup> the force which acts on this particle, is not something which you specify, but rather something which is determined by the state in which the particle happens to be. If you change the state, you change the force acting upon the particle. But what it means is <sup>that</sup> for every quantum mechanical state you

get a picture in which the particle moves classically, provided a suitable force acts on it. The force always acts on it in such a way as to make it behave statistically according to the laws of

quantum mechanics.

Podolsky: Dr. Guth.

Guth: I think what this equation means along some comparison with classical statistical theory on the scattering process. You assume the conservation of number of particles and that can be expressed by...At this point I would like to add that these equations are incomplete because we reach a boundary condition and single value.

But you can get the boundary condition to be expressed in terms of  $r$  and  $S$  or in terms of  $r$  and  $p$ . These are essentially the quantum mechanical current and  $v$  is essentially the quantum mechanical current, and I think you see that it is completely equivalent to the Schrodinger equation, just in a little different form.

Then we ask the question, which form is the most useful, one consideration or the other consideration? I think the classical picture comes in only if one adds something to the formalism. The classical picture adds something which is really not important to the formalism. If one says that it is a quantum potential, we consider it like a classical elastic or hydrodynamic potential subject to a classical potential. But that might be helpful and might throw some

interesting light, but I would like to express the point that this is completely equivalent mathematically with the Schrodinger equation

Podolsky: Dr. Rosen already said that this is completely equivalent to the Schrodinger equation, that this is just another way of writing it.

Guth: There is nothing classical about it except the looks. You

see the quantum force changes everything completely. Just let me say one word about this question of completeness or incompleteness. You see, classical scattering theory is not a complete theory because it is a statistical theory. The classical scattering theory is not a theory like quantum mechanics. Nothing about statistical theory and classical scattering theory and what not, hidden parameters too. In classical theory we can introduce hidden parameters, in scattering theory we can introduce hidden parameters with which we can measure position and velocity, but it would be to go astray. Nobody as far as I know has even carried out an experiment to show that particular algebra, particular gauge transformation, particular alpha particle...So it seems to me that with the quantum force in classical scattering theory one can introduce as a hidden variable, but there is no point doing it because everything becomes terribly complicated and it is a useless theory. I think I could reproduce something from the pages of this paper.

Rosen: Now, I want to come back to this point for a moment. When I discussed this I was somewhat more cautious, I think, than Bohm. I pointed out that this was a possible way of interpreting quantum mechanics. I also pointed out that there are some difficulties, perhaps, in such an interpretation, and I gave an example of this sort of treatment. One likes to think of classical mechanics as being the limit of quantum mechanics when you let  $h$  go to zero. If you take these equations and let  $h$  go to zero, the first of these equations goes over to the usual Hamilton-Jacobi equation, or at



least it looks as though it does. However, I want to caution you that that will not always be the case in practice. It depends on the nature of the function  $\Psi$  that you have to start with. For example, if you use a free particle and you take  $\Psi$  to be of the form  $Ae^{(i/\hbar)px}$  then this expression  $V_q$  comes out zero. On the other hand, if you take  $\Psi$  to be of the form  $A\cos(px/\hbar)$  then  $V_q$  comes out to be  $p^2/2m$ . Here it is a constant. You see in this

case that it ~~doesn't~~ <sup>does not</sup> go to zero, but it goes to a constant value. We could have more complicated wave functions having the same feature, which gives ~~you~~ <sup>for  $V_q$</sup>  a function of position that does not go to zero when  $\hbar$  goes to zero. ~~As I said, if the  $\hbar$  itself is present in the wave function in a certain way, when differentiating we make it a factor of  $1/\hbar^2$  coming out which cancels the  $\hbar^2$ .~~ <sup>In the case of</sup> Suppose there ~~are certain~~ <sup>are</sup> states for which  $V_q$  does not tend to zero as  $\hbar$  tends to zero, ~~and~~ what one can do is to interpret these states as superpositions of two or more states for which the  $V_q$  does tend to ~~zero.~~ <sup>zero.</sup>

~~do.~~ Well, ~~at any rate,~~ <sup>will not</sup> you see that there are some complications here. I ~~won't~~ go into them because I ~~don't~~ <sup>do not</sup> have enough time here.

However, it does provide a certain rough picture of a classical nature, if one wants such a thing in order to interpret quantum mechanics. I think someone wants to ask a question.

Podolsky says: Oh yes, Dr. von Roos.

von Roos: In my opinion, the difficulties that you have according to the classical limit theory, are due to the fact that  $\Psi$  has an essential singularity as  $\hbar$  goes to zero. But if you do all this,

for instance, with a quantum mechanical distribution function, then there is no trouble in taking this limit.

Rosen: Isn't the last case an example of a quantum mechanical distribution function?

von Roos: No, that's a wave function. That's not a quantum mechanical distribution function.

Wigner: What do you mean, a quantum mechanical distribution function?

von Roos: Well, by <sup>that I mean a</sup> ~~the~~ quantity that <sup>maybe</sup> you derive from the density matrix, for instance. <sup>the Wigner distribution function.</sup>

Wigner: Well.

Rosen: Well, perhaps ~~I will~~ just say one thing about this. You see, in the second case, you can think of the <sup>cosine</sup> ~~causal processes~~ as a linear combination of two exponential functions corresponding to

particles moving in opposite directions, so that you can ~~think~~ <sup>picture a</sup> now of a more complicated sort of distribution where the particles pass through each other. <sup>It is a</sup> ~~It's~~ not the hydrodynamical flow, <sup>since there are</sup> ~~but there are~~ many <sup>more than one velocity at a given point.</sup> ~~superpositions with regard to flow.~~ But then, of course, you ~~cannot~~ <sup>cannot</sup> understand classically how you get this interference between

them that you get here and so...

Furry: This example brings out very clearly that you have ruled out the superposition principle when you impose these reality conditions.

Rosen: Yes.

Furry: Of course, taking a real part is not a linear operator.

Rosen: That <sup>is</sup> right, and in my paper I comment <sup>ed</sup> ~~in this respect~~  
 that ~~it~~ <sup>there</sup> seems to ~~me~~ <sup>be a Compelementarity</sup> ~~an addition to the superposition principle~~

that you can either talk about particles or talk about interference, but not about both at the same time. But I think perhaps I have said enough about this question. I simply brought it in to give an example of what some people might consider to be the introduction of hidden parameters.

Wigner: It doesn't seem to me that von Neumann said that it is not possible to introduce hidden parameters. Surely it is possible, but he said you can't explain the results of the measurements and their statistical nature as a consequence of hidden parameters. And now returning to what Professor Aharonov said, namely, that you did not give a theory of the measurement and therefore, you surely did not, excuse for being so explicit, you surely did not give the explanation of the measurement of the statistical element which, according to everybody, occurs in the course of measurement, as a result of hidden parameters. In principle, it seems to me that this example is saying that we don't have to have the uncertainty principle. (pause) Well, I had a very malicious remark to make.

Furry: Go ahead.

Rosen: The more malicious the better. (laughter)

Wigner: One could say just as well that the velocities always travel with (Wigner seems to say) seven c's. Then the uncertainty principle would be completely abolished. The velocity would always

Correction to page 44:

Rosen: It is true that in this picture there are no particles where the potential  $V_q$  is infinite. It would be worse if it were otherwise, but ....

be absolutely certain. The position would always, of course, be given by quantum mechanics and the velocity would always be (he seems to say)  $77 c$ 's. Well, the uncertainty principle would be a good excuse to me. This abolishes the uncertainty principle in very much the same way. It introduces something that he calls velocity and nobody else will call it velocity. It will be infinite on numerous occasions and it doesn't seem to make very much sense. The potential which he introduces is for a single particle in a stationary field, a time dependent potential, which also has infinities in general and it has infinities where the particle surely is not. It doesn't seem that this is the most

reasonable picture. (gap in record)

Rosen: ...where the potential is infinite, it would be worse the other way around, but...

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by material  
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Wigner: What causes the potential?

Rosen: I want to express again that this is not anything that can have a bearing on the outcome of the measurements beyond what quantum mechanics predicts. It simply enables us to visualize, if one wants

this. Of course, <sup>many</sup> people are happy without having a picture of what <sup>is</sup>

going on in a classical way. If one wants to have a classical picture this, in principle, provides one. Now the uncertainty

principle arises, you might say, as a result of the process of

measurement, not as a result of the behavior of the individual particle itself, whatever that may mean.

Wigner: You cannot imagine a state in which position and velocity are very closely determined. But it doesn't seem that the velocity which is obtained this way has more to do with anything observable than if I say the velocities, but perhaps I should relent and say only point 77 c (0.77c). (laughter)

Rosen: It doesn't matter. This is not a relativistic theory. You can take 7 hundred and 7 c if you want to. (More laughter) I want to say one thing in connection with your remark, Professor Wigner.

Wigner: I'm sure I wouldn't mind.

Rosen: This is simply a way of providing a more complete description within <sup>the</sup> framework of quantum theory. <sup>It is</sup> ~~It is~~ not necessary. One <sup>does not</sup> ~~doesn't~~ have to have it, and perhaps many people <sup>will not</sup> ~~won't~~ like it. But for those who want something of this sort, here it is.

Podolsky: Thank you, Dr. Rosen.

## THE PANEL DISCUSSION

Panelists: Y. Aharonov, W. Furry, B. Podolsky, N. Rosen, and E. P. Wigner.

Professor N. Rosen, Chairman, opening this session Tuesday afternoon, October 2.

Ladies and Gentlemen: As you know, the purpose of this gathering is to have a question and answer session. Dr. Werner has given me a list of questions which were formulated this morning by, I believe, most of the members of this group. We'll just take them one at a time and see what we can do. I understand that we are to refer these questions to the people sitting on the stage. However, if they don't know the answers, we will refer them to the audience, but if they don't know the answers, well (laughter). So let's begin. The first question is as follows: "What is meant by the statement that an operator is observable? How does one distinguish which are observable?"

I'd like to refer this to Dr. Furry, ~~because he is such an expert.~~

Furry: Well, this depends on who is talking. Well, if I use the vernacular, it depends on whom do you string along with. Professor Wigner remarked last night, and I remarked yesterday afternoon, that if you're making a mathematical theory, it's nice to have powerful mathematical weapons. When you make the assertion that every Hermitian operator has a spectrum that can be measured, that is if a set of eigenvalues and eigenfunctions exist for this operator, then you assume that it is measurable and that the possible values obtained in measurement are the eigenvalues. This is what you do if you're interested in powerful mathematical assumption to make it easy to do various deductions. On the other hand, very eminent physicists have taken the position, held strongly to the position, that one should regard as measurable only things for which we can describe, at least in principle, an actual physical arrangement for making the measurement.

And ~~such~~, one finds <sup>such arrangements</sup> in the descriptions that Pauli worked out in the early

part of his handbook article. This adds a little bonus, I might say, for the old custom of learning to read German which was universal among graduate students when I was one, and is not so universal today. These include, of course, position within certain limits, and momentum, energy, angular momentum, and, as Professor Wigner said last night, that's just about the end of the list. I can't think of any case where anyone has worked out a way of measuring anything else. And, of course, it is rather rarely in the

theory that ~~at all~~ <sup>at all</sup> we refer to anything else. That is the practical situation.

When we do physics we talk about position, momentum, energy, angular momentum.

Is there something else? Yes, I guess we measure time. But that comes under a special category. Time, of course, is not an operator in the non-relativistic

quantum mechanics. This is an important distinction. So that our procedure for measuring time is just a procedure for tagging things with a parameter, time being the parameter.

Rosen: "How about energy?"

Furry: "Energy, angular momentum, momentum, and position. These are things which are represented by operators that are genuinely measurable in the sense that people have described them by some experimental arrangement. Now if you arm yourself only with positions, it is much more difficult to prove all the theorems which are proved so rapidly if you arm yourself with more powerful assumptions.

Wigner: How can you measure position?

Furry: Well, with Heisenberg's gamma ray microscope.

Wigner: You don't measure position with that. At what time do you measure position? When you send out the gamma ray, or when it arrives, or in between?

Furry: I would say at a time which is calculated from the time when the gamma ray is sent out, allowing for effects (of transmission).



Wigner: But that is not an operator anymore. That is not an operator because an operator gives  $x$  at time  $t$  equals zero, let us say.

Aharonov: But what about using separating shutters?

Wigner: That comes closer.

Furry: Yes, that is the method Bohr ordinarily used. I was "off the beam" in mentioning this other thing. On the other hand, in connection with that business of measuring with the gamma ray microscope, one should remember that one can plan ahead and send out the rays which will hit the particle in a certain limited region located at a given time. When one did this, of course, one might fail and might not see a particle. There might not be a particle in that position. It's typical of these discussions of experiment that one allows for them to fail frequently and that one agrees that the successful cases will be regarded as typical.

Rosen: Mr. Aharonov.

Aharonov: Well, I just wanted to mention that in the case where one is limited to a small number of operators one might simply measure the energy. If the energy is a sufficiently detailed function of position and momentum, one can measure energy jumps and from the spectrum calculate operators which are functions of energy. So life is not so bad.

Furry: That's right. The single measurement of energy will get you quite a lot of different operators associated with it.

Rosen: Are there questions from the audience?

Dr. Carmi: (questioning from the audience to Professor Wigner) a) What is a measurement apparatus? b) What is the relationship between observables and dynamical invariants of the system? Some people feel that there is much more to this relationship than there appears to be on the surface.

Wigner: Well, I am afraid I am one of those people for several reasons.

When Dr. Furry explained how you measure position, he said that to measure position - well, first he said that with a gamma ray microscope. I think that it is a very useful thing to analyze in detail what you really measure by the gamma ray microscope. But he withdrew from the gamma ray microscope, and I think, with good reason, from the point of view of orthodox measurement theory. Not, of course, from the point of view of really withdrawing from a microscope with a gamma ray, or with visible light or ultraviolet. But then he said, "Let us erect barriers between, so that they separate the space into many regions. Then the electron or a particle will be in one of them, and then we can leisurely investigate in which one it is". Well, now this shows that we convert a position into a stationary state and therefore, what is measured at all with ease are stationary properties. How this point was brought out very much more generally and much more formally (by generally already means much more formally) by an investigation which I hesitate to mention because I embarrass one of the audience, Yanase of Arake and Yanase. They investigated in general what operators can be measured, according to the orthodox theory of quantum measurement, which we heard yesterday from Dr. Furry, and they found that only those operators can be measured without approximation really bona fide which commute with all conserved quantities. Now one of the conserved additive quantities is energy, so that they must be already then stationary quantities. But it is also evident that in a relativistic theory, if it commutes with energy, it will have a very hard time unless it commutes with momentum also. And, of course, in the previous example which Dr. Furry mentioned, namely the measurement of the position, he destroyed the invariants of the system by erecting the barriers. The barriers were supposed to be at rest in one coordinate system but not at rest in other coordinate systems so that this is not really a contradiction,

In fact. Furthermore, it isn't a bona fide measurement because it does not leave the system alone. It changes the system. It changes the wave function very considerably, even the particles. But, let us not go into that. But you see as a result there is both a visualizable connection and a formal connection between the two. There is a visualizable connection in as much as it is very difficult to mention something that is really easily measurable, that is not stationary, let me say. There is also a formal connection because, by an analysis of the general theory of observation which Dr. Furry explained to us yesterday, it does follow that no such measurement is possible, unless the measured quantity is among other things stationary. Now Dr. Furry postulated an interaction between instrument and object and said, "Well, there is such an interaction." However, it is clear that such an interaction must be consistent with the principles of invariance. By analyzing the possible interactions, which are consistent with the principles of invariance, their conclusion was drawn by Araki and Yanase.

Rosens Any other comments on this question? Then we'll go on to the next. The previous question was, "What is meant by the statement that an operator is observable? How does one distinguish which are observable?" The next question is: "Is it justified to make a theory ignoring at the outset questions of the measuring process, and then expect to obtain, by means of that theory, a description of the measurement process?" I would like to refer this question to my colleague, (laughter)

Aharonov: The point of view that measurement theory is something very special seems to me a very subjective point of view. Some people think that action and interaction between human beings and nature is something very specific and very different from other interactions and that, therefore, it should have a specific kind of consideration in the theory. But this is not the

case. There are all kinds of interactions going on all around. In general, interaction takes place when there is no human being around. There are all kinds of interactions going on which define things in the same way as a measurement of these prepared especially by a human being. Therefore, when we extend the theory to describe other things consistently, we eventually hope that these considerations would also be valid for measurement processes in that, after all, only special kinds of interaction take place in nature anyhow. So my answer is, of course, that we don't have to put it in a form where the theory is consistent with any other kind of interaction which is not a measuring process. We believe it should also be valid for consideration of measurement processes.

Podolsky: That assumes, however, that measurement process involves nothing but interaction. But actually it involves a good deal more.

Wigner: But, Doctor, would you consider, would you continue this statement a little bit further?

Podolsky: No, not much, (laughter) This involves reference to the question of reduction of a wave packet. You say at a certain point you read a pointer or something like that. You have the object on which the measurement is performed. You have the measuring instrument. You establish a correlation through interaction at the appropriate time, establish a correlation between what the instrument shows and what the object is doing, or the state of the object. Then you say we read these measurements and ignore the others. As you pointed out, Professor Wigner, we cannot separate the measuring instrument from all the other objects, and so what we are saying is merely that in order to measure something about the electron, we have to measure something about this measuring instrument. Well then, how do we go about measuring that about the measuring instrument? Then we've got another measuring instrument

unless we can somewhere say "Well now, I know what this measuring instrument is doing". But that is an additional assumption.

Wigner: Thank you, that makes that point very clarified.

Aharonov: May I just add my point of view? The idea of the interaction details should be reduction of the wave packet. I think it is inconsistent to say that when there is a special interaction which we call a measurement process, namely, which we expect only when we human beings are coming and looking at the thing, that then it should, collapse suddenly. We really should believe that when we consider a large enough system, independent of the fact that we call it a measurement process, that simply this kind of interaction is going on. There the collapse should happen independent of whether we call it a measurement process or not, or whether we prepare it as a measurement process. So if we find by analysis that there is some difficulty about the reduction of the wave packet, it is a difficulty of the theory as a whole and not only of the measurement process. That's my point. I'm saying that if the theory is consistent independently of the question of measurement theory, it should also answer problems in measurement theory, because measurement theory serves only to point out some special difficulties of the theory because these are independent of the question of measurement.

Podolsky: I don't agree with that and I stick to my previously stated opinion which I don't think is necessary to repeat.

Wigner: Well, let me say something, if you permit me, Mr. Chairman. There are perhaps two points of view on this subject. The one pertaining to - (almost drowned out by laughter) that seems to be a controversial statement! In view of your radical perspective, there is a German physicist, Ludwig, who made use of exactly the point of view of Dr. Aharonov. He says that quantum mechanics is not suited for describing macroscopic objects because, if you

have an interaction with a macroscopic object, this mysterious thing which is called the collapse or contraction of the wave packet takes place under all conditions. Now this is the view of Ludwig and evidently of Dr. Aharonov. I must say that there is another point of view. Ludwig's paper appears in the Heisenberg Festschrift. They evidently have very good security because about two months before the paper appeared I asked Heisenberg what his view was on this question and he had no idea of Ludwig's paper. But he quickly characterized it by a similar description to the one Dr. Podolsky gave. Anyway, the other point of view is that quantum mechanics applies even to macroscopic objects and the collapse of the wave packet takes place (excuse me for the laughter) only through the act of cognition. And this, of course, is an entirely tenable argument - a tenable point of view. It says, if I can place into other words the statement which has been repeated over and over again, that quantum mechanics gives us only probability connections between subsequent impressions or observations or cognitions. Now I never succeeded to find out what Dr. Dirac thinks about it, because he dodges the issue. (laughter) But there are two points of view, and I think we must admit that we don't know with absolute certainty the answer. Is that correct? I agree with Dr. Podolsky's opinion.

Rosen: I'd like to add a few remarks first. I'm a little worried at the use of the word cognition because the human being himself is involved in a particular way in this. I prefer to believe that the physical world is not determined by what we think about it or know about it. If it were a machine rather than a human being which carried out the measurement and recorded the results of the observation, I prefer to believe that the results would be

the same, regardless of whether there ~~would be~~<sup>were</sup> a human being present to ~~watch~~<sup>see</sup> the results, ~~in order to know it took place at all.~~ That's my opinion on the question.

Wigner: It is contrary to the principles of present day quantum mechanics. It may be true, but it is contrary to the principles of present day quantum mechanics.

Rosen: I would like to clarify this. Do you mean to say that if a machine wrote down on a piece of paper the results rather than for a human being to observe it, that it <sup>would</sup> ~~would~~ make any difference in the situation?

Wigner: But the machine would not write it down (according to quantum theory - editor). The piece of paper on which the machine was supposed to write it down would be in a linear combination of two states, with one answer and with the other answer, and therefore the statement that the machine wrote it down is (Wigner struggles to find words) And therefore, the statement that the machine wrote it down is, eh, uh, eh, ... It is very difficult to say things. It's really very difficult to say these things without giving the impression that one, well, is as, uh, uh, orthogonal to the fact - an if an electron is, as if I would say that an electron is either in this state or in that state. If it is actually this state, - .

Rosen: Do you mean you want to treat the electron as a quantum mechanical system and the sheet of paper as a classical system?

Wigner: I think, well, according to the principles of quantum mechanics, the present day principles of quantum mechanics, there is no distinction because both are described by state vectors and not by classical concepts.

Furry: There is an old tradition in the quantum theory of justifying the various statements about what the result of observation might be in cases where they are sometimes very surprising from the classical point of view, by illustrating that the amount of physical intervention in the system involved in the procedures necessary to get the measurement in question, the disturbance of the system is sufficient to produce the given results. This

is the standard argument, of course, for explaining the fact, that an

observable can have for values of results of measurement only its eigenvalues, *and*

these eigenvalues are perhaps quite different from each other, When we prepare the systems exactly the same way, we sometimes get one eigenvalue and sometimes another. This means that the system did not actually have one of these eigenvalues. (At least I hope Professor Rosen will let me finish my considerations before he attacks this.) The orthodox view is that systems do not actually have those values. But it should be possible in all of these cases to show, if we actually examine the amount of intervention necessary to make the measurement, that it was capable of communicating to the system the right amount of this quantity to shift it by amounts comparable to the difference

between eigenvalues, *and* thus we can account for these various results turning

up when we repeat the same experiment identically, under identical conditions several times. Examples of this are well known. I could multiply them. Now it seems to me with regard to this sort of argument, that the original particle, atomic or subatomic, is on a quite different footing from the piece of paper or the counter dial on which the machine records results. We cannot really agree that the amount of intervention we use, namely a flashlight to look at the dial or to look at the paper, is going to be enough actually to disturb physically what is written on the paper or the setting of the counter. In this position, I am sorry Professor Wigner, but I believe that I must align myself with the gentleman on my right (Aharonov).

Podolsky: Well, I would object to that.

Rosen: (chuckles)

Podolsky: (continuing) This may take a minute. I feel some kind of an indication here that if human beings were not mixing into this measurement process, that things would go on just the same as if they were mixing in.



My idea is that if they were not mixing in, you wouldn't have this microphone here, you wouldn't have that recording device, we wouldn't have most of these things if we just left everything to nature.

Aharonov: Suppose that now we go away and all these things are here. What do they do?

Wigner: I think that on the argument Dr. Furry went over on that point, that the instrument can't impart sufficient angular momentum, or whatever it is, there is no question. The question is only "what is the end result of the interaction?" The end result of the interaction, according to quantum mechanics (and again quantum mechanics may not be valid) is not that it is written down on paper with certainty, either of the two answers, but that it is a linear combination of the two and, up to that point, there is no reduction of the wave packet. The wave packet is still there. I could make many examples, but let me read a statement which I happen to have here. Heisenberg made it. "The conception of objective reality evaporating into the mathematics". He says in so many words that there exists a conception of objective reality evaporating. You can't say it much more strongly!

Furry: Well, with all due respect to one of the greatest figures of twentieth century physics, Werner Heisenberg, I would much sooner take your authority, Professor Wigner, to the extent which I have taken it, which everybody can observe (laughter), because I have an opportunity to try to get you to try to explain what you're saying. I can't make him try to explain it. In fact, I think this just reflects some philosophical point of view on the part of Heisenberg with which one might or might not agree. I think there is a real point here, that they think there is a difference between the amount of intervention when we look at a counter, say, and when we look directly at the electron. There is a word which Professor Rosen used repeatedly this morning which I think is a good one in this connection,

and that is the word coherence in respect to these states. I think it is really a question of coherence. I am afraid these remarks are not very mature. They have just been dashed off partly on the bus ride in a conversation with Professors Carmi and Aharonov. You may say that I am just a "reed in the wind", and that Aharonov just influenced me last in what I am saying. (laughter) The question of coherence is really important here, and we have to remember what we mean by coherence. A lot of the books we use are bad on this point. They say the scattering is coherent, for instance, when the frequencies do not change. Well, that is true. In incoherent scattering the frequencies ought to be distinct. But that is not the point. The point in scattering being coherent or incoherent is this. If we have a couple of atoms here and you scatter some waves around them, it is really the following. You have a wave function originally here for the particles, we'll call it  $t$ ; you have a couple of wave functions, say  $U_0$  and  $V_0$ , for these two scatterings. They are probably the same wave function, ground state say. I use different letters because I want to associate one with one atom and one with the other. The initial wave function is this, (he writes on the board) Then the scattering occurs and there is some outgoing wave from each of these. So I have a fancy wave function  $t^1$ ; after the passage of time and scattering has happened (still writing on the blackboard) and there will be two parts. There will be many parts, in fact. There will be one which I might better call  $t_{00}$  and that is the one which is still associated with both the state  $U_0$  for this particle and the state  $V_0$  for the other. This will, of course, contain two actual waves: the one that was scattered out from this one, and the one that was scattered out from this one. Those two waves both have the same functional coefficient, depending on the coordinates of these two things. We can just cancel if we want to and

calculate the relative phase of the two contributions. We can thus get a definite interference. We have a definite phase relation. Now there are other parts, of course. There is  $t_{10}$  which is the function for the electron that has to be multiplied by this one shifted up to the state one and the other one to state zero. Since this one is the particle that's disturbed, we know this is the one that did the scattering. On the other hand there is  $t_{01}$ , which will be a wave more or less coming out from that one, and will have disturbed it. Then you get from  $v_0$  to  $v_1$ . Of course there will be other ones if there are other higher states these things can have. Now the point is, this wave coming off from this one, and that wave coming out from that one will not interfere, because here are different functions of the other variables associated with them and there is no way to assign a phase relation between these two waves. That is then the actual case of coherence. It may be that the state one has exactly the same energy as the state zero. But it's a different wave function and thus one can't say that there's a definite phase relation here. Now it just seems that whenever we

bring in this large scale argument — this large scale phenomenon, of getting it, as far as a pen making a jiggle on a piece of paper, or a counter shifting

its position — that we always use something like, say, the filament in an amplifier tube. We could probably use lots of amplifier tubes, transformers, and what not, and who knows what all. We don't know the position of all those particles. Depending on what's happened, the wave functions of many things have changed. They are put in at random without our knowing much about them, without our knowing about them actually to begin with, the way one actually builds apparatus. It seems to me that there is complete lack of coherence then between the two possible positions of the counter, or between the two possible things the pen may have written on the paper. It

seems that there is no possibility of interference between them, because the wave function that we write then is long and complicated. It always contains quite a number of factors associated with it, depending on which of the two things has happened in the counter or to the pen. In this sense we know the wave function has this form. Because the wave function has this form, even though it is a wave function, it has exactly the same properties, so far as the counter or the piece of paper is concerned, as the mixed state. That is, interference is absolutely impossible and, from, this point of view, one might as well call it a mixed state. Now this argument, of course, did not originate on the bus ride. It has been attempted in various papers. I have never personally been terribly satisfied with it because one can never take the mathematical steps of changing this into an actual density matrix. But I think it should carry a good deal of weight in our attempts to think about it.

Podolsky: Professor Furry, just for completeness, will you give us an example of a coherent state, a coherent case? You have given an example of an incoherent case.

Furry: Well, of course the coherent case never actually comes in precisely in a measurement. The thing we think of in measuring here is finding out which particle scattered it. If there is no change in the state of the scatterer we cannot tell which one scattered it. It is precisely because we can't tell which one scattered it that we can get the interference pattern.

Aharonov: May I just say one more word? First of all, I would like to say that I did not mean to imply that one can get from the usual quantum theory the situation in which we know enough about all the macroscopic things so that we can really say that this is a collapse of the wave packet. We get rid of this, so to say, collapse of the wave packet. I think we can use

quantum theory as such to describe any kind of interaction in any large system with any number of degrees of freedom. If you take it as a closed system you will never get any kind of a collapse, and you will always get all of these possibilities at the same time. Now there are some people who feel that you're not allowed to discuss the case of measurement. You can never put observables in the system that you are considering and therefore you are in a good situation as long as you discuss what you are allowed to by the mathematics. You have no problem because all these possibilities together are true enough that you can leave them as long as you, the observer, don't come and look at it. When you come and look at it then the collapse has occurred, has happened. But an observer is such a complicated thing. It includes all kinds of other things involving biological problems and so on, that we shall never be able to describe by quantum theory. Therefore I doubt that we can treat it as a problem at all. Therefore, I doubt that there exists any problem at all, because as long as you describe things that don't involve the observer there is no necessity for this collapse. If we were to try to describe the observer, we would have to give up from the beginning, because the observers anyhow are too complicated to describe. What I try to say now is that there is a very nice example which Einstein once raised. If you take a radioactive atom and a geiger counter and you let both stay alone, the geiger counter is supposed to make a huge boom when the radioactive atom emits a particle. Now you can think that this huge boom happened even when there was no observer around, therefore, there really should be a wave function of the geiger counter and the atom which should undergo some kind of collapse independently of whether there was an observer in the room to get deaf when this huge boom happened, or not. That's my point of view, - that quantum theory is not complete in the sense that it does



Wigner: No! I think that there is, according to quantum mechanics -

Aharonov interrupts: You're right!

Someone else: Exactly.

Aharonov: According to quantum mechanics --

Rosen: ~~My~~ My question is <sup>whether</sup> ~~that~~ you have a certain cut somewhere <sup>such that</sup> ~~and~~ on the other side of the cut you don't have this situation; <sup>you</sup> you have a classical

system which gives you a definite answer.

Furry: (starts to interrupt)

Rosen: (interrupts the interruption) Now I have a second question which is

this. If <sup>one</sup> ~~you~~ accepts <sup>your</sup> ~~this~~ point of view, ~~that this is so~~, then would you also say that, if an observer is present, <sup>the</sup> ~~that~~ human brain cells also have

two states corresponding to this in a linear combination? In that case, I would like to know where is the decision finally made, (low chuckles in the audience)

Wigner: This is a very pertinent and very disagreeable question. (There is much laughter) Let me say that I agree first of all with Dr. Aharonov. I fully recognize the validity of his point of view. He says quantum mechanics is not valid for such processes and nobody told me that it is valid. I have no special message from anybody which tells me that it is valid. And I also agree with Dr. Furry that it's a very important point. In the case of a complicated system this wave function is, in practice, terribly difficult to distinguish from the mixture of these states. But there are, in principle, methods to distinguish it. I could give examples in simple cases when it really can be distinguished. I can give a general description how it always can be distinguished, but it's not a practical one. Namely, I put a little mirror in front of every particle which reflects it back and then the whole thing runs back in time. Then this state will produce back this state, but

but the mixture of these two states will not produce this state. But as Professor Furry so aptly said, it is awfully difficult to do such a mirror experiment, to put such a mirror in front of every particle. Therefore, in practice the two are not distinguishable. Now whether you therefore say that it is not a wave function, not a linear combination, but a mixture — well, I think this is a matter of taste. It doesn't make any difference whatever if I owe you a hundred dollars or not, because I will deny it anyway. (Much laughter) I will pretend that I don't owe you a hundred dollars. This is a matter of taste or what not. There is no practical difference. I fully agree with Professor Furry that if this is at all complicated there is no practical way to distinguish between linear combinations and mixtures. But if I talk of a mixture, that is along the same line of question as whether I owe you a hundred dollars. So you see, this is not a scientific question but a question of expression.

Furry: There are, in fact, you know, two traditional ways to talk about what we mean by a mixture. And it may not be an accident that Bohm, in his book, does not ordinarily use the one that I used yesterday. He uses the other one. The one I used yesterday is in terms of this density matrix with a bilinear form in wave functions. Bohm prefers usually to talk about a mixed state, but he has only a linear form which specifies that the phases are random. Now if you will accept that as a definition of a mixture, then this, of course, is a mixture as soon as the phases have become random.

Wigner: But they are not random.

Aharonov: (starts to interrupt)

Furry: It depends upon the definition of random, then. Random is --

Wigner: No! (laughter)

Furry: You define random as something that there's no human control over,



and no way at all of having knowledge of.

Wigner: (interrupts) You can say that of the other one also. You wrote here a factor one, not a factor  $i$  or minus one. You wrote here a factor 1, and not a factor  $i$ , which would --

Furry: (interrupts the interruption) But it doesn't matter because the phase of  $v_1$  relative to  $v$  is completely arbitrary.

Wigner: No!

Furry: (keeps going on)

Wigner: (keeps declaring) No! No! No!

Aharonov: (attempts to interject) The  $v_1$  relative to  $u$ .

Wigner: (goes on) No, you told us exactly that  $u_1$ , to  $u_0$  is in the same relation as  $v_1$  to  $v_0$  so that —

Furry: (then interrupts) There is just the same change in energy that a delta function makes.

Wigner: But then you didn't write down the right wave function.

Carmi: (Speaks from the audience) May I just add one word to this discussion? This is a question from Professor Wigner's point of view. Something about spin echo. This is probably the situation which you are trying to —

Wigner: (starts to speak again) Well, I did not think of the spin echo also, but as Dr. Carmi pointed out, the skill of the experimentalists makes it possible to measure something which, up to that time, nobody ever dreamed of measuring. We should not be too quick to decide that it cannot be measured.

Aharonov: That's right. But you agree that when we push it up to something that has written up and down there is probably —

Wigner: I do agree with you, that I don't believe it is possible to bring it back to an interference. Certainly, I have no idea how to bring

it back. But you see, your point of view is terribly dangerous because there is a continuous transition from a very simple system to a very complicated, system. Therefore, if I follow Dr. Aharonov's argument, there is a continuous transition in quantum mechanics between a wave function and a mixture. That is all right. But if I, just on my own, decide that from now on I will call it a mixture, then for somebody else, this is a different story, because I either call it a mixture or a wave function. Furry; Now there appear here, of course, only two other factors, namely, in the case we talk about macroscopically there are not two, there are ten to

*twenty-second,*  
the ~~twentieth-second~~, something of this sort.

Aharonov: But it is all continuous from one to the other.

Furry: Oh yes, you can go continuously if you count all the way from two up to ten to the twenty-first, but -

Aharonov: (interrupts) So the point is that the theory is not very satisfactory. The theory is not telling us when exactly the wave function will collapse. Now it's a question in principle, not only a practical one.

Furry: Something to make the argument interesting. There is a prevailing-climate of feeling that the theory is not satisfactory; that I also am not completely satisfied with the theory, (laughter)

Rosen: I think perhaps I have a question from the floor.

Merzbacher: (speaks from the audience) I think the question has already been answered whether the consistent orthodox - Professor Furry calls it orthodox, I gather - (much laughter)

Furry: (interjects) I am not fully orthodox. My classes never hear a word of this, (a great deal of laughter)

Merzbacher: There is the orthodox interpretation that the Einstein-Podolsky-Rosen paradox, so-called, does not really require us to go so far as Professor Wigner goes. It seems to me that it does.

Rosen: We're coming to a question which deals with this. Perhaps we can go on to that point. Oh, a question from the audience.

Soules: (speaks from the audience) I'm a little bit confused as to how and where we found out that the two scattering atoms were in the states  $u_0$  and  $v_0$  except by just doing what we have already done. Don't we beg the question.

Furry: Oh, no. You can look at it afterwards you see.

Soules: That's what the experiment told us. That they were in the ground state then.

Furry: Before the scattering. Then after the scattering we find that one of them is not in the ground state and we know that it was the one that did the scattering. And since both these last two terms are associated with the situation of the scatterers, which would let us look and see which one did the scattering, then there can't be any interference. This is the statement in words. The statement in mathematics is quite clear that we don't know anything about the phase between the two uv products.

Professor Wigner thinks we do know the relative phase.

Aharonov: (interrupts) Certainly we do, because we could reverse it in time and then if you take -

Furry: (is talking) and now

Wigner: (interrupts) There is a lot in quantum mechanics. I don't think I know everything that is determined by the laws of quantum mechanics. I didn't say I know everything that is determined by the laws of quantum mechanics. I know I don't know everything of that. But it is determined by the laws of quantum mechanics, even if I don't know what it is.

Aharonov: (begins to speak)

Furry: (jumps in) Yes, you would say it is the orthogonality of these

functions, and not any question about their phase fundamentally, that makes the incoherence.

Aharonov: Yes, that's right.

Furry: There is a distinct probability that you are right, (much laughter)

Rosen: Well, I see we have covered one question so far on the list, so perhaps we should go on to the next.

Podolsky: We don't have to answer all the questions, (more laughter)

Rosen: Well, are there any more remarks on this one? One question out of

four it was. All right, then we'll go on to the next. ~~Today~~ <sup>T</sup> The question

asks "How would you formulate what you consider to be the best reply to the arguments of the Einstein-Podolsky-Rosen?" I suppose I should refer this to -

Furry: People who want to reply to it. (laughter)

Wigner: What about Dr. Podolsky?

Podolsky: No, I'm on the wrong side, (more laughter)

Rosen: Before we start answering this question, I would like to make some remarks for the benefit of the team on the other side. In our paper the point that was made is essentially as follows; It was not asserted that quantum mechanics is incorrect. It was only stated that it was believed

that quantum mechanics is incomplete. *The discussion is based on* ~~I think you all got the idea, by~~  
~~carrying out~~ an analysis of the ~~possible proposed~~ <sup>a</sup> situation, in which the  
carrying out of two different kinds of measurement on one ~~system~~ <sup>particle</sup> would give  
two different kinds of information on another ~~system~~ <sup>particle</sup> which would not be in  
interaction with the first one at the time of measurement. ~~In particular,~~  
~~we can do it in such a way that it is possible to decide what the situation~~  
~~should be a little earlier.~~ For example, we could do it on one particle, say,  
and get the coordinate. In fact ~~we~~ <sup>W</sup> could do it in such a way that at one  
time, say, we ~~get the coordinate~~ <sup>determine</sup> indirectly ~~determined for~~ <sup>the coordinate of</sup> the second ~~system,~~ <sup>particle</sup>

*or else its*  
and in the other cases, the momentum, even though these two, of course,  
do not commute, and *it is* ~~its~~ impossible by direct measurements to determine  
them both simultaneously. For that matter, *it is* ~~its~~ impossible by indirect  
measurements to get them both. *However* ~~But~~ we can determine either one or the

other without disturbing the second system for which the information is being  
obtained, since there is no interaction. We can do this since we have the  
wave function that gives the correlation between the two systems in

*X*  
either case. *X* That is the idea involved here. So the question that is really  
being raised is perhaps more a philosophical or metaphysical question, since

*X Insert  
from  
opposite  
page*

it is not something that can be settled by any operational procedure of  
measurement. All right, now let somebody else consider the question.

Aharonov: May I try to give the answer that I think Bohr would give to this -  
what Bohr would say about it? Bohr would say that the problem has come because  
we do something not in a correct way. What we do in the wrong way is to think  
about the two particles, that they are distinctly separate systems, which we  
consider to be quite independent systems. We think about them as existing  
independently of what the experiment is doing on it. We can choose to do one  
experiment and get one kind of result, or to do another experiment and get  
another kind of result. Before it was done we can choose to do one kind of  
experiment. He seems to think of a system such as two electrons separated from  
all the rest of the things that exist in nature. Consider a system of two  
electrons in one environment and consider it one system, and consider a system  
of two electrons in another environment and call it a different system. So then  
if we choose to carry out a measurement on the first particle in one kind of  
environment we put it in one kind of environment, which is a measuring apparatus  
for a position. Together these two electrons with the measuring apparatus we  
call it one kind of system.

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Since either the coordinate or the momentum can be determined without disturbing the system, it is asserted that both of these are elements of physical reality; and since quantum mechanics cannot describe the possession of both of them by the particle, it is asserted that the quantum mechanical description of this reality is incomplete.

If, on the other hand, we have chosen to make an experiment to measure momentum, then this was an entirely different kind of system. It's not the same system we had for the other experiment, but it's an entirely different system. We see, therefore, it's not inconsistent to think that one system has a well defined momentum and another system has a well defined position. These are two different systems which cannot be considered at the same time. Two different systems. He uses the word "complementarity" here to say that these are really two different systems and we can never talk about them in the same context. This is the way that I think Bohr would try to answer it. I'm certainly not saying that this is my answer. I'm just trying to say

Rosen: I would like to add one more remark, ~~as Einstein did,~~ and I hope the orthodox people will correct me. The idea in this interpretation <sup>seems to be</sup> ~~is~~ that

if a system is in a state in which a given physical variable does not have

an exact value, <sup>that is, one</sup> ~~which~~ <sup>is</sup> not an eigenstate, the physical quantity does not exist, does not have reality in that state, <sup>It is</sup> ~~It is~~ only when we carry out a

measurement, when we carry out a determination of some kind, so that one

~~knows the value of it, and then if the system is independent of its environment so that one knows the value of it,~~ then it has reality.

what I think he would say.

Aharonov: To this I think Bohr would say that it's not that the system hasn't a coordinate when it is an uncertain coordinate state, but it's a different system. There is one system with a measuring apparatus for momentum and another system with a measuring apparatus of coordinate. These are two different systems and you can't compare them. It's not that in one case the same system has a coordinate and the other one doesn't have a coordinate, because these are two different systems. He says it's not possible to call it an electron apart from its classical environment ....

Kaiser Kunz: (from the audience) If those two measurements commute, then what are you going to say?

Aharonov: Well, you can say it is true that in the theory you have development of a complete system, and you consider only one at a time. Well, you can say you're making measurements on a complete system and you consider it only in its own environment. If you make measurements in another environment, it's an entirely different system, but this is still a consistent scheme. You can make it two systems. It's a mixture. But it seems to me that the case in which you measure two complementary things are two entirely different systems. You can't call it an electron with a well defined position in one case, and the same electron with a well defined momentum in another case. This is the only consistent way that I know of translating into words what the mathematics of the theory is saying. I'm not saying that this is an acceptable way. I'm saying this is the only consistent way of translating the mathematics into words.

Merzbacher: (speaks from the audience) Would Professor Wigner say that this is the only consistent way to translate the mathematics of quantum theory into words?

Wigner: I think this is an awfully strong statement that it is the only consistent way. I would feel much happier if this very, very strong statement were a little, were not made in --

Aharonov: (interrupts) I should correct it to say that this is the only consistent way that I know.

Furry: Well, that is interesting, because you're a student of Bohm, and I would have thought that Bohm's doctrine of potentialities was also a consistent way.

Aharonov: Well, you see, uh, when Bohm looks at the paradox, he always has trouble. He hasn't solved the paradox yet.



Merzbacher: (exclaims from the audience) Does <sup>not</sup> Professor Wigner's suggestion that the act of cognition collapses the wave packet, actually resolve it

in a much more satisfactory way?

Aharonov: Ah, but then how would you transform the collapse?

If you really look at the system as a quantum mechanical system, then you can do just one measurement, or another measurement. Then you really have to look at this collapse as something that you have done to the system and have just transformed it far away.

Wigner: No, no, I don't think so. This collapse of the wave packet, in my opinion, is only an expression. Well, what is the wave packet good for, is the question which one asks. In my opinion, the wave function has only the one purpose, namely, to calculate the probabilities of future events. And that is the only purpose of the wave function. Now if I look at the wave function as a tool for calculating things, then clearly, if I learn something and some information enters my cognition, from then on I will use a different wave function. This is not even quantum theory. If I pull out, perhaps I should do that, a bill out of my pocket and look at one side and I say I know how the other side looks, from there on my description of this bill will be different. The purpose of the wave function is nothing else. It does not have a mysterious reality. It is only a tool for calculating probabilities for the outcome of events.

Aharonov: The mathematics is entirely clearly satisfactory, I think.

But now, how do we translate it into a picture, to look on the problem in a pictorial way? Namely, I want to think of the electron, not as something mathematical, something to calculate probabilities, but to think of it as some kind of a system. How should we look at it, picture it? That's the point.

Wigner: I don't know. It seems to me that I have a hard time understanding this. Perhaps, if I were very disagreeable, I would say that we should not –

Aharonov: Have a picture of it?

Wigner: I wouldn't say we shouldn't have a picture of it. But we shouldn't elevate the picture to a principle which stops us from thinking. And we should not elevate a particular picture to such a degree which stops us from thinking in terms of quantum physics.

Aharonov: I see.

Wigner: I think that a picture is a wonderful thing for some purposes, but for other purposes it will not work so well.

Aharonov: Yes, but then I think you are not criticizing the picture that Professor Bohr had in mind when he tried to convey this language into pictures. That's the only picture that he is willing to accept, and indeed that's the only picture consistent with quantum physics, I think.

Wigner: Please don't misunderstand me. I'm perfectly willing to concede that you may be right, that quantum mechanics is inaccurate for macroscopic systems, that there is something else. Ludwig, well Ludwig, goes terribly far but there is something along that line that really the accuracy of, or the appropriateness of, quantum mechanics for macroscopic bodies may be questioned.

Aharonov: I'm trying also to ask how is quantum theory visualized if we take quantum theory as it is at present without any changes at all. It still does not give us an exact basis to calculate mathematically. I also want to have some kind of picture in mind. We have said that if we follow Einstein and others at the beginning, we have a picture of some kind of a wave packet. And we interact with it with an apparatus. The wave packet collapses in a measurement of position or expands into an apparatus if it

measures momentum. It's hard to explain, but we can still get used to it - the idea that the apparatus can do all these things. And then, suddenly with this example we can already explain how the apparatus can do this here but not there faster than the speed of light. It's not a consistent or satisfactory picture to see all these things happening without any reason. Right? So then the only picture that I think one can follow reasonably is the one where we say that there is a different system, the electron interacting with one system, one kind of apparatus is an entirely different system from an electron interacting with another apparatus.

That is what I think Bohr is trying to do. And I don't know of any ---

Wigner: Could I go along with Dr. Merzbacher and say this is exaggerated because the electron will not be two kinds of pictures where the electron is different depending on -

Aharonov: (interrupts) Yes, I go along with that too. I don't like it myself, but I don't know of any language that -

Wigner: I don't know either. It is a fact that these wave functions are awfully difficult, relatively difficult to visualize, and what can we do about it?

Band: (speaks from the audience) May we not look at this wave function that Dr. Furry wrote down there as telling us the probability of two alternative events, you might say the scattering from one and the scattering from the other. The probability is referring to a whole series of observations, one observation cannot change this. The wave function is still there to guide the future observations. One observation would tell me I have a scattering from the top particle. This does not change the wave function for following observations.

Furry: We'll re-prepare it.

Band: You have to re-prepare it to give any meaning to the wave function.

Furry: Yes, but if you collect all the observations, then you will get the full pattern and will cause some interference because of that top term (points to the blackboard). Or if you can tell which, it will show a general smear because the two bottom terms will not interfere. Now we can do otherwise. You can collect only those in which subsequently you learn that the top particle is scattered. In that case you would, of course, only get a broad smear here at the top. Where, if you collected only the ones that the bottom one had scattered, you get a smear at the bottom. If you cover both of those in a little region, that's the region you get some interference in the top term when you don't know that it's been scattered.

Band: My point is that for one measurement, just because you find one of these particles has been scattered from the top to the center, this does not mean you should collapse the wave function.

Furry: It means you should correct.

Band: Yes, just correct.

Furry: You could send in a new particle and then, of course, you have the same wave function.

Band: I see no mystery about collapsing of the wave function after you've done something to it.

Furry: Everyone has said that. When we think about what you do when you make the observation finally, you obtain knowledge about the system and there's nothing miraculous. There's nothing more natural than that the formula you write to treat your probability predictions about the system should change when you change your knowledge of it. I don't think it's a real paradox. The essential paradox of Einstein, Podolsky, and Rosen comes, I think, only from the strong temptation that it offers to a number of us.

but not to me actually. I am too orthodox for that. You see really the definition of orthodox is how you're going to spend your time, how you're willing to spend your time, (laughter) Bohm, by the time he finished his book, I'm sure had strong inclinations not to be orthodox. But he remained, orthodox until he finished his book. He then became heterodox because he then began spending his time trying to make a different theory. Well, I never spent my time being unorthodox, (laughter) Well, uh, what was I saying?

Aharonov: You were trying to say there is no difficulty.

Furry: Well, I was saying the difficulty it really raises is not this one. It tempts a person to think that there must be hidden parameters, by George!

(uneasy chuckle in the audience) Because, if you can find out the position or the coordinate, at the same time that you're on one side of the room and the particle is on the other side of the room, you can make either of these measurements on something that you have separated from the particle.

Band: Dr. Furry, some of our group would like you to say over again what you said about the cards before -- the card trick you played on us.

Furry: Oh, I'll play the card trick in a moment.

Band: That is exactly on this line.

Furry: If I could do that, the feeling is that, by George, that particle over there really has a position because I can find it out, if I choose. It also really has a momentum because I can find that out, if I choose, without touching the particle, or without coming near it. Since it really has both, and since quantum mechanics does not allow it really to have both, the theory must be incomplete. But there must be a better theory which contains both as real properties of the particle. Now the danger is the hidden parameters, because they are not visible in quantum mechanics.

Aharonov: How about hidden parameters and your card game?

Furry: The card game has the hidden parameters in it because, by George, the cards are classical objects.

Band: Would you explain that card trick for this audience?

Furry: Well, I explained it pretty fully before when I talked in terms of ordinary playing cards, but now I'll explain it better by providing two decks of cards. All of one pile of cards look the same on the back. Half of them have a red spot on the front side and half of them have a black spot on the front side. Now the other pile of cards is just like it, except they look the same on the back, but half of them have a blue spot on the front, and half of them have a yellow spot on the front. And the spots are good size, you see, so if I tear a card into two halves I'll have part of the spot on each half. So now I have two boxes. Each box has two envelopes, a right-hand envelope, and a left-hand envelope. And now I have Mr. X to do this bit of service for us. Mr. X takes a card from the red-black pile. He can select one or draw it at random. I don't care. He takes a card, tears it in two and puts half of it in the right-hand envelope of each box. He takes a card, from the blue-yellow pile, tears it in two and puts half of it into the left-hand envelope in each of the two boxes. I mean half in the left-hand envelope of one box and half in the left envelope of the other. And then one box is mailed to Chicago. How this is a classical experiment, you see so far. I mean it corresponds to a classical situation, because now I can open this one at my leisure. I can now open both envelopes at my leisure. But these boxes correspond a little more closely to quantum mechanics than that, because each of these boxes is rigged with a little charge of incendiary explosive alongside of each envelope. And each charge is rigged, in such a way that it will explode and burn up its envelope

instantly if the other envelope is removed. That means you can't measure one if you measure the other. Now that's true of both boxes. Now if I look at this box to find out if it's red or black, I'm forever deprived of looking into the box to see if it's blue or yellow, and vice versa. That's also true of the other box which is now in Chicago. Of course, in the meantime, Mr. X has jumped off of the top of a building or out of a window or something. He just corresponds to interaction. (laughter) He just corresponds to the interaction which existed only from time zero up to capital T. So we now have this situation - we don't really need to look at either one of them, in fact. We don't need to look at the right-hand envelope in the Chicago box to find out whether it has red or black in it, if you look at this one. If you look at this one, you'll know it will be the other half of the same card, the same for the blue or yellow. If you do pull out the same one in both boxes, you'll find the same answer. You'll find that they match. If you want to get a complete measurement, you look at one envelope in one box, and the other envelope in the other. But that doesn't have anything to do with this illustration. Now the point I made in discussing this box thing this morning, was that there is no transmission of a signal faster than light or anything like that. Well, if I look at this, say the right-hand envelope, and find red or black, then I can at once say what the same one is in Chicago. The transmission all happens when the box is taken to Chicago. There's nothing about sending a signal, sending information or a signal. We know it just because we know the way these boxes were prepared. The fact that the box was actually prepared in this way is now brought into play, and the same holds true for

these particles. The only thing I think you see, Dr. Aharonov, keeps coming back to the slightly dirty cracks about sending signals faster than light and

(dash)

so on. I do not think there is anything and I do not believe there is anything in this theory. (He pounds the table)

Aharonov: I don't believe it either, of course. That's one way to speak about it.

Furry: It is not right.

Aharonov: And I agree, all through the illustration of the box, for in quantum mechanics we say the particle has a wave function and it may be a perfectly natural way of keeping a record. The information we have about it is due to the notebook that we kept on all that happens, you see.

Band: If you put a half-red in a left-hand box and the other half-red in the other box -

Furry: Half of the card that came out of the red-black pile will go into one of the two envelopes in each of the two boxes, and half of the card that is blue or yellow will go into the other.

Band: How do you know the red half-card is in this box? How do you know that the other half of that one isn't in the other box?

Furry: (declares emphatically) It is!

Band: But why can't you pull that out?

Furry: (exclaims) You can! If you check the same envelope in both boxes you'll always get a consistent result. But you know from the way the thing is set up the results will be consistent.

Band: Oh, you keep them in the envelope No. 1, or the envelope No. 2, and the other half of the card is in the corresponding envelope.

Furry: Right! If the little man does the job for us and then ceases to exist. He took the card and tore it in two, put half of it in one box and half in the other, in the proper envelopes. And for this reason, I know what the color in one is if I look in the other, without needing to look in the other. If I do look, I merely get a check.



Rosen: I think Dr. Soules has a question.

Soules: I was just going to ask, with regard to the paradox we're talking about, is it well established that a state actually exists in which the red --

Furry: This, of course, is just a game. This is a classical example. I have brought it as close to the quantum mechanics as possible with those charges of incendiary. But it is not the proper quantum mechanical case. There really is half of the red or black card and half of the blue or yellow card in the box in Chicago. In the quantum mechanical case that would correspond to saying that the particle that's now over on the other side of the room really has a position and really has a momentum, and I can find out what they are, one or the other of them. And this is denied by wave mechanics, because there is no wave mechanical state that has both precisely defined position and precisely defined momentum. So it's precisely this. You see, in other words, this classical thing I have reeks with the

kind of parameters which are outlawed in quantum mechanics, and driven into

the very dubious and unorthodox phase space of hidden parameters. And Professor Wigner doesn't believe they exist and neither do I. We're orthodox to that extent. Incidentally, the argument he gave last night for disproving them - I deny that it's the von Neumann argument. I think if he rereads chapter six, or whatever chapter it is, or maybe it's chapter four, he will find that it's not the von Neumann argument. It is a better argument than the von Neumann argument because it is not merely mathematical. But it's much more convincing. (laughter) In fact, I think it is much more of a scourge of the infidels (laughter) and I propose to call it the Wigner proof.

Kaiser Kunz: I remember in my elementary work having to work out certain problems involving, let us say, a quadratic equation. I get two solutions.

Then the question is, are they both good or not? We substitute back and find that one of them is an extraneous solution. It seems to me that there is a certain parallel case here of a more sophisticated kind. We're simply saying that quantum mechanics will give us a right or correct solution.

But it gives us too many and ~~that~~ the collapse of the wave function, so to

speak, is that which actually occurs. Whether it occurs during cognition, or whether somehow or another we blame it on the process of measurement that occurs, seems to be the debate. The basic thing seems to be pretty clear. It is that quantum mechanics gives us multiple values, so to speak, and our problem philosophically is, when do we pick the solution. We make it. We

~~just~~ simply force it to agree with what we have observed. So this is the

correct solution.

Furry: If you're positivistic minded enough, there is no problem, there is no trouble. The logical positivists love this..

Podolsky: The question is, really, what is it you do observe and how do we observe it?

Kunz: I think this morning we got even another viewpoint, which is that even the observation doesn't determine which one we really have. Regardless of whether we get the multiple valuedness, it continues on indefinitely.

It seems to me that we all agree that there is multiple valuedness and ~~that~~ quantum mechanics gives it to us. But where we disagree is, if we select <sup>can</sup> out, ~~where it is~~ <sup>one of these values,</sup> and when ~~it is~~ <sup>the selection is made.</sup>

Wigner: It depends also on whether we select out.

Kunz: Yes.

Wigner: Yes it does. Now that is the point of view of Dr. Everett.

Rosen: Would you like to comment, Dr. Everett?

Everett: Yes. Well, what he said pretty much covers it.

Rosen: Well, who else would like to add to this? Professor Podolsky thinks

*it is*  
~~its~~ time perhaps ~~we should~~ have an intermission. What would you like?

So then we'll continue.

Soules: I was going to ask you if my point of view of the photon as a quantum particle is a correct one. It may not be. It seems to me the photon exists only in two states. Either it exists or it does not exist. And the collapse of the wave function is a very natural thing to happen to photons. One finds a photon by killing it and the wave function immediately is annihilated. Is this analogy perfectly fair?

Furry: The trouble is that the wave function doesn't collapse to nothing in these cases that we're talking about, say that of a non-relativistic electron. It collapses down to something we now know about the particle.

Soules: Of course, this is a much more complicated set of states compared to what I'm talking about.

Furry: Yes.

Wigner: I think Professor Werner -

Rosen: Oh, sorry.

Werner: I think the group who met at a Question Workshop this morning would also be interested particularly in hearing what the two members, Rosen and Podolsky, might like to say about their present view on the Einstein, Podolsky, and Rosen question.

Podolsky: Well, I do not agree with Bohr for one thing. We can take the case of two particles that constitute a system of total, say angular momentum, zero.

Wigner: For example, the spin angular momentum.

Podolsky: Oh, yes. They separate. Suppose they were together and then they separate. Then we know the net change in angular momentum will be zero.

Then we know that the angular momentum of one will be opposite to the angular momentum of the other. After they have separated we can bring in the apparatus for measuring the angular momentum on one of them. The other particles, being far away, I don't think should be affected by it. So we can then measure the component of angular momentum in the x direction. Or we can change the apparatus around and measure the component of the angular momentum in the y direction. In each case we will know what the angular momentum of the other particle will be. The x and y components do not commute, so we get back again the same paradox. The whole question, it seems to me, hinges on this: How much reality are we going to attribute to the wave function? If the wave function is merely a statement of our knowledge summarized in some way, well, then, there is nothing wrong with saying that when we find out something about one particle, then we can change the wave function in some way, so that we will know something about the other particle. But if we're going to attribute reality to the wave function, the situation is different. Then by doing something to one particle and its wave function we change the wave function for the other particle. We have a collapse of the wave function, if you like. But that, I think, implies a kind of action-at-a-distance. We do something here, and something else happens some place else instantaneously. This is not the kind of action that you can use to transmit signals -- that the box experiment with cards pretty well establishes -- so there is no contradiction with the theory of relativity. We do not transmit the signal faster than light, but we can change the wave function all over the place instantaneously. Of course, if it doesn't have reality instantaneously, because it doesn't have reality imputed to it, then..... I do not want to assert one or the other. Let's see, there are two possibilities. Either it has reality, in which case we are doing something, uh --

Werner: You mean to say that if it has reality, then we are doing some kind of action at a distance. If it has reality, then something or other happens over there all of a sudden when you are doing something over here.

Podolsky: That's right. Either it has no reality or else we're doing something so that we have an action-at-a-distance.

Band; (interjects) What is --

Podolsky: But which one of those two, I wouldn't commit myself.

Band: What is reality, Mr. Podolsky?

Podolsky: Something more than just subjective information.

Aharonov: Who would like to challenge that? (Podolsky chuckles)

Rosen: Well, I would like to say that I <sup>have not</sup> ~~haven't~~ changed my views in any ~~special~~ way since more than twenty-five years ago when I first started working on <sup>this question.</sup> ~~it.~~ But, mind you, I accept quantum mechanics because we don't have any better theory than that, and it works very nicely, ~~at least in many cases,~~ at least in non-relativistic cases.

Furry; You mean you also teach your students quantum mechanics?

Rosen: Oh, yes. I certainly do. ~~I never discuss these things with them, especially the Einstein-Podolsky-Rosen paradox.~~ I never discuss these things with them, <sup>However,</sup> ~~at least~~ in the elementary course. ~~But~~ when I picture an electron,

I picture it as a particle with position and momentum, even though the wave

function <sup>is not</sup> ~~isn't~~ able to describe these things. Maybe I should <sup>not</sup> ~~do~~ that, <sup>I would</sup> ~~but~~ <sup>interpreting</sup> ~~However,~~ I do. And ~~I~~ like to make the picture consistent by ~~changing~~

quantum mechanics in the following way. Since quantum mechanics gives

probabilities, I <sup>believe</sup> ~~feel~~ that the assertions of quantum mechanics are

assertions that apply not to one particle but to an ensemble of particles, and I would like to say something along the lines that Dr. Furry discussed

yesterday. <sup>I would</sup> ~~I'd~~ like to think of the ~~kind of ensemble which is known as a~~

coherent ensemble that corresponds to a pure state, and an incoherent

ensemble that corresponds to a mixed state. ~~And~~ <sup>Case of a</sup> in the coherent ensemble ~~deal with probabilities, amplitudes, in the case of incoherent ensembles,~~

~~you get probabilities. When we use ensembles, we get probabilities,~~  
<sup>with probabilities.</sup> ~~To begin with, the given particle~~  
~~probabilities of various things, except when a particle is a particle in~~  
~~is in a certain ensemble.~~  
~~the classical way .... (obscured by mumbling, banging, and coughing).~~

However, if we <sup>make</sup> ~~try~~ a measurement on this particle to get information  
about it, and at the same time disturb it <sup>in accordance with</sup> ~~as according to~~ quantum mechanics,  
then <sup>we</sup> ~~one~~ change its state, <sup>and thus</sup> ~~put~~ it into a different ensemble, ~~which one~~

~~needs to make predictions about probabilities.~~ So I would say that I  
<sup>that</sup> still think quantum mechanics is incomplete because <sup>it is</sup> ~~its~~ a statistical theory,  
not a theory of <sup>a</sup> single entity.

Band: One question from our group, Dr. Shimony.

Dr. Shimony: (speaks from the audience) I think much of what I wanted to

ask has been covered, but let me say it anyway because you have thought of  
more things to add <sup>It concerns</sup> ~~to this problem.~~ <sup>may</sup> ~~That is~~ what Professor Wigner  
<sup>what has</sup> ~~is~~ emphasized last night and ~~is~~ come up again today. Even when one discusses

macroscopic objects, there are states that have no classical analog, they are  
states which are superpositions, states in which macroscopic observables have  
different values. And various physicists, Ludwig in particular, have claimed  
that these states are in a sense undetectable. They are undistinguish-able from  
mixtures. Now we know that experiments are devilishly ingenious.

<sup>Ludwig's</sup> So can ~~this~~ view be maintained? Is it really inconceivable that no real  
experiment, not <sup>merely a</sup> ~~the~~ gedanken experiment, can be designed which can detect  
differences on this point. ~~Perhaps all of it has been said.~~

Furry: Well, we've thought about this awfully hard up here. And I must say,  
I am convinced that I was incorrect and that Professor Wigner is entirely  
correct in saying there's no trouble about phases in the little case

of three coordinates I had up here. The reason for the incoherence is actually an orthogonality here and not a question of indeterminate phase. This will remain true, I'm sure, no matter how many coordinate systems there are, no matter how many more coordinate systems I put on. He is entirely correct about all of that. One could, I think, offer in this connection a third way of defining what we mean by a mixed state. We can say that typically the mixed state is defined either as I did it yesterday, with an actual collection of bilinear expressions in the wave functions. Or it's defined with a linear combination, with the prescription that one is to average over all the phases that are completely unknown. One can also say that one has a mixed state whenever one has a linear combination in which independent coordinates or orthogonal wave functions occur in each of the terms so that interference is made impossible by that. Now if one did that, that would justify this idea - which has been so often suggested and never satisfactorily established mathematically -- of what happens to the macroscopic case. So that the reduction of the wave packets, so-called, has practically been accomplished. It merely remains for us, perhaps, to look and see which parts it's collapsed into. However, Professor Wigner made a remark which you have just made also. There's no telling what the experimentalists will learn to measure next. It seems a little hard for me when I think of something that has been done on a photographic plate. And then when this technician reaches up and grabs a large bottle of reagent and slops some into the tray and develops this thing and certain grains get developed. It seems to me that so many new coordinates are brought in and such completely unknown and randomly chosen coordinates in this reagent that determines the development of the grains. Well, really I believe those grains developed whether I looked at them or not, you see, and I'm really too old

to believe in the branching that Mr. Everett believes in, - in the parallel universes of Mr. Everett and things like that. But for instance, if I were to take cosmic rays that come right down through the air of this room rather frequently. They are leaving trails of ionized molecules. The fact that we haven't set up the right conditions of super-saturated vapor to render them visible doesn't mean they aren't really there. But according

to the point of view that *puts* ~~puts~~ all the emphasis on cognition, they aren't

even in the cloud chamber unless you take a picture! (Furry shouts) And they are not even in the cloud chamber or in the picture then unless you look at it! (Furry shouts until Wigner finally speaks again).

Wigner: It is done. It is surely agreed that it is done. We will surely admit that it is done.

Aharonov: (tries to speak)

Furry: I can't go that far, somehow.

Wigner: It is done. If I will surely admit that it is done.

Rosen: Any other comments?

Carmi: I would like to pose the same question from a little bit different angle. Again I would like to ask Professor Wigner about it. What would he say is the quantum mechanical definition of the classical body?

Aharonov: You mean macroscopic.

Carmi: Yes, macroscopic.

Wigner: I might use the example which Professor Furry put forward.

Namely, a classical object is an object which I cannot break into two coherent states and observe that it's in two coherent states. Let me amplify this just a little bit. If I have an electron, and catch it in these two states in which the spin is up and down, I can break it into a state of a linear combination of these two in which the spin is in this direction and I



can afterwards check. In other words, I can prepare a linear combination and then see that it was a definite linear combination, we know that, of course. I can combine it with coefficient 1 and 1, it will be directed this way, directed in this direction. If I take a linear combination with 1 and  $i$  it will be in this direction, and with 1 and  $-1$ , down.

Shimony: This is a most interesting definition, partly because of what it omits. You don't make any reference to number of degrees of freedom in this definition as you have just said it, so that (Wigner begins to interrupt him, but stops.) Shimony continues: It could be that the structure is important.

Wigner: It could be, but I don't know. And in following Professor Furry's thought -

Merzbacher: I don't think you've given the definition quite yet.

Wigner: No, I did not.

Merzbacher: Oh, I don't think you've finished. You haven't referred to the definition yet.

Wigner: No, I did not give a very complete example for the other thing. Now similarly it is clear that if I have a solid body, it could be here, it could be here, it could be here. Or I could make possibly a linear combination of it's being here, and its being here, with a coefficient 1. And I could make a linear combination with a coefficient  $i$ . Now the two would have different properties. If I let it fall on a mirror and reflect it back, they could behave differently. But if I, in practice, am not able to do it so that I can check afterwards that the two coefficients are in the ratio of either 1 or  $i$ , then I would say it is macroscopic. This also means that it is not the body which is macroscopic, but certain properties of it are macroscopic.

Aharonov: What about the fact that if it's here or there? It's only a different phase relation between momentum states.

Wigner: Well, that is true.

Aharonov: I mean because, after all, that's the fact that if you say it can be either here or here, it means you can distinguish between different combinations, linear decompositions of momentum states of the same classical object.

Wigner: Yes, that is true. Let me say again what Dr. Aharonov said, because he said it very fast. "Now surely", Dr. Aharonov said, "you talk foolishly"

Aharonov: (interrupted) I didn't say that.

Wigner: If I had been he, I would have said it - "you talk foolishly". Because the mere fact that the body is here and that we certainly can accomplish, means phase relations between its states of momentum in this direction. Right?

Aharonov: Right.

Wigner: Because the fact that we say that its at point zero means that the different momentum states are in phase, and that is just another expression for this. But you said, and I hope now to say, that the body is not macroscopic, but as I said, it is the property that is macroscopic and the property which is not macroscopic.

Furry: In fact, you said that only some coordinates of it would be macroscopic. You

mentioned position and by this one presumably means the center of mass position. The center of mass, of course, is never a macroscopic coordinate.

You could always reduce it to another Schrodinger equation in that coordinate. The center of mass, even of the moon, you see is a quantum mechanical thing. It is, of course, so heavy that the uncertainty principle doesn't make any trouble with astronomy, but the center of mass --

Wigner: If I follow your definition, I would say it is a macroscopic coordinate, the center of mass, because I cannot make a linear combination with definite coefficients between different positions of the center of mass.

Furry: Oh, yes, you can. It's just a coordinate of this point in space, but there is one factor in the wave function for the whole business, for the whole moon. You can write one which depends on the center of mass only and that's just as definite a factor that wave function has as we would say a hydrogen atom has such a factor.

Wigner: Yes, but it is one position. If I want to put the article here and here, if I want to take something as light as this, - even something as light as this, - and I want to put it here and here with equal probability and establish a phase relation between the two parts of the wave function, then I will not be able to.

Aharonov: Is not your point the following: Make the phase relation between these two positions - you get more definite momentum. Now since the mass is very heavy, the momentum can be different in quite a large amount without affecting the velocity of the particle. Therefore, you can have quite an arbitrary phase relationship between position and still not say very much as far as velocity is concerned. Then we have to wait for a very long time until these two states are really distinguished as far as velocity or later position will be. So it's a problem of how long can you wait and how long can you really isolate the system. And there are all kinds of complicated questions.

Wigner: We have the two states if one waits long enough. You can distinguish them, because the difference is magnified.

Furry: It's being bombarded by all those photons.

Aharonov: Yes. You can have a way.

Wigner: Well, I don't know. I am probably a little out of my depth when I answer that question.

Rosen: We have covered two questions so far. If there are no further comments on them we have just time enough to touch on the next question, which is as follows: "Does the concept of gauge have physical significance? If so, what. If not, why not get rid of it in the mathematical formulation." I think perhaps Dr. Aharonov would like to answer.

Aharonov: Well, I consider the question of gauge. One has to distinguish between the classical electromagnetic theory and the quantum electromagnetic theory. By the way, does the question refer to the gauge of electromagnetic theory?

Rosen: It doesn't say.

Aharonov: Well, there's a gauge in general relativity theory. Let us stick to electromagnetic theory. First of all, when you consider the gauge in classical electromagnetic theory it will disclose invariance under gauge transformation. On the other hand, it's also true that if one wants to discuss the theory in canonical formalism, namely to introduce the Hamiltonian and so on, one has to use the potentials, and therefore, one gets the problem of gauge. The question - why do you use it, why don't you get rid of it, even there is a matter of convenience. If, for example, it's more convenient you might choose to describe the theory in a canonical formalism, or to use the theory in canonical formalism, and therefore to use some things which are not actually observable. To the question of why don't you get rid of these things, well, it's a matter of convenience. We don't just get rid of potentials just to avoid something that we use in classical theory, because the formalism with potentials is more - well, I think it's

more convenient to handle. When one comes to quantum theory, as long as one discusses c-number gauge transformations -- by this I mean that if you change potentials only by well defined classical numbers -- then the same story is still true. One can make such a gauge transformation and not change any observable consequences of the theory. Again the gauge is more convenient to use with potentials because of the reasons that I gave before. But then, there is something new here in quantum theory because one can describe quantum gauges. Namely, one can describe a situation in which potentials are not exactly defined even though they don't correspond to electric or magnetic fields. They are correlated with different kinds of quantum operators. And in this case I want to get to make a point that there is some new significance of these quantum fluctuations of potentials, some new theories of quantum fluctuations of potentials. But I think these theories are now being formulated and we don't know how far one can get. There is only some indication that there are some new possibilities. Perhaps there is some new information for interaction. So to conclude, I would say the following: As far as classical electromagnetic theory is concerned, it is nearly impossible to discover a theory in which the gauge and electromagnetic potentials of the theory are physically necessary. They are just for convenience in classical questions of calculating interaction. Now when you get to quantum theory, there are some other problems. It's still an open question how far one can go.

Rosen: Any further comments on this question?

Wohlkopf: (speaks from the audience) Even if one accepts the fact that potentials are very useful in the description of an electromagnetic system, one might ask the question, "Exist there mathematical quantities in which the potentials are uniquely given so that the equation of a gauge transformation doesn't even enter the picture?"

Rosen: (to Aharonov) Do you want to answer this too?

Aharonov: Yes. I do not think there already exists such a case because when you go into quantum theory one can take any classical well defined function, and perform a gauge transformation, and no observable will be changed. So, therefore, I don't believe that one has a theory where the potentials are defined uniquely completely. Maybe from the measurements in the laboratory this distinction will apply. I hope that I have answered your question. So that is the case as far as I know it

Rosen: Even if you impose the Lorentz condition on the potentials, there is still a possibility of a gauge transformation of a restricted kind. And when the gauge function satisfies the wave equation that's as much restriction as one can impose upon it. Any other questions or comments? If not, I think our time is about up. I would like to thank the audience for its patience all through this discussion.

End of Tuesday afternoon Panel Discussion.

# THE EVOLUTION OF THE PHYSICIST'S PICTURE OF NATURE

by P. A. M. DIRAC

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# The Evolution of the Physicist's Picture of Nature

*An account of how physical theory has developed in the past and how, in the light of this development, it can perhaps be expected to develop in the future*

by P. A. M. Dirac

In this article I should like to discuss the development of general physical theory: how it developed in the past and how one may expect it to develop in the future. One can look on this continual development as a process of evolution, a process that has been going on for several centuries.

The first main step in this process of evolution was brought about by Newton. Before Newton, people looked on the world as being essentially two-dimensional—the two dimensions in which one can walk about—and the up-and-down dimension seemed to be something essentially different. Newton showed how one can look on the up-and-down direction as being symmetrical with the other two directions, by bringing in gravitational forces and showing how they take their place in physical theory. One can say that Newton enabled us to pass from a picture with two-dimensional symmetry to a picture with three-dimensional symmetry.

Einstein made another step in the same direction, showing how one can pass from a picture with three-dimensional symmetry to a picture with four-dimensional symmetry. Einstein brought in time and showed how it plays a role that is in many ways symmetrical with the three space dimensions. However, this symmetry is not quite perfect. With

Einstein's picture one is led to think of the world from a four-dimensional point of view, but the four dimensions are not completely symmetrical. There are some directions in the four-dimensional picture that are different from others: directions that are called null directions, along which a ray of light can move; hence the four-dimensional picture is not completely symmetrical. Still, there is a great deal of symmetry among the four dimensions. The only lack of symmetry, so far as concerns the equations of physics, is in the appearance of a minus sign in the equations with respect to the time dimension as compared with the three space dimensions [see top equation on page 8].

We have, then, the development from the three-dimensional picture of the world to the four-dimensional picture. The reader will probably not be happy with this situation, because the world still appears three-dimensional to his consciousness. How can one bring this appearance into the four-dimensional picture that Einstein requires the physicist to have?

What appears to our consciousness is really a three-dimensional section of the four-dimensional picture. We must take a three-dimensional section to give us what appears to our consciousness at one time; at a later time we shall have a

different three-dimensional section. The task of the physicist consists largely of relating events in one of these sections to events in another section referring to a later time. Thus the picture with four-dimensional symmetry does not give us the whole situation. This becomes particularly important when one takes into account the developments that have been brought about by quantum theory. Quantum theory has taught us that we have to take the process of observation into account, and observations usually require us to bring in the three-dimensional sections of the four-dimensional picture of the universe.

The special theory of relativity, which Einstein introduced, requires us to put all the laws of physics into a form that displays four-dimensional symmetry. But when we use these laws to get results about observations, we have to bring in something additional to the four-dimensional symmetry, namely the three-dimensional sections that describe our consciousness of the universe at a certain time.

Einstein made another most important contribution to the development of our physical picture: he put forward the general theory of relativity, which requires us to suppose that the space of physics is curved. Before this physicists



had always worked with a flat space, the three-dimensional flat space of Newton which was then extended to the four-dimensional flat space of special relativity. General relativity made a really important contribution to the evolution of our physical picture by requiring us to go over to curved space. The general requirements of this theory mean that all the laws of physics can be formulated in curved four-dimensional space, and that they show symmetry among the four dimensions. But again, when we want to bring in observations, as we must if we look at things from the point of view of quantum theory, we have to refer to a section of this four-dimensional space. With the four-dimensional space curved, any section that we make in it also has to be curved, because in general we cannot give a meaning to a flat section in a curved space. This leads us to a picture in which we have to take curved three-dimensional sections in the curved four-dimensional space and discuss observations in these sections. During the past few years people have been trying to apply quantum ideas to

gravitation as well as to the other phenomena of physics, and this has led to a rather unexpected development, namely that when one looks at gravitational theory from the point of view of the sections, one finds that there are some degrees of freedom that drop out of the theory. The gravitational field is a tensor field with 10 components. One finds that six of the components are adequate for describing everything of physical importance and the other four can be dropped out of the equations. One cannot, however, pick out the six important components from the complete set of 10 in any way that does not destroy the four-dimensional symmetry. Thus if one insists on preserving four-dimensional symmetry in the equations, one cannot adapt the theory of gravitation to a discussion of measurements in the way quantum theory requires without being forced to a more complicated description than is needed by the physical situation. This result has led me to doubt how fundamental the four-dimensional requirement in physics is. A few decades ago it seemed quite certain that one had

to express the whole of physics in four-dimensional form. But now it seems that four-dimensional symmetry is not of such overriding importance, since the description of nature sometimes gets simplified when one departs from it.

Now I should like to proceed to the developments that have been brought about by quantum theory. Quantum theory is the discussion of very small things, and it has formed the main subject of physics for the past 60 years. During this period physicists have been amassing quite a lot of experimental information and developing a theory to correspond to it, and this combination of theory and experiment has led to important developments in the physicist's picture of the world.

The quantum first made its appearance when Planck discovered the need to suppose that the energy of electromagnetic waves can exist only in multiples of a certain unit, depending on the frequency of the waves, in order to explain the law of black-body radiation. Then Einstein discovered the same unit of energy occurring in the photoelectric effect. In this early work on quantum theory one simply had to accept the unit of energy without being able to incorporate it into a physical picture.

The first new picture that appeared was Bohr's picture of the atom. It was a picture in which we had electrons moving about in certain well-defined orbits and occasionally making a jump from one orbit to another. We could not picture how the jump took place. We just had to accept it as a kind of discontinuity. Bohr's picture of the atom worked only for special examples, essentially when there was only one electron that was of importance for the problem under consideration. Thus the picture was an incomplete and primitive one.

The big advance in the quantum theory came in 1925, with the discovery of quantum mechanics. This advance was brought about independently by two men, Heisenberg first and Schrodinger soon afterward, working from different points of view. Heisenberg worked keeping close to the experimental evidence about spectra that was being amassed at that time, and he found out how the experimental information could be fitted into a scheme that is now known as matrix mechanics. All the experimental data of spectroscopy fitted beautifully into the scheme of matrix mechanics, and this led to quite a different picture of the atomic world. Schrodinger worked from a more mathematical point of view, trying to find a beautiful theory for describ-

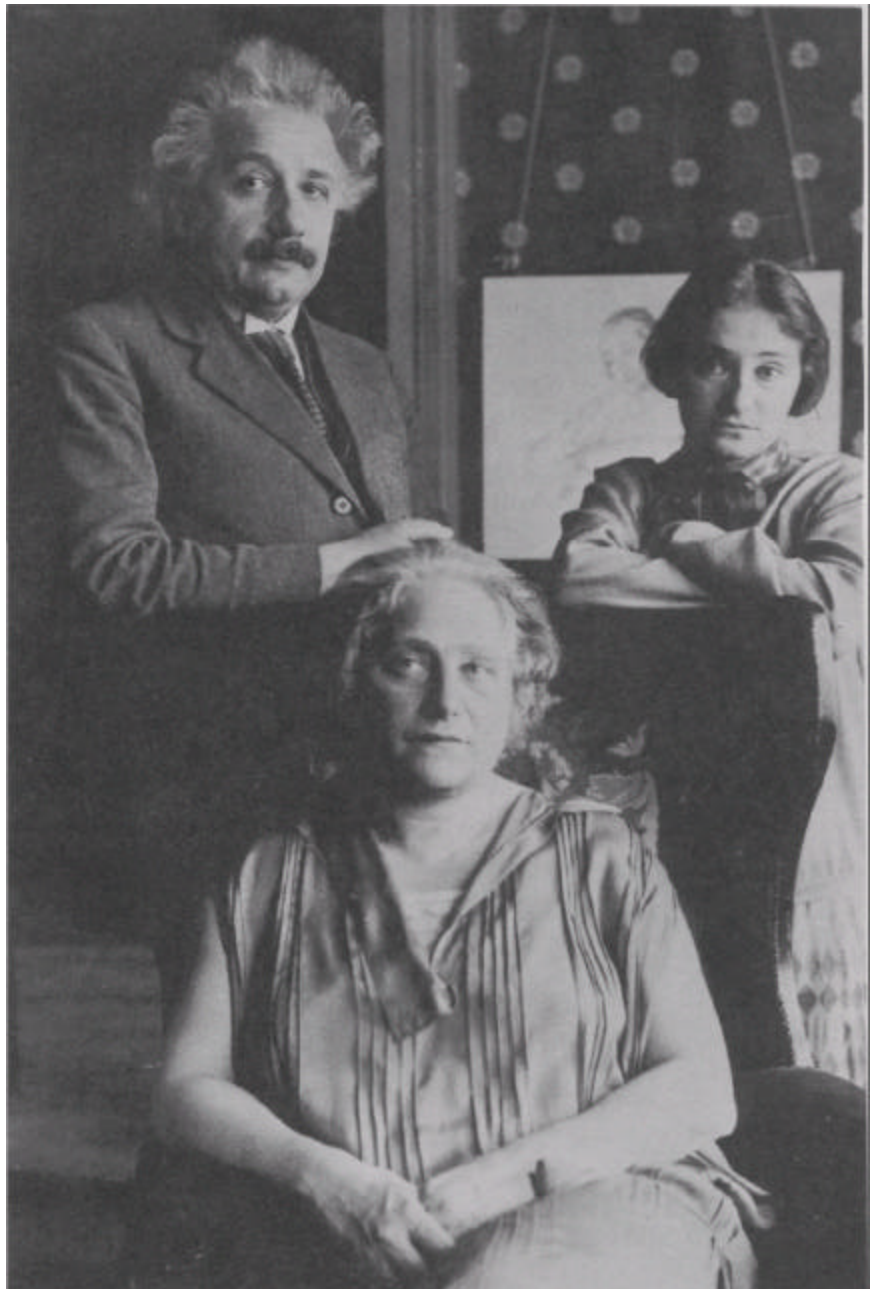


**ISAAC NEWTON (1642-1727), with his law of gravitation, changed the physicist's picture of nature from one with two-dimensional symmetry to one with three-dimensional symmetry. This drawing of him was made in 1760 by James Macardel from a painting by Enoch Seeman.**

ing atomic events, and was helped by De Broglie's ideas of waves associated with particles. He was able to extend De Broglie's ideas and to get a very beautiful equation, known as Schrodinger's wave equation, for describing atomic processes. Schrodinger got this equation by pure thought, looking for some beautiful generalization of De Broglie's ideas, and not by keeping close to the experimental development of the subject in the way Heisenberg did.

I might tell you the story I heard from Schrodinger of how, when he first got the idea for this equation, he immediately applied it to the behavior of the electron in the hydrogen atom, and then he got results that did not agree with experiment. The disagreement arose because at that time it was not known that the electron has a spin. That, of course, was a great disappointment to Schrodinger, and it caused him to abandon the work for some months. Then he noticed that if he applied the theory in a more approximate way, not taking into account the refinements required by relativity, to this rough approximation his work was in agreement with observation. He published his first paper with only this rough approximation, and in that way Schrodinger's wave equation was presented to the world. Afterward, of course, when people found out how to take into account correctly the spin of the electron, the discrepancy between the results of applying Schrodinger's relativistic equation and the experiments was completely cleared up.

I think there is a moral to this story, namely that it is more important to have beauty in one's equations than to have them fit experiment. If Schrodinger had been more confident of his work, he could have published it some months earlier, and he could have published a more accurate equation. That equation is now known as the Klein-Gordon equation, although it was really discovered by Schrodinger, and in fact was discovered by Schrodinger before he discovered his nonrelativistic treatment of the hydrogen atom. It seems that if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one's work and experiment, one should not allow oneself to be too discouraged, because the discrepancy may well be due to minor features that are not properly taken into account and that will get cleared up with further developments of the theory.



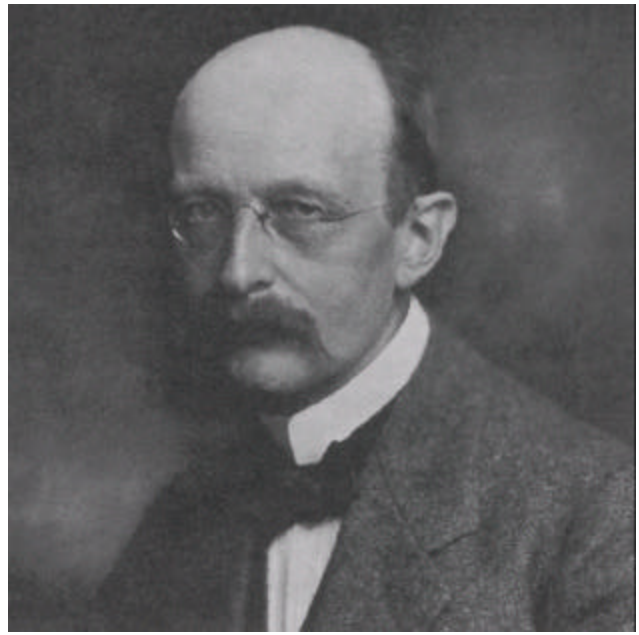
**ALBERT EINSTEIN (1879-1955), with his special theory of relativity, changed the physicist's picture from one with three-dimensional symmetry to one with four-dimensional symmetry. This photograph of him and his wife and their daughter Margot was made in 1929.**

That is how quantum mechanics was discovered. It led to a drastic change in the physicist's picture of the world, perhaps the biggest that has yet taken place. This change comes from our having to give up the deterministic picture we had always taken for granted. We are led to a theory that does not predict with certainty what is going to happen in the future but gives us information only about the probability of occurrence of various events. This giving up of determinacy has been a very controversial subject, and some people do not like it at all. Einstein in particular never liked it.

Although Einstein was one of the great contributors to the development of quantum mechanics, he still was always rather hostile to the form that quantum mechanics evolved into during his lifetime and that it still retains. The hostility some people have to the giving up of the deterministic picture can be centered on a much discussed paper by Einstein, Podolsky and Rosen dealing with the difficulty one has in forming a consistent picture that still gives results according to the rules of quantum mechanics. The rules of quantum mechanics are quite definite. People



**NIELS BOHR (1885-1962)** introduced the idea that the electron moved about the nucleus in well-defined orbits. This photograph was made in 1922, nine years after the publication of his paper.



**MAX PLANCK (1858-1947)** introduced the idea that electromagnetic radiation consists of quanta, or particles. This photograph was made in 1913, 13 years after his original paper

know how to calculate results and how to compare the results of their calculations with experiment. Everyone is agreed on the formalism. It works so well that nobody can afford to disagree with it. But still the picture that we are to set up behind this formalism is a subject of controversy.

I should like to suggest that one not worry too much about this controversy. I feel very strongly that the stage physics has reached at the present day is not the final stage. It is just one stage in the evolution of our picture of nature, and we should expect this process of evolution to continue in the future, as biological evolution continues into the future. The present stage of physical theory is merely a steppingstone toward the better stages we shall have in the future. One can be quite sure that there will be better stages simply because of the difficulties that occur in the physics of today.

I should now like to dwell a bit on the difficulties in the physics of the present day. The reader who is not an expert in the subject might get the idea that because of all these difficulties physical theory is in pretty poor shape and that the quantum theory is not much good. I should like to correct this impression by saying that quantum theory is an extremely good theory. It gives wonderful agreement with observation over a wide range of phenomena. There is no doubt that it is a good theory, and the only reason physicists talk so much about

the difficulties in it is that it is precisely the difficulties that are interesting. The successes of the theory are all taken for granted. One does not get anywhere simply by going over the successes again and again, whereas by talking over the difficulties people can hope to make some progress.

The difficulties in quantum theory are of two kinds. I might call them Class One difficulties and Class Two difficulties. Class One difficulties are the difficulties I have already mentioned: How can one form a consistent picture behind the rules for the present quantum theory? These Class One difficulties do not really worry the physicist. If the physicist knows how to calculate results and compare them with experiment, he is quite happy if the results agree with his experiments, and that is all he needs. It is only the philosopher, wanting to have a satisfying description of nature, who is bothered by Class One difficulties.

There are, in addition to the Class One difficulties, the Class Two difficulties, which stem from the fact that the present laws of quantum theory are not always adequate to give any results. If one pushes the laws to extreme conditions—to phenomena involving very high energies or very small distances—one sometimes gets results that are ambiguous or not really sensible at all. Then it is clear that one has reached the limits of application of the theory and that some further development is needed. The Class Two difficulties are important even for

the physicist, because they put a limitation on how far he can use the rules of quantum theory to get results comparable with experiment.

I should like to say a little more about the Class One difficulties. I feel that one should not be bothered with them too much, because they are difficulties that refer to the present stage in the development of our physical picture and are almost certain to change with future development. There is one strong reason, I think, why one can be quite confident that these difficulties will change. There are some fundamental constants in nature: the charge on the electron (designated  $e$ ), Planck's constant divided by  $2\pi$  (designated  $\hbar$ ) and the velocity of light ( $c$ ). From these fundamental constants one can construct a number that has no dimensions: the number  $\hbar c/e^2$ . That number is found by experiment to have the value 137, or something very close to 137. Now, there is no known reason why it should have this value rather than some other number. Various people have put forward ideas about it, but there is no accepted theory. Still, one can be fairly sure that someday physicists will solve the problem and explain why the number has this value. There will be a physics in the future that works when  $\hbar c/e^2$  has the value 137 and that will not work when it has any other value.

The physics of the future, of course, cannot have the three quantities  $\hbar$ ,  $e$  and  $c$  all as fundamental quantities. Only two

of them can be fundamental, and the third must be derived from those two. It is almost certain that  $c$  will be one of the two fundamental ones. The velocity of light,  $c$ , is so important in the four-dimensional picture, and it plays such a fundamental role in the special theory of relativity, correlating our units of space and time, that it has to be fundamental. Then we are faced with the fact that of the two quantities  $\hbar$  and  $e$ , one will be fundamental and one will be derived. If  $\hbar$  is fundamental,  $e$  will have to be explained in some way in terms of the square root of  $\hbar$ , and it seems most unlikely that any fundamental theory can give  $e$  in terms of a square root, since square roots do not occur in basic equations. It is much more likely that  $e$  will be the fundamental quantity and that  $\hbar$  will be explained in terms of  $e^2$ . Then there will be no square root in the basic equations. I think one is on safe ground if one makes the guess that in the physical picture we shall have at some future stage  $e$  and  $c$  will be fundamental quantities and  $\hbar$  will be derived.

If  $\hbar$  is a derived quantity instead of a fundamental one, our whole set of ideas about uncertainty will be altered:  $\hbar$  is the fundamental quantity that occurs in the Heisenberg uncertainty relation connecting the amount of uncertainty in a position and in a momentum. This uncertainty relation cannot play a fundamental role in a theory in which  $\hbar$  itself is not a fundamental quantity. I think one can make a safe guess that uncertainty relations in their present form will not survive in the physics of the future.

Of course there will not be a return to the determinism of classical physical theory. Evolution does not go backward. It will have to go forward. There will have to be some new development that is quite unexpected, that we cannot make a guess about, which will take us still further from classical ideas but which will alter completely the discussion of uncertainty relations. And when this new development occurs, people will find it all rather futile to have had so much of a discussion on the role of observation in the theory, because they will have then a much better point of view from which to look at things. So I shall say that if we can find a way to describe the uncertainty relations and the indeterminacy of present quantum mechanics that is satisfying to our philosophical ideas, we can count ourselves lucky. But if we cannot find such a way, it is nothing to be really disturbed about. We simply have to take into account that we are at a transitional stage

and that perhaps it is quite impossible to get a satisfactory picture for this stage.

I have disposed of the Class One difficulties by saying that they are really not so important, that if one can make progress with them one can count oneself lucky, and that if one cannot it is nothing to be genuinely disturbed about. The Class Two difficulties are the really serious ones. They arise primarily from the fact that when we apply our quantum theory to fields in the way we have to if we are to make it agree with special relativity, interpreting it in terms of the three-dimensional sections I have mentioned, we have equations that at first look all right. But when one tries to solve them, one finds that they do not have any solutions. At this point we ought to say that we do not have a theory. But physicists are very ingenious about it, and they have found a way to make progress in spite of this obstacle. They find that when they try to solve the equations, the trouble is that certain quantities that ought to be finite are actually infinite. One gets integrals that diverge instead of converging to something definite. Physicists have found that there is a

way to handle these infinities according to certain rules, which makes it possible to get definite results. This method is known as the renormalization method,

I shall merely explain the idea in words. We start out with a theory involving equations. In these equations there occur certain parameters: the charge of the electron,  $e$ , the mass of the electron,  $m$ , and things of a similar nature. One then finds that these quantities, which appear in the original equations, are not equal to the measured values of the charge and the mass of the electron. The measured values differ from these by certain correcting terms— $\Delta e$ ,  $\Delta m$  and so on—so that the total charge is  $e + \Delta e$  and the total mass  $m + \Delta m$ . These changes in charge and mass are brought about through the interaction of our elementary particle with other things. Then one says that  $e + \Delta e$  and  $m + \Delta m$ , being the observed things, are the important things. The original  $e$  and  $m$  are just mathematical parameters; they are unobservable and therefore just tools one can discard when one has got far enough to bring in the things that one can com-



**LOUIS DE BROGLIE (1892- )** put forward the idea that particles are associated with waves. This photograph was made in 1929, five years after the appearance of his paper.

pare with observation. This would be a quite correct way to proceed if  $\Delta e$  and  $\Delta m$  were small (or even if they were not so small but finite) corrections. According to the actual theory, however,  $\Delta e$  and  $\Delta m$  are infinitely great. In spite of that fact one can still use the formalism and get results in terms of  $e + \Delta e$  and  $m + \Delta m$ , which one can interpret by saying that the original  $e$  and  $m$  have to be minus infinity of a suitable amount to compensate for the  $\Delta e$  and  $\Delta m$  that are infinitely great. One can use the theory to get results that can be compared with experiment, in particular for electrodynamics. The surprising thing is that in the case of electrodynamics one gets results that are in extremely good agreement with experiment. The agreement applies to many significant figures—the kind of accuracy that previously one had only in astronomy. It is because of this good agreement that physicists do attach some value to the renormalization theory, in spite of its illogical character.

It seems to be quite impossible to put this theory on a mathematically sound basis. At one time physical theory was all built on mathematics that was inherently

sound. I do not say that physicists always use sound mathematics; they often use unsound steps in their calculations. But previously when they did so it was simply because of, one might say, laziness. They wanted to get results as quickly as possible without doing unnecessary work. It was always possible for the pure mathematician to come along and make the theory sound by bringing in further steps, and perhaps by introducing quite a lot of cumbersome notation and other things that are desirable from a mathematical point of view in order to get everything expressed rigorously but do not contribute to the physical ideas. The earlier mathematics could always be made sound in that way, but in the renormalization theory we have a theory that has defied all the attempts of the mathematician to make it sound. I am inclined to suspect that the renormalization theory is something that will not survive in the future, and that the remarkable agreement between its results and experiment should be looked on as a fluke.

This is perhaps not altogether surprising, because there have been similar flukes in the past. In fact, Bohr's elec-

tron-orbit theory was found to give very good agreement with observation as long as one confined oneself to one-electron problems. I think people will now say that this agreement was a fluke, because the basic ideas of Bohr's orbit theory have been superseded by something radically different. I believe the successes of the renormalization theory will be on the same footing as the successes of the Bohr orbit theory applied to one-electron problems.

The renormalization theory has removed some of these Class Two difficulties, if one can accept the illogical character of discarding infinities, but it does not remove all of them. There are a good many problems left over concerning particles other than those that come into electrodynamics: the new particles—mesons of various kinds and neutrinos. There the theory is still in a primitive stage. It is fairly certain that there will have to be drastic changes in our fundamental ideas before these problems can be solved.

One of the problems is the one I have already mentioned about accounting for the number 137. Other problems are how to introduce the fundamental length to physics in some natural way, how to explain the ratios of the masses of the elementary particles and how to explain their other properties. I believe separate ideas will be needed to solve these distinct problems and that they will be solved one at a time through successive stages in the future evolution of physics. At this point I find myself in disagreement with most physicists. They are inclined to think one master idea will be discovered that will solve all these problems together. I think it is asking too much to hope that anyone will be able to solve all these problems together. One should separate them one from another as much as possible and try to tackle them separately. And I believe the future development of physics will consist of solving them one at a time, and that after any one of them has been solved there will still be a great mystery about how to attack further ones.

I might perhaps discuss some ideas I have had about how one can possibly attack some of these problems. None of these ideas has been worked out very far, and I do not have much hope for any one of them. But I think they are worth mentioning briefly.

One of these ideas is to introduce something corresponding to the luminiferous ether, which was so popular among the physicists of the 19th century. I said earlier that physics does not evolve back-

$$ds^2 = c^2 dt^2 - dx^2 - dy^2 - dz^2$$

**FOUR-DIMENSIONAL SYMMETRY** introduced by the special theory of relativity is not quite perfect. This equation is the expression for the invariant distance in four-dimensional space-time. The symbol  $s$  is the invariant distance;  $c$ , the speed of light;  $t$ , time;  $x$ ,  $y$  and  $z$ , the three spatial dimensions. The  $d$ 's are differentials. The lack of complete symmetry lies in the fact that the contribution from the time direction ( $c^2 dt^2$ ) does not have the same sign as the contributions from the three spatial directions ( $- dx^2$ ,  $- dy^2$  and  $- dz^2$ ).

$$\left( \frac{ih}{2\pi c} \frac{\partial}{\partial t} + \frac{e^2}{cr} \right) \psi = \left[ m^2 c^2 - \frac{h^2}{4\pi^2} \left( \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} \right) \right] \psi$$

**SCHRODINGER'S FIRST WAVE EQUATION** did not fit experimental results because it did not take into account the spin of the electron, which was not known at the time. The equation is a generalization of De Broglie's equation for the motion of a free electron. The symbol  $e$  represents the charge on the electron;  $i$ , the square root of minus one;  $h$ , Planck's constant;  $r$ , the distance from the nucleus;  $\psi$ , Schrodinger's wave function;  $m$ , the mass of the electron. The symbols resembling sixes turned backward are partial derivatives.

$$\left( E + \frac{e^2}{r} \right) \psi = - \frac{h^2}{8\pi^2 m} \left( \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} \right) \psi$$

**SCHRODINGER'S SECOND WAVE EQUATION** is an approximation to the original equation, which does not take into account the refinements that are required by relativity.

ward. When I talk about reintroducing the ether, I do not mean to go back to the picture of the ether that one had in the 19th century, but I do mean to introduce a new picture of the ether that will conform to our present ideas of quantum theory. The objection to the old idea of the ether was that if you suppose it to be a fluid filling up the whole of space, in any place it has a definite velocity, which destroys the four-dimensional symmetry required by Einstein's special principle of relativity. Einstein's special relativity killed this idea of the ether.

But with our present quantum theory we no longer have to attach a definite velocity to any given physical thing, because the velocity is subject to uncertainty relations. The smaller the mass of the thing we are interested in, the more important are the uncertainty relations. Now, the ether will certainly have very little mass, so that uncertainty relations for it will be extremely important. The velocity of the ether at some particular place should therefore not be pictured as definite, because it will be subject to uncertainty relations and so may be anything over a wide range of values. In that way one can get over the difficulties of reconciling the existence of an ether with the special theory of relativity.

There is one important change this will make in our picture of a vacuum. We would like to think of a vacuum as a region in which we have complete symmetry between the four dimensions of space-time as required by special relativity. If there is an ether subject to uncertainty relations, it will not be possible to have this symmetry accurately. We can suppose that the velocity of the ether is equally likely to be anything within a wide range of values that would give the symmetry only approximately. We cannot in any precise way proceed to the limit of allowing all values for the velocity between plus and minus the velocity of light, which we would have to do in order to make the symmetry accurate. Thus the vacuum becomes a state that is unattainable. I do not think that this is a physical objection to the theory. It would mean that the vacuum is a state we can approach very closely. There is no limit as to how closely we can approach it, but we can never attain it. I believe that would be quite satisfactory to the experimental physicist. It would, however, mean a departure from the notion of the vacuum that we have in the quantum theory, where we start off with the vacuum state having exactly the symmetry required by special relativity.

That is one idea for the development of physics in the future that would



**ERWIN SCHRODINGER (1887-1961)** devised his wave equation by extending De Broglie's idea that waves are associated with particles to the electrons moving around the nucleus. This photograph was made in 1929, four years after he had published his second equation.

change our picture of the vacuum, but change it in a way that is not unacceptable to the experimental physicist. It has proved difficult to continue with the theory, because one would need to set up mathematically the uncertainty relations for the ether and so far some satisfactory theory along these lines has not been discovered. If it could be developed satisfactorily, it would give rise to a new kind of field in physical theory, which might help in explaining some of the elementary particles.

Another possible picture I should like to mention concerns the question of why all the electric charges that are observed in nature should be multiples of one elementary unit,  $e$ . Why does one not have a continuous distribution of charge occurring in nature? The picture I propose goes back to the idea of Faraday lines of force and involves a development of this idea. The Faraday

lines of force are a way of picturing electric fields. If we have an electric field in any region of space, then according to Faraday we can draw a set of lines that have the direction of the electric field. The closeness of the lines to one another gives a measure of the strength of the field—they are close where the field is strong and less close where the field is weak. The Faraday lines of force give us a good picture of the electric field in

classical theory.

When we go over to quantum theory, we bring a kind of discreteness into our basic picture. We can suppose that the continuous distribution of Faraday lines of force that we have in the classical picture is replaced by just a few discrete lines of force with no lines of force between them.

Now, the lines of force in the Faraday picture end where there are charges. Therefore with these quantized Faraday lines of force it would be reasonable to

suppose the charge associated with each line, which has to lie at the end if the line of force has an end, is always the same (apart from its sign), and is always just the electronic charge,  $-e$  or  $+e$ . This leads us to a picture of discrete Faraday lines of force, each associated with a charge,  $-e$  or  $+e$ . There is a direction attached to each line, so that the ends of a line that has two ends are not the same, and there is a charge  $+e$  at one end and a charge  $-e$  at the other. We may have lines of force extending to infinity, of course, and then there is no charge.

If we suppose that these discrete Faraday lines of force are something basic in physics and lie at the bottom of our picture of the electromagnetic field, we shall have an explanation of why charges always occur in multiples of  $e$ . This happens because if we have any particle with some lines of force ending on it, the number of these lines must be a whole number. In that way we get a picture that is qualitatively quite reasonable.

We suppose these lines of force can

move about. Some of them, forming closed loops or simply extending from minus infinity to infinity, will correspond to electromagnetic waves. Others will have ends, and the ends of these lines will be the charges. We may have a line of force sometimes breaking. When that happens, we have two ends appearing, and there must be charges at the two ends. This process—the breaking of a line of force—would be the picture for the creation of an electron ( $e^-$ ) and a positron ( $e^+$ ). It would be quite a reasonable picture, and if one could develop it, it would provide a theory in which  $e$  appears as a basic quantity. I have not yet found any reasonable system of equations of motion for these lines of force, and so I just put forward the idea as a possible physical picture we might have in the future.

There is one very attractive feature in this picture. It will quite alter the discussion of renormalization. The renormalization we have in our present quantum electrodynamics comes from starting off with what people call a bare electron—an electron without a charge

on it. At a certain stage in the theory one brings in the charge and puts it on the electron, thereby making the electron interact with the electromagnetic field. This brings a perturbation into the equations and causes a change in the mass of the electron, the  $\Delta m$ , which is to be added to the previous mass of the electron. The procedure is rather roundabout because it starts off with the unphysical concept of the bare electron. Probably in the improved physical picture we shall have in the future the bare electron will not exist at all.

Now, that state of affairs is just what we have with the discrete lines of force. We can picture the lines of force as strings, and then the electron in the picture is the end of a string. The string itself is the Coulomb force around the electron. A bare electron means an electron without the Coulomb force around it. That is inconceivable with this picture, just as it is inconceivable to think of the end of a piece of string without thinking of the string itself. This, I think, is the kind of way in which we should try to develop our physical picture—to bring in ideas that make inconceivable the things we do not want to have. Again we have a picture that looks reasonable, but I have not found the proper equations for developing it.

I might mention a third picture with which I have been dealing lately. It involves departing from the picture of the electron as a point and thinking of it as a kind of sphere with a finite size. Of course, it is really quite an old idea to picture the electron as a sphere, but previously one had the difficulty of discussing a sphere that is subject to acceleration and to irregular motion. It will get distorted, and how is one to deal with the distortions? I propose that one should allow the electron to have, in general, an arbitrary shape and size. There will be some shapes and sizes in which it has less energy than in others, and it will tend to assume a spherical shape with a certain size in which the electron has the least energy.

This picture of the extended electron has been stimulated by the discovery of the mu meson, or muon, one of the new particles of physics. The muon has the surprising property of being almost identical with the electron except in one particular, namely, its mass is some 200 times greater than the mass of the electron. Apart from this disparity in mass the muon is remarkably similar to the electron, having, to an extremely high degree of accuracy, the same spin and the same magnetic moment in proportion to its mass as the electron does. This



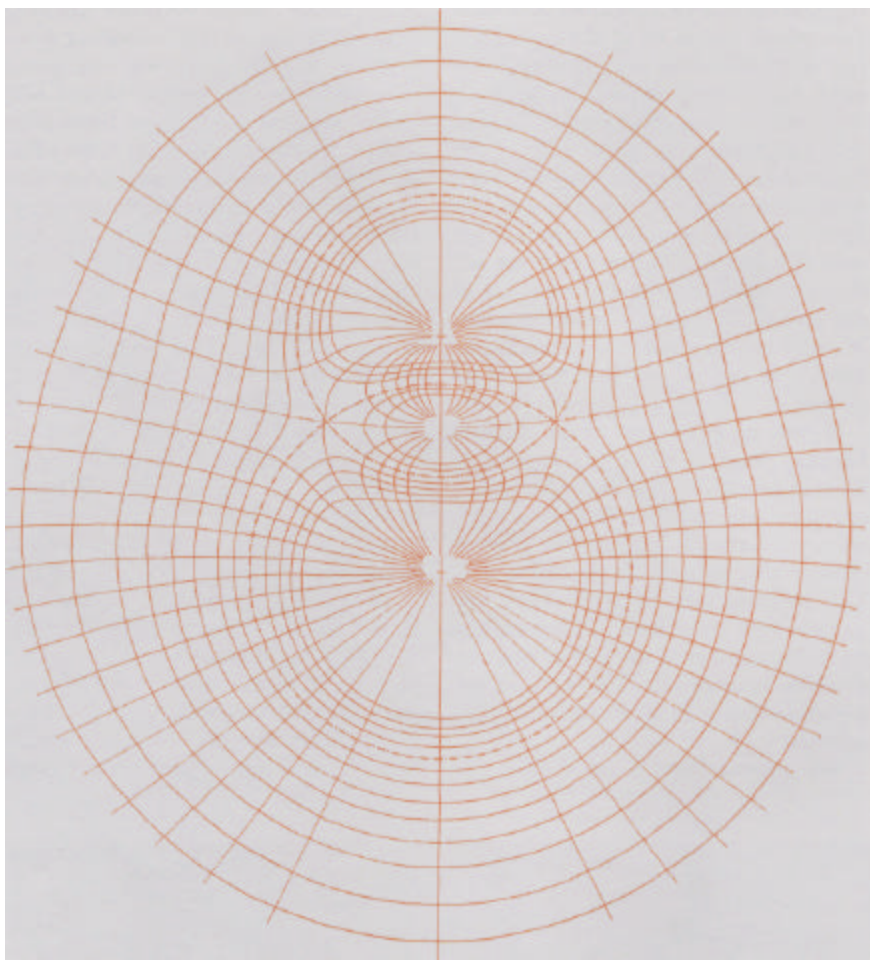
**WERNER HEISENBERG (1901- )** introduced matrix mechanics, which, like the Schrödinger theory, accounted for the motions of the electron. This photograph was made in 1929.

leads to the suggestion that the muon should be looked on as an excited electron. If the electron is a point, picturing how it can be excited becomes quite awkward. But if the electron is the most stable state for an object of finite size, the muon might just be the next most stable state in which the object undergoes a kind of oscillation. That is an idea I have been working on recently. There are difficulties in the development of this idea, in particular the difficulty of bringing in the correct spin.

I have mentioned three possible ways in which one might think of developing our physical picture. No doubt there will be others that other people will think of. One hopes that sooner or later someone will find an idea that really fits and leads to a big development. I am rather pessimistic about it and am inclined to think none of them will be good enough. The future evolution of basic physics—that is to say, a development that will really solve one of the fundamental problems, such as bringing in the fundamental length or calculating the ratio of the masses—may require some much more drastic change in our physical picture. This would mean that in our present attempts to think of a new physical picture we are setting our imaginations to work in terms of inadequate physical concepts. If that is really the case, how can we hope to make progress in the future?

There is one other line along which one can still proceed by theoretical means. It seems to be one of the fundamental features of nature that fundamental physical laws are described in terms of a mathematical theory of great beauty and power, needing quite a high standard of mathematics for one to understand it. You may wonder: Why is nature constructed along these lines? One can only answer that our present knowledge seems to show that nature is so constructed. We simply have to accept it. One could perhaps describe the situation by saying that God is a mathematician of a very high order, and He used very advanced mathematics in constructing the universe. Our feeble attempts at mathematics enable us to understand a bit of the universe, and as we proceed to develop higher and higher mathematics we can hope to understand the universe better.

This view provides us with another way in which we can hope to make advances in our theories. Just by studying mathematics we can hope to make a guess at the kind of mathematics that will come into the physics of the future.



**LINES OF FORCE** in an electromagnetic field, if they are assumed to be discrete in the quantum theory, suggest why electric charges always occur in multiples of the charge of the electron. In Dirac's view, when a line of force has two ends, there is a particle with charge  $-e$ , perhaps an electron, at one end and a particle with charge  $+e$ , perhaps a positron, at the other end. When a closed line of force is broken, an electron-positron pair materializes.

A good many people are working on the mathematical basis of quantum theory, trying to understand the theory better and to make it more powerful and more beautiful. If someone can hit on the right lines along which to make this development, it may lead to a future advance in which people will first discover the equations and then, after examining them, gradually learn how to apply them. To some extent that corresponds with the line of development that occurred with Schrodinger's discovery of his wave equation. Schrodinger discovered the equation simply by looking for an equation with mathematical beauty. When the equation was first discovered, people saw that it fitted in certain ways, but the general principles according to which one should apply it were worked out only some two or three years later. It may well be that the next advance in physics will come about along these lines: people first discovering the equa-

tions and then needing a few years of development in order to find the physical ideas behind the equations. My own belief is that this is a more likely line of progress than trying to guess at physical pictures.

Of course, it may be that even this line of progress will fail, and then the only line left is the experimental one. Experimental physicists are continuing their work quite independently of theory, collecting a vast storehouse of information. Sooner or later there will be a new Heisenberg who will be able to pick out the important features of this information and see how to use them in a way similar to that in which Heisenberg used the experimental knowledge of spectra to build his matrix mechanics. It is inevitable that physics will develop ultimately along these lines, but we may have to wait quite a long time if people do not get bright ideas for developing the theoretical side.



## The Author

P. A. M. DIRAC ("The Evolution of the Physicist's Picture of Nature") is Lucasian Professor of Mathematics at the University of Cambridge. His article is based on a lecture given at the Conference on the Foundations of Quantum Mechanics, which took place at Xavier University in Cincinnati in October of last year. The editors especially wish to thank John B. Hart, chairman of the department of physics at Xavier University, for his assistance in preparing the article for publication. In 1928 Dirac proposed his theory of the electron, which led him to predict, three years later, the existence of an antiparticle of the electron. The antiparticle, or positron, was discovered in 1932 by C. D. Anderson of the California Institute of Technology. For this work Dirac shared (with the Austrian theoretical physicist Erwin Schrodinger) the 1933 Nobel prize for physics. He was elected Fellow of the Royal Society in 1930.

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Wed. P.M. - 1

Conference: October 1-5, 1962

Wednesday Afternoon - October 3, 1962

Supplement  
by P.A.M. Dirac

Dirac: I shall talk about a classical model of the electron that has a finite size. It will be assumed to have a definite boundary surface on which all the charge is concentrated. In a relativistic theory there is a difficulty in attaching a definite shape (e.g. a spherical shape) and a definite size to the electron, because when the electron is accelerating, the concepts of shape and size are not well defined, unless one brings in artificial constraints. So I shall assume that the shape and size are variable, i.e., the electron is deformable.

We then have to postulate a new force to hold the electron together, otherwise it would fly apart under the Coulomb repulsion of its surface charge. The simplest assumption for the new force is that it is of the nature of a surface tension. This assumption can easily be formulated relativistically.

Pages 12 to 26  
rearranged by  
Dirac

Conference: October 1-5, 1962

Wednesday Afternoon - October 3, 1962

~~Professor Dirac~~ <sup>P</sup> This model, of course, has the great defect that there is no spin in it. <sup>I'm afraid</sup> ~~I suppose~~ this is necessary because it is just a preliminary attempt. <sup>P</sup> ~~Before we can develop~~ <sup>for</sup> ~~ing~~

this theory, we shall want, of course, to apply quantum ideas to it. We start off with a classical picture to which we can apply quantum ideas later on. That means we must have a Hamiltonian, or, in more general terms, we must have an action principle. So I take it as essential that we should have an

action principle for the motion of this electron. <sup>P</sup> By an action principle I mean one comprehensive action principle, such that applying it gives us all the equations of motion that we want, namely, field equations for the field outside the electron together with the equations of motion for the electron as a whole, and equations of motion telling us how the shape and size

of the electron changes. All these equations should come out from one comprehensive action principle. <sup>P</sup> Let us now try to set up such an action principle. ~~That means if I write an action principle by expressing the variation of this action integral, it will give us all these equations which we want.~~ Let us assume the total action consists of an action integral over the space outside the electron,  <sup>$I_0$ , plus a surface integral  $I_s$ ,</sup> ~~or I should like to write this~~  

$$I = I_0 + I_s$$
~~where  $I_0$  is an action integral over the space outside the electron.~~ We take ~~it~~  <sup>$I_0$</sup>  to be just the usual action

integral for the Maxwell field, because we want to get simply the Maxwell equations outside <sup>the electron.</sup> So this is  $(-\frac{1}{4} f_{\mu\nu} f^{\mu\nu})$ . <sup>factor  $1/4\pi$  for brevity.</sup> I'm leaving out the ~~two pi's which were not put in,~~ <sup>it</sup> ~~which will~~ be necessary to put <sup>it</sup> in later when one does the numerical work,

but ~~which~~ <sup>it</sup> one can drop out from ~~this~~ <sup>The</sup> general theory. ~~They are~~ <sup>It is</sup> put in, in the printed paper actually. ~~This~~ <sup>\* P The integral  $I_0$</sup>  is taken over the

region of space outside the electron. I should say that we are

considering only one electron. The theory applies <sup>also</sup> to several electrons provided they never collide or come into contact with each other. If we do formulate the equations for one electron, then the same work will formulate the equations for several electrons provided they don't collide. So it is sufficient

for us to confine our attention just to a single electron. <sup>P</sup>  $I_0$

is one part of the action integral,  $I_s$  is another part of the action integral, which is the surface integral taken over the surface of the electron. This has to bring in the surface tension term. The precise form of this I will leave unspecified

for the present, and will deal with it later. <sup>P</sup> Now at this stage

it becomes important to understand exactly how an action principle works. Let us put it in general terms like this.

~~A classical action principle is the one we are considering,~~  
~~where~~ We have some action integral,  $I$ , ~~as~~ a function of certain variables,  $q$ , which specify the physical conditions throughout

space-time. What we do is to vary the  $q$ 's and get  $\delta I$  <sup>for particular</sup>  
<sup>delta q's</sup> when we vary the  $q$ 's. The conditions must be such that  $\delta I$   
 $I$  is linear in the  $\delta q$ 's.  $\delta I$  is ~~some coefficient~~ <sup>of the form</sup>  
 $C_n \delta q_n$ , summed over all the  $q$ 's in the case of discrete

$q$ 's. Then we put these coefficients  $c_n$  equal to zero and get a set of equations that are the equations of motion which

follow from the action principle. <sup>P</sup> It is important here that we must choose the  $q$ 's such that  $\Delta I$  is a linear function of the  $\Delta q$ 's. If we are careless in doing that, we <sup>may</sup> have  $\Delta I$  not

\* Proc. Roy. Soc. A 268, 57 (1962)

a linear function of the  $\Delta q$ 's and then the action principle just doesn't work at all.

Wigner: No matter what function of  $q$  you take, the  $\Delta I$  is a first order expression, accurate to first order in  $\Delta q$ , so that it is by definition linear.

Dirac: People usually think so, but that is rather sloppy thinking and doesn't hold when one goes into it closely. Let us apply this action integral to these ideas here. What do we take as our  $q$ 's? Here is our extended electron (he indicates a circle on the board). Well, we can take as some of our  $q$ 's all the potentials outside.

Wigner: As functions of what?

Dirac: As functions of a system of four coordinates. The

$A_\mu$ 's as functions of the coordinates,  $x$ , will be some of the  $q$ 's. Then we shall need some further  $q$ 's. ~~These further  $q$ 's specify a surface, and we will need some further  $q$ 's~~ to specify

the surface.

Wigner: The  $q$ 's specify the surface variables?

Dirac: The  $q$ 's <sup>complete set of</sup> must specify the surface and also the field outside the surface, because we <sup>must</sup> have a comprehensive action

principle.

Wigner: The  $q$ 's specify the surface, as a result of your assumptions, namely, that the electric field is parallel to the surface but there is no electromagnetic force acting on the surface itself.

Dirac: The things that you are saying now should come out as consequences of the action principle. They are not the starting point. The starting point is that we must have an  $I$  as a

function of certain  $q$ 's, and then we proceed to vary the  $q$ 's.

Now what are the  $q$ 's? That is the question you have been asking. The  $q$ 's must be sufficient to describe the physical state throughout space-time.

Podolsky: Isn't it the  $q$ 's and their derivatives that have to be adequate to describe state?

Dirac: Well, the  $q$ 's will be the set of things which will describe the physical conditions throughout all space-time.

Wigner: Excuse me Doctor, but it seems to me that's at least not clear to me. The  $q$ 's will describe not only the surface, but also the outside so that there will be only  $q$  variables.

Dirac: All the variables in the action principle are  $q$ 's.

Wigner: Are  $q$ 's, ah!

Dirac: We have one comprehensive action principle which must give all the equations of motion.

Wigner: That's right, well right, but whether you denote them all by  $q$  is another question.

Dirac: Well in this discussion here, I have just a single variable,  $q$ , to denote all the physical variables entering into  $I$ , and these will be a set of  $q$ 's. We shall need some further  $q$ 's to specify the surface. The easiest way to do that will be to introduce a parametrization of the surface. You can introduce things which are not physically meaningful if you want to; they don't disturb the action principle. Then we may specify the positions in space-time of points on the

surface; ~~we're specifying~~ <sup>labelled by</sup> parameters, and we can take ~~those~~ <sup>these</sup> ~~things~~ <sup>positions</sup> as  $q$ 's. Then we will have enough  $q$ 's to specify the

physical conditions completely. We could express our  $I$  in

terms of those  $q$ 's, and then we could consider how  $I$  varies

when we vary these  $q$ 's. Then we see that ~~the~~  $I$  does not vary

linearly when we vary the  $q$ 's, because we might consider a variation of the  $q$ 's in which the surface is pushed out a little bit like that, (he draws a bump on the circle) then consider a second variation of the  $q$ 's which is just minus the first variation of the  $q$ 's, corresponding to the surface being pushed in. It will be a reflection of the first one pushing out (he draws an inward bump). Now the  $\Delta I$  in the second case will not be minus the  $\Delta I$  in the first case because the field here in this hump is not the same as the field in this depression. The change that we make in  $I$  when we stick a hump like that onto our surface is just not minus the change, which we make in  $I$  when we put a depression in the surface like that, and those  $q$ 's, therefore, will not work.

Wigner: We are not able to say, within first order of the  $q$ 's?

Dirac: Not even within the first order of the  $q$ 's, no.

Wigner: Because that is the usual situation.

Dirac: That is the usual situation, but it doesn't apply here because the integral taken over the hump is not the same as the integral taken over the depression, even to the first order. In fact, the integral taken over this depressed region is zero.

Wigner: Integral over, what do you call the first region?

Dirac: I call this the first region here. (He points to the bump.)

Wigner: Integral of what over the depressed region?

Dirac: I call this region the depressed region. (He points to the indentation.)

Wigner: You said the integral. What is the integrand?

Dirac: The integrand will be this. (He points to the black-board.)

*on*  
~~the term~~  $-\frac{1}{4} f_{\mu\nu} f^{\mu\nu}$

Furry: There seems also to be a little difficulty in using these surface coordinates as q's along with the A's.

Dirac: I'm not saying this is the only difficulty; there are others as well.

Furry: Because when you change the surface you change what points x there are at which to specify the A's outside. You change some of the q's and affect how many of the other q's there are.

Dirac: Yes, I agree that is also a difficulty. I don't want

to assert that ~~this is the only difficulty, but this is certainly a devastating difficulty.~~  
*The difficulty I talked about is the only one but it*  
*one*  
 difficulty.

Schwebel: May I ask a question with regard to the relative size? This would be the relative size, because if you had it larger than your proposed minimum size, you could have the...

Dirac: No, this is not the minimum size, this is an arbitrary size for the particle. We must have the action principle working for an arbitrary state. In fact, there's just no

minimum size. ~~This is the electromagnetic case.~~  
*We are dealing with*

Schwarzschild minimum size in it.

Aharonov: The theory assumes that there is no reason to describe the inside of this surface here. You obtain this well defined expression. It's just a kind of constraint, if you like.

Dirac: You can bring them in if you like, but the electro-



magnetic field is zero inside. You can bring them in, there's no harm in doing it, but it's...

Aharonov: No, I mean you don't assume that there can be any other thing...going on in the inside.

Dirac: I'm taking this simple model where the field is zero inside, and where the whole of the action consists of this outside action and this surface action.

Wigner: I understand why you say the  $I_0$  change is not oppositely equal if you push it out the surface and if you pull it in. It would seem that if you push it out, you abolish the electromagnetic field within the...

Dirac: Within this region. Shall I draw a bigger picture here?

Wigner: Yes, you abolish it within that region.

Dirac: That's right.

Wigner: So suppose the field was electric, then you decrease the total energy. (Again he says) You decrease the total energy.

Dirac, in the middle of this says: Yes, within the electron, yes.

Wigner: When you pull it in?

Dirac: Yes

Wigner: You will create that field and...

Dirac: You mustn't create that field. You mustn't change these A's. You must change the parameters which specify the surface and not change anything else.

Wigner: Well, that is not possible.

Dirac: Well I should have quite a bit of difficulty if that

is not possible. Why do you say it is not possible?

Aharonov: It is not possible according to the equation of motion.

Dirac: But we haven't got any equations of motion yet.

Aharonov: Yeah, that's the trouble.

Furry: It's not possible because you abolish some of the variables outside when you push out this surface.

Dirac: Let us have these A's defined throughout space-time and perhaps simplify the discussion.

Wigner: Well, you say the reason for this is, if I understand it right, because you want to define some of the q's as functions of x's where the x is a definite point in space-time.

Dirac: Yes, yes.

Wigner: That is what you want.

Dirac: Yes.

Wigner: And this is what makes the definition of q and, let us say, the radius, impossible if you want this kind of equation.

Dirac: No, it is not impossible.

Wigner: Well, it will not be linear outside.

Dirac:  $\Delta I$  is not linear here. Yes, that's what I am saying.

Wigner: Yes, if you use as some of the variables  ~~$A_\mu$~~  <sup>*The  $A_\mu(x)$*</sup>  of ~~x's~~ with x a definite point in space-time, with the other q's some parameters which describes the surface. Is that the statement?

Because that, I think, we could understand. Suppose the definition of the q's is impossible. Some of the q's are the  $A_\mu(x)$ 's where  $\mu$  is of course 1, 2, 3, 4, and x is a definite point in space-time.

Dirac: Yes.

Wigner: If all the  $q$ 's are some parameters which determine the position of the surface, this kind of  $q$ 's is not possible because if you do this, then indeed,  $\Delta I$  would not be linear.

Dirac: In the  $\Delta q$ 's.

Wigner: In the  $\Delta q$ 's.

Dirac: Precisely correct. Yes.

Wigner: I understand. Thank you.

Carmi: Excuse me, I still don't understand, because it seems to me that these two sets of variables are dependent on each other by their definition.

Wigner: That's just the trouble. The definition does not want them to be dependent on each other. It wants them to be independent and if they are not actually independent of each other, one obtains difficulty. One assumes they are independent and works that way with the action principle.

Dirac: You want to start off with the action principle in terms of  $q$ 's which are independent of one another, and which you will vary independently and then equate the coefficients to zero and get the equations of motion.

Carmi: What, then, is your definition of those parts of the  $q$ 's which make the  $A$ 's.

Furry: Well, perhaps this is one of the difficulties that Dirac mentions.

Dirac: There may be other difficulties as well.

Wigner: Yes. But I think part of the trouble is that most

people are not used to the fact that the  $A_{\mu}(x)$  <sup>are</sup> ~~is~~ the  $q$ 's. In

other words, that there is a continuum of  $q$ 's. But that is

just what we must have here.

Dirac: If you just have any action principle for a field theory it has to be like that.

Wigner: Well, that's, of course, evident.

Dirac: Well, we must choose our  $q$ 's differently and, so far as I know, the only way of choosing  $q$ 's that will work is with the help of curvilinear coordinates. By introducing curvilinear coordinates in a way that I shall describe, one can get over this difficulty. Curvilinear coordinates, of course, mean quite heavy extra complications in the mathe-

atics. ~~I couldn't avoid the curvilinear coordinates, and I don't know how to avoid the curvilinear coordinates.~~ <sup>All the same</sup> ~~them.~~ <sup>1</sup>

Aharonov: Could I ask just one more question? Wasn't it possible to introduce a set of  $A_p(x)$  also inside and outside?

Dirac: Yes. You can do that.

Aharonov: And there are also  $q$ 's for the surface. Then when you consider the solutions in which we have the ~~field~~ <sup>field</sup> zero inside, <sup>1</sup>

as a result of the equations of motion, then you don't have to describe it as the  $q$ 's being dependent on the surface. Then you don't get this trouble that the  $q$ 's are dependent on each other.

Podolsky: I don't think that would work because you will be assuming that the  $A_p(x)$ 's vary continuously across the boundary.

Wigner: The result is that they don't vary continuously.

Podolsky: Exactly.

Aharonov: That's not the problem so much.

Wigner: I think Professor Dirac did it differently.

Podolsky: All right. We want to know how.

Wigner: Yes.

Aharonov: Yeah, but I wanted to ask whether Professor Dirac thinks it is impossible to do it in this way.

Dirac: I don't think it's impossible. If you want to, I don't mind your introducing the A's inside as well as outside, as further q's. Then let us consider what happens when you vary the q's which specify the surface, leaving these other q's invariant. We have to consider that possibility and you will find non-linearity.

Furry: You will find non-linearity only if you make this integral  $I_0$  an integral over all space. That will change...

Dirac: I don't mind. That's not essential.

Wigner: No. No. That would be fatal. I think you would have to choose the integral on the outside and further introduce also  $A_\mu(x)$ 's on the inside. But I don't think it might work then.

Dirac: It won't work because we want to get solutions for which these are discontinuous.

Wigner: Yes, and it will be discontinuous if the action integral is discontinuous.

Dirac: If there is discontinuity, you won't have  $\Delta I$  linear in the  $\Delta q$ 's.

Wigner: I don't think, in our opinion, it will be linear in  $\Delta q$ .

Furry: Now that you have erased that boundary condition and the integral, I don't see how it will be non-linear. But when you drop that boundary condition, you lose some of the coupling

between the field outside when you shave off the bump.

Wigner: In this case the integral must be confined to the outside.

Dirac: I can put it like this. There may be some solutions for which  $\Delta I$  is linear in the  $\Delta q$ 's, but for the solutions in which we are interested,  $\Delta I$  will not be linear in the  $\Delta q$ 's, <sup>because</sup> ~~Of course,~~ for those solutions, the field is not continuous.

Furry: Then you must vary the  $\Delta q$ 's pretty arbitrarily to get all the equations.

Dirac: You have to subject them to arbitrary independent variations. This  $\Delta I$  has to be zero for arbitrary independent variations of the  $\Delta q$ 's, and that's not possible with this choice of  $q$ 's. We can make it possible by introducing curvilinear coordinates and suitably choosing our  $q$ 's with respect to the curvilinear coordinates. The trick there is to introduce curvilinear coordinates so that we have a special equation referred to these coordinates for the surface of the electron, let us say the equation  $f(x)=0$ , and when we do the variation process, we don't change  $f$ . ' $f$ ' is something which is kept fixed all through the calculation. In fact,

we may take the equation of <sup>the</sup> ~~a~~ surface to be  $x_1 = 0$ , and for the space outside,  $x_1$  greater than 0; that will do very well.

~~Wigner: There is a danger to these things because they are Hermitian, but not self-adjoint in fact, as Professor Furry pointed out.~~

~~Dirac: When did he point it out?~~

~~Furry: Yes, when did I point that out? (Laughter)~~

~~Wigner: Well,...~~

~~Dirac: Well, that balances the integral over the outside space which is also infinite in space-time.~~

~~(There is a gap in the recording of unknown duration here)~~

~~(After the gap)~~

~~Dirac is speaking:  $x_1$  greater than  $x_0$ ,  $x_1$  greater than 0 to make the thing general. Then we take  $A_\mu(x)$  outside for some~~

of our  $q$ 's. The  $A_\mu(x)$ 's inside we can disregard since they are all zero. Then we shall need also some  $q$ 's which fix the curvilinear system of coordinates with respect to some rigid system of coordinates which we may take to be rectilinear. We may call this other system of coordinates  $y$ . I use the capital Greek suffixes in this second system of coordinates to make a sharp distinction between them and the first system of coordinates, the  $x$ 's. So we have one system of coordinates,  $x$ , with small Greek suffixes, a second system of coordinates,  $y$ , with capital Greek suffixes. ¶ When we make the variation process, this  $y$  system of coordinates is kept

unchanged, but the  $x$  system of coordinates gets changed. The surface  $x_1 = 0$  is changed *with reference to the  $y$  coordinate systems,* and that brings in sufficient

variation for us to be able to have an action principle.

¶ Well, those really are the preliminaries of the theory. Our  $q$ 's now consist of the following: the  $q$ 's must consist of sufficient parameters to fix one of these coordinate systems with respect to the other. We may take either the  $x$ 's as functions of the  $y$ 's or the  $y$ 's as functions of the  $x$ 's. It is more convenient to take the  $y$ 's as functions of the  $x$ 's.

~~Thus we take the function  $y_\Lambda(x)$  for~~  
So  $y$  has enough relations to all  $x$ 's to fix the connections

between the two coordinate systems. And  $A_p(x)$  for all  $x$ 's with  $x_1$  greater than 0 will fix the field outside the electron. This will be the complete set of  $q$ 's which fixes all the things which are physically important. It also fixes some things which are not

physically important, namely, ~~this~~ <sup>the</sup> curvilinear system of coordinates outside and inside, and coordinates apart from  $x_1$ , <sup>namely</sup>  $x_2$ ,  $x_3$  and  $x_0$ , which form the parametrization of the surface.  $\parallel$  All these things are

fixed by these  $q$ 's, so that these  $q$ 's fix a good deal more than is physically necessary, but that does not disturb the working of the action principle. We can still proceed in the standard way of varying the  $q$ 's and then setting the coefficients equal to zero,

and taking those equations as the equations of motion.  $\parallel$  I should say something now about bringing in the boundary conditions. <sup>There will be</sup> ~~no field inside the electron, so the potentials will be zero inside the particle by a suitable choice of the gauge. In any case, it will be constant and~~ <sup>potentials will be constants.</sup> ~~the constants~~

can be brought to zero by a suitable choice of the gauge. Even if there are several electrons, you can have the  $A$ 's zero inside

every one of them by a suitable choice of gauge. <sup>This</sup> ~~which~~ is quite

possible, although that choice of gauge is rather different from the ones physicists usually work with. We have here the boundary conditions which correspond to the surface being a perfect conductor. These boundary conditions will lead to  $A_0$ ,  $A_2$ , and  $A_3$ , vanishing just outside the surface because they have to be continuous, while  $A_1$  does not have to be continuous and does not



vanish just outside the surface. I'm not sure whether I've got these things correctly written here. These are just the conditions for a conducting surface expressed in terms of curvilinear coordinates.

Furry: The A's then are a covariant vector in the curvilinear system.

Dirac: Yes, that is correct. They express the conditions for a conducting surface in curvilinear coordinates. I shall use the

notation  $\mu$ , ~~mu~~ <sup>mu</sup> take on the values 0, 1, 2, 3. Suffixes a, b,

take on the values 0, 2, 3. The suffix 1 is different when one is working with the surface because of the equation of the surface

being  $x_1 = 0$  and the surface conditions are that  $A_a = 0$  just outside the surface.  $F_{ab}$  equals 0 just outside the surface. This gives the usual conditions on the normal component of the electric field and the tangential component of the magnetic field vanishing to obtain a reference which we want here.

Furry: What are the Latin subscripts?

Dirac: 0, 2, 3. This gives the usual conditions for the vanishing of the normal component of the electric field and the tangential component of the magnetic field in a frame of reference in which the particular element of the surface which we are considering is at rest.

Podolsky: I don't understand why you want the normal component of electric fields to vanish. Usually, of course, that would be better.

Dirac: Tangential component of electric field to vanish?

Podolsky: Right.

Dirac: and the normal component of magnetic field?

Podolsky: Oh, yes.

Dirac: Yes. That's perfectly right.

Carmi: Could you explain again what the y's are?

Dirac: The y's are a fixed system of coordinates which are rectilinear and orthogonal coordinates.

Carmi: And the x's take part in the motion?

Dirac: And the x's take part in the variation principle. The y's are fixed. They are introduced just in order to specify the x's and how they vary. Well, to complete our action principle, it

just remains to fill in this surface integral here. The simplest thing to take is one which corresponds to surface tension, which means putting in some numerical coefficients here in this term, giving us the strength of this term. I'm taking this just to be the three-dimensional surface area. This tube, you see, is a three-dimensional thing in four dimensional space-time. It will have a three-dimensional area which will be just what one might call  $m^2 dx_0, dx_2, dx_3$ , where  $m^2$  is the determinant of  $g_{ab}$ . The  $ab$

take on the values 0, 2, 3. *The action integral outside the electron*  
*This* now needs modification. To make it apply to curvilinear coordinates, we must put in  $j$ , where  $j^2$  is the determinant of  $\begin{matrix} g_{\mu\nu} \\ \Gamma_{\mu} \end{matrix}$ . *letter* This is a 4 by 4 determinant and  *$m^2$*  ~~this~~ is a 3 by 3 determinant.

Wigner: And the  $g$  is a symmetric tensor in terms of the x's?

Dirac: Yes. The  $g$  is symmetric in terms of the  $x$ 's. That completes the assumptions of the theory and the remainder of the work is just pure deduction according to standard methods. I don't need to fill in all the details. We have to work out  $\Delta I_0$ . We get terms here coming from terms involving  $\Delta f$  and some other terms involving  $\Delta g_{\mu\nu}$ . Then we express  $g_{\mu\nu}$  in terms of our  $y$ 's, namely

*the  $y_\mu(x)$*   
~~these quantities here, and by this formula here  $g_{\mu\nu}$  put in terms of  $y$ .~~  
 $g_{\mu\nu} = y_{\lambda,\mu} y^\lambda_{,\nu}$

Wigner: How is the  $y$  upper defined?

Dirac: Just by a suitable change in signs from the  $y$  downstairs.

Wigner: Just that?

Dirac: The  $y$ 's are just Minkowski coordinates. Then, of course, one carries out the integration by parts in order to get this to

be the integral of something  $\int \Delta A_\mu$  plus the integral of something  $\int \Delta y$ .

Podolsky: Excuse me, Dr. Dirac. But the equation with a  $\mu$   $\nu$  equals  $f_{\mu\nu}$ , I don't understand.

Furry: Capital letters.

Dirac: I'm sorry. I did that wrong. (He writes  $A_\mu$  on blackboard.) That's the way it should be.

Podolsky: Thank you.

Dirac: We carry out the integration by parts and get a four-dimensional integral here. We also get a surface integral coming in so we get another term here,  $dx_0, dx_2, dx_3$ . (he writes) and that gives us the expression for  $\Delta I_0$ . We also have to work out

what  $\Delta I_s$  equals. We have to have a minus sign here in order to have omega positive. There's ~~a~~ <sup>the</sup> minus sign. We want to have omega positive to give stable electrons. This comes to just minus a half omega times the integral of  $m c^{ab} \delta g_{ad} dx_1, dx_2, dx_3$ , where  $c^{ab}$  is the reciprocal matrix to  $g_{ab}$ .  $c^{ab}$  is, of course, quite

different from the  $g$  with two suffixes upstairs because ~~it's a~~ <sup>it is the</sup> reciprocal of ~~the~~ <sup>a</sup> matrix with three rows and columns only. Well,

then we take the sum of these two and put it equal to zero for arbitrary variations. We get then some equations of motion referring to this four-dimensional region of space outside the electron.

They must be the Maxwell equations, because ~~they~~ <sup>we</sup> are just working

with the Maxwell action for the field outside. This is just a deduction of the Maxwell equations for the action principle in terms of curvilinear coordinates. This surface term which we get

here has to be added on to this term here to give us equations of motion for the surface of the electron. These equations of motion for the electron look like this. We have there four equations for the surface corresponding to four delta  $y$ 's, which we have appearing

here. We can put these equations in <sup>a</sup> more tractable form if one multiplies these equations through by  $y^{\rho}$ , <sup>so</sup> that one gets equations which refer entirely to <sup>the</sup>  $x$  coordinate system. We now have ~~the~~ four equations referring to the four values of  $\rho$ , but we see that three of ~~these equations~~ <sup>them</sup> reduced to  $0 = 0$ . Namely, when  $\rho$  is equal to 0, 2 or 3 we have  $c^{ab}$  equals <sup>the</sup> reciprocal of the matrix  $g^{ab}$ .

This matrix here, when  $r$  is equal to 0, 2 or 3, is reduced to a single term which cancels with this term here. This vanishes when  $r$  is equal to 0, 2 or 3 and this also vanishes when  $r$  is equal to 0, 2 or 3 on account of the surface condition  $f_{ab}$  equals 0. So that of the four equations, the four surface equations which we get from our action principle, three of them are satisfied identically and only one of them remains effective as an equation of motion. That is, of course, what we want physically. We just want one equation to determine how the surface moves normally to

itself. ~~However, we want to~~ <sup>We may</sup> write this left-hand side ( $\frac{1}{4} f^{\mu\nu} f_{\mu\nu}$ )

on account of the other components of this vanishing at the surface.

We get that finally as our equation of motion for each element of

the surface. <sup>As</sup> ~~As~~ a simple physical meaning. <sup>The</sup> This, of course, is

just the invariant which can be constructed on a field just outside the surface, and this thing here has the physical meaning of being

the total curvature of ~~this~~ <sup>The</sup> three-dimensional surface in the four-

dimensional space-time. It's got this geometrical meaning. We have,

therefore, an equation connecting the total curvature with the invariant of the field just outside. That equation is adequate

to give a complete theory. <sup>P</sup> In order to get an appreciation of what

these equations of motion mean, I've applied them to the spherically-symmetric solution. In this solution we have our electron in the

form of a spherical shell. The outside is just the coulomb field.

You can't have any electromagnetic radiation outside because that

would disturb the spherical symmetry. The only thing you can have

in a spherically symmetrical solution is the coulomb field outside. We can have the radius of the electron pulsating. Then the electron is expanding or contracting, and we have this equation of motion which fixes the pulsation of the electron.  $\rho$  Let us see what the equation of motion reduces to under these conditions. This is the total curvature. It is fairly easy to see where that comes from. This is the contribution to the total curvature of the two space directions. You get two over the radius, with this correction coming in on account of the Minkowski space and the motion of the surface, and this is a further term coming in, depending on the acceleration of  $r$  and giving the effect of this acceleration as an additional curvature. So that this is what this right-hand side becomes. You see it is only the radial electric field which contributes to this and this contribution is just given by the coulomb

law electric field. ~~What gives us this term here?~~ You get that as the equation of motion for ~~a~~ <sup>the</sup> surface.  $\rho$  You see that there is an equilibrium radius ~~over~~ <sup>for</sup> which  $\dot{\rho}$  is equal to zero. Now  $\ddot{\rho}$  is

equal to zero when  $r$  is equal to  $a$ , where  $a$  is the equilibrium radius. That gives us the connection between the equilibrium radius and the surface tension. Then we want to get the total energy of the equilibrium state or distribution just to check that with  $\omega$  positive the equilibrium is stable.  $\omega$  has to be taken to be

positive and ~~this is~~ <sup>we see then</sup> that the equilibrium is stable. This is what we need for a physical theory ~~and~~  $\rho T$  then we can work out the total

energy. The easiest way to get the total energy is to note that

the energy  $m$  consists of two terms, one being a coulomb term. We shall obtain a formula for the energy when  $r'$  is zero. Take  $r'$  equal to zero instantaneously but not permanently, and the energy will consist of  $e^2/r$  plus a surface energy term which is proportional to  $r^2$  from elementary physical consideration. And then the minimum value of this energy must correspond to the state when  $r$  equals zero permanently. Therefore we just have to take the minimum value of this quantity and put that equal to  $m$  and in that

way we fix  $a$  and  $\omega$  in terms of  $m$ , the <sup>rest-mass</sup> ~~radius~~ of the electron. All of the constants that appear <sup>in this theory</sup> here can be fixed in terms of the electronic charge and the mass  $m$ . <sup>P</sup> One <sup>may</sup> ~~must~~ work out the frequency of ~~the~~ small oscillations, and then multiply this frequency by Planck's constant so that one would get one quantum of energy corresponding to <sup>this</sup> ~~discrete~~ frequency of oscillation. The result is

that one finds this one quantum of energy is very much bigger than

$m$ , something around 400 times  $m$ , ~~and~~ <sup>T</sup> that result doesn't have <sup>much</sup> ~~any~~

physical meaning because the one quantum oscillation is not a small

~~oscillation and the one quantum oscillation~~ cannot be treated by

the method of small perturbations. The one quantum oscillation

corresponds to quite a <sup>big</sup> ~~bit~~ of disturbance. The change in the radius

of the electron is many times the radius itself, so that we have to set up some more elaborate theory if we want to treat the one quan-

tum oscillation. <sup>P</sup> The natural road to take for getting this more

elaborate theory is to obtain a Hamiltonian. We have an action

principle, so we have a Lagrangian. We can work out a Hamiltonian from it by applying the standard methods. I don't think I need to go through this work because the rules for getting a Hamiltonian are all very well determined. Just to mention the results that we get, the Hamiltonian that we find is always positive definite for this theory. That is a satisfactory result, because it means we can't get motions such as the non-physical motions which we have in the classical point electron. These non-physical motions of

the classical point electron can arise only because the <sup>energy</sup> equation is not positive definite, and ~~has to~~ <sup>one can</sup> bring in a negative energy to

compensate for the positive coulomb energy. And this negative energy means that we have the possibility of growing energy from it to any extent that we like, which enables us to have a runaway electron without violating the law of conservation of energy. These unphysical solutions which we have with the classical point electron cannot occur for this extended electron on account of the

energy always being positive definite. <sup>P</sup> One can work out a Hamiltonian for the general case which ~~one finds to~~ <sup>need not</sup> be spherically symmetrical and then one can specialize it. We ~~have just~~ <sup>get</sup>  $(\omega^2 \rho^4 + \eta^2)^{1/2} + e^2/2\rho$  This  $e^2/2\rho$  is just the contribution to the Hamiltonian for the coulomb energy. This <sup>1/2</sup> is the kinetic energy term, ~~This term~~  <sup>$\eta$</sup>  being the momentum conjugate to the <sup>radius</sup> variable  $\rho$ . There is just one single effective variable left, ~~we call it~~ <sup>the radius</sup>  $\rho$  and it has ~~a~~ <sup>the</sup> conjugate momentum  <sup>$\eta$</sup> . <sup>P</sup> One would like to apply the Schroedinger equation with

that kind of Hamiltonian but that is quite an awkward thing to



work out. I first of all treated that Hamiltonian from the point of view of the Bohr-Sommerfeld method of quantization. With this method of quantization you have to put the integral of the action over one complete cycle which means twice the value of that integral extending from the minimum value of  $r$  to the maximum value of  $r$  for a particular motion. One puts this equal to some integral multiple of  $h$ . If one wants to take the lowest excited state, one would put this numerical coefficient equal to one, so we have just  $h$  here. Well, one can work out what the energy is with this quantum condition here and one finds it to be about 53 times  $m$ .

*P*

I have a student at Cambridge who is trying to get a Schrodinger equation to correspond to this Hamiltonian here. There is some ambiguity when one tries to use that Hamiltonian for a Schrodinger equation, because there is more than one Schrodinger energy operator which may correspond to a given classical Hamiltonian. This term here seems to be pretty definite, but that term could be interpreted in various ways. We might take it simply as it stands there, or we could equally justifiably take it to be  $\{(w\rho^2 + i\omega)(w\rho^2 - i\omega)\}^{1/2}$  ~~this in here~~ or take the two factors in reverse order. All this in classical theory is the same. That is also something which is classically the same as this, but in the quantum theory, it's different. You see the various possible things we might take in the quantum theory, which are not equivalent in the quantum theory, although they correspond to the same thing in the classical theory.

(addition to page 23)

Wigner: There is a danger to these things because they are Hermitian, but not self-adjoint in fact, as Professor Furry pointed out.

Dirac: When did he point it out?

Furry: Yes, when did I point that out? (Laughter)

Wigner: Well,...

Furry: Oh yes. This is the example I mentioned that there are Hermitian operators for which one has no spectrum and for which Professor Wigner uses the technical term that they are Hermitian, but not self-adjoint. The famous example, in fact, is a momentum conjugate to a variable which always has only a semi-infinite range of variation. You may remember that in the first edition of your book you gave a proof that this Poisson bracket relation is actually possible algebraically only for variables which have completely infinite ranges of variation.

Dirac: Yes.

Furry: And here, since  $r$  has only a semi-infinite range of variation, one will have troubles if one doesn't watch out.

Wigner: Well that actually, excuse me. I shouldn't have embarrassed you Doctor Furry. Well, that's not quite it exactly.

Aharonov: Excuse me. I...Why doesn't one find something... in the same way you find that...

Dirac: I shall do that a moment later. All these attempts at getting a Schroedinger equation lead to <sup>the</sup> ~~a~~ difficulty of giving rise to quite a substantial zero point energy, which gets handed on both to the zero state and to the first excited state. The effect of this term is to bring down the ratio of the energy of the first excited stated to the energy of the zero state, to something of the order of two, or something like that, which is no good at all from the point of view of getting the muon. So it would seem that one would have to define things differently in quantizing this Hamiltonian in order to cut out the zero point energy, if one is to get

anything which is to be at all hopeful for the muon. Well, that is the present situation so far, in terms of quantization of this theory. There is of course, the natural thing to do: to try to linearize it by bringing the spin variables in. Some people thought about it, but there is difficulty in bringing in spin variables, which in the first place requires us to bring them in at each point on the surface. That's going to bring in infinite degrees of freedom and make the electron far more complicated than one would like to have it. I think maybe future progress on this idea will consist in finding a

suitable <sup>way</sup> ~~variable~~ for bringing in spin variables which ~~don't~~ <sup>does not</sup> complicate the theory too much. But that is, for the present, an open question. That is really all that I have to say on

the electromagnetic case. <sup>P</sup> I think I might spend just a

minute or two saying something about the gravitational case. There is just one interesting result there, and I will take half a minute. This is the gravitational particle. What are you to take for your boundary conditions? For the electromagnetic case you have the boundary conditions provided by assuming that the surface is a conductor with no electromagnetic field inside. What is the corresponding condition in the gravitational case? There is nothing corresponding immediately to a gravitational conductor. The natural thing to assume is that there is no gravitational field inside the particle and that space-time is flat inside the particle. It was a bit disturbing when I had that idea in the first place, because

I had to convince myself that the space-time inside the ~~sharp-~~ <sup>Schwarzschild</sup> ~~shelled~~ particle is flat. It turns out that it is, <sup>So</sup> ~~but~~ it seems

reasonable to assume, when one is setting up a corresponding gravitational particle, that space-time is flat inside the particle. <sup>PP</sup> One can proceed to set up a theory which is mostly parallel to electromagnetic theory. There is, though, a further difficulty, that if you merely bring in a surface tension term the signs are not right to give equilibrium. You have Newtonian attraction instead of coulomb repulsion and you can't balance out the Newtonian attraction. You would have to have a surface pressure instead of a surface tension. If you just bring in a surface pressure, then you find that the signs are wrong to give stability, and with just gravitational forces and surface pressure the particle is not stable. One has to bring in an extra term with a suitable coefficient to make the particle stable. Of course it is a complication in the theory, <sup>But one has a</sup> ~~One has little~~ possible basis for setting up an exact theory of particles in an Einstein gravitational field.

Band: Professor Dirac, could I ask if you would clarify a little bit more the picture of a three-dimensional surface extended in time. Is it closed in the time dimension?

Dirac: It is a tube.

Band: Is the surface integral a bounded integral? What you're doing is building a model, and I don't have a picture of what's happening.

Dirac: Is this that business about how to define the action integral?

Band: Yes.

Furry: Yes, over infinite time, say, there seems to be a problem.

Dirac: Well, that balances the integral over the outside space which is also infinite in space-time.

Conference - October 1-5, 1962

Thursday Morning - October 4

Merzbacher: I thought I would remind you of something very old that I recalled in connection with Professor Dirac's talk yesterday about the quantization of the extended electron, where you have a given Hamiltonian and want to learn how to write down the wave equation, or something like that. The question came up as to how you do this. You have this problem where there are central forces, where the Hamiltonian has this very central nature - R dependence. I don't have any particular proposal, but I would like to remind you that when Professor Dirac previously solved this problem for the point electron, people subsequently looked at it in all kinds of different ways. I think we might learn something from doing this. The paper (which unfortunately I have never seen, although I have quoted it) by Schrodinger, was published in the proceedings of the Papal Academy.\* Professor Dirac will probably have easier access to this than I do, since he is a member of the Papal Academy. It is possible that Xavier University has this. It's a very hard paper to find. I've never been near a library that had it. You can't even get it on inter-library loan. It's a rarity. Perhaps there's somebody here who...

Professor Furry: The Widener library does not have it.

Merzbacher continues: It is at the University of Michigan at

E. Schrodinger, Commentationes  
Pontificis Academiae Scientiarum 2, 231 (1938)

Ann Arbor, but they don't ship it out. The paper I think must be in German but the reference is in Latin. (He writes on the blackboard.) I can't abbreviate it easily because I don't know how to abbreviate Latin. I will look in the library here. Do you think there is a possibility that you have it?

Podolsky: There is always a possibility but I doubt if they have it.

(Merzbacher continues to write while someone says something about Latin. Dirac comes into the room.)

Merzbacher: The main speaker has arrived. I'll be glad to yield.

Dirac: Thank you, but please continue.

Merzbacher continues: From references to it that I have seen, the point of this paper which is a very long one, is apparently an effort to write the relativistic quantum theory of the electron in general coordinates.

Von Roos says: I have a copy of it at home.

Merzbacher: We can infer from a subsequent paper of Pauli what is in this paper that might possibly be of interest to us. All I want to suggest is that this is a way of looking at an electron that could be used again. I will only point out the physical basis of it as I don't have here the four equations as applied to the Dirac theory of the electron. I will do this in a two-dimensional formalism, which can be carried over to a four-dimensional situation, for a non-relativistic spin particle. Our spinors will have two

components rather than four. The generalization is quite straight forward, as Pauli showed. The idea is this. If we have an electron with a spin, then of course in the usual theory you write something like this: two wave functions which you put together like this. One of them refers to spin up, say, and one of them refers to spin down. Up and down refers to these axes, say X, Y, Z. You single out the Z axis and then, of course, you get a representation of sigma matrices, or in the four spinor component case, you get alpha, beta, gamma, or rho matrices. You work this out and then solve your problem, say, with the problem of the hydrogen spectrum. Schrodinger pointed out what's contained in this equation, in the observation that instead of using this representation you can use one which in a certain sense is more physical. I'm reluctant to use that term, but certainly this representation is adapted to any problem that has spherical symmetry. Instead of speaking about the spin being up or down, you say that when I'm at a point p, with coordinate x, y, z, I will analyze my spin not in terms of up or down but in terms of in or out. In other words, I will quantize at every point; I will use a different 'direction of quantization,' as the old term went, at every point in space. When I go to this point I will study the property of my wave function. Here I will again not use up or down, but will use outward or inwards toward the center or away from it. This is, of course, a representation which was extremely appropriate in the days of helicity studies when people talked of it. In other words, you project



the spin in the direction of the position vector. Of course, the origin is prescribed and fixed, but you have freed yourself from the restriction of the coordinate system, much more than you had before, when you had sort of a hybrid situation where you solved a spherically symmetric problem. Here you talk of spin up and down, but the coordinates you discuss in terms of  $r, \theta, \phi$ . You solve your problem in terms of spherical coordinates when you use the representation that I'm talking about now. Again, of course, you have two components, say  $X_1$  and  $X_2$ , and you would usually express the functional dependence of these on  $r, \theta, \phi$ , like this

$$\begin{pmatrix} \varphi_1(x, y, z) \\ \varphi_2(x, y, z) \end{pmatrix} = \int \begin{pmatrix} \chi_1(r, \theta, \phi) \\ \chi_2(r, \theta, \phi) \end{pmatrix}$$

You make some sort of distinction, and say that distinction to

remind me that this is an entirely different representation. It's one in which this means the amplitude of finding this particle at a position with coordinates  $r, \theta, \phi$ , like here, but with the spin pointing away from the center. This one, then, is the amplitude for the spin pointing toward the center. Now these two descriptions are related by a unitary transformation, of course, that is very simple. It is some exponential with sigma x's or sigma y's, or something like that. It is, of course, dependent on the position of the particle. You make a different spin transformation depending on where you are in space. Now you might say "Why do all this?" It turns out that the radial equation that you get is quite simple and nice to look at, and gets rid of this preferred direction in space. I think there are applications in scattering theory.

This representation, the helicity representation, has been used in the last few years. I'll just end on one example of a case where the mathematics really becomes very, very simple when you want to solve the problem of finding the eigenfunctions and eigenvalues of  $J_z$ . Now by Z, I do mean this preferred direction here. When you do this, in this particular representation, then you find that

the  $J_z$  operator, ~~you remember what it is normally,~~ <sup>which</sup> usually the  ~~$J_z$~~  operator is  $\frac{\hbar}{i} \frac{\partial}{\partial \phi} + \frac{\hbar}{2} \sigma_z$ , ~~But this  $J_z$  is in the old representation.~~  
<sup>in</sup> this new representation  $J_z$  is just  $\frac{\hbar}{i} \frac{\partial}{\partial \phi}$  <sup>and</sup> nothing else. It has

no  $s_z$  in it. And so you see there is a certain simplicity when you work in this representation. I just wanted to remind you of these very old things and suggest that possibly they might be of use in connection with some new problems.

Dirac: I suppose you have to have special boundary conditions at the origin for this transformation.

Merzbacher: This transformation, of course, has a singularity at

the origin depending on which direction you come <sup>from</sup>, But there's no

need to put down the boundary conditions or enumerate them.

Dirac: Suppose we had written down these equations. Perhaps you will need to work out the boundary conditions, or at least enumerate them for a new wave equation.

Merzbacher: That's right.

Dirac: Of course they were all worked out in the paper.

Merzbacher continues: ~~Pauli's contribution was essentially to show that Schroedinger gave the formal set of equations.~~ Pauli, I think

pointed out the physical meaning of this transformation, and it's quite straightforward. Then you might wonder, of course, how this can be, because the eigenvalue problem of  $J_z$  is this (he writes  $J_z \psi = m \hbar \psi$ ). You solve this differential equation (he writes  $(\hbar/i) (\partial \psi / \partial \mathbf{j}) = m \hbar \psi$ ) and then, of course, everybody knows that the answer is very simple,  $e^{imj}$ . Then the traditional argument goes that when you go around the circle by  $2\pi$  you come back to the original value and, therefore,  $m$  must be an integer. But that's preposterous because we know that the eigenvalues of  $J_z$  must be half integral, a fact which doesn't seem to follow from this theory. But, of course, upon reflection you see that it does precisely follow, because when you go  $360^\circ$  around the  $Z$  axis you are changing your spin coordinate system as it were\* Everybody knows what happens when you change your spin coordinate system by  $360^\circ$ : the sign changes, so you must not take those solutions which are single-valued, but rather those which change sign upon going around the circle. Those are just the half integral ones, and you get them quite straightforwardly.

Podolsky: Does anyone have any comment on this paper?

Furry: This is a very interesting point about the single-valuedness. When you use this representation, it turns out that it has to be double-valued and I know that Professor Merzbacher could make some further remarks on this. It really has a bearing on these flux questions of Aharonov and Bohm. We might, perhaps, ask him whether he feels like extending his remarks a little bit.

Podolsky says, chuckling: When you say everybody knows, well, I'm one of those that doesn't know.

Furry: Yes, this was, in fact, a great mystery to me. I was very stupid about it. Aharonov and Bohm in their second paper made a few obvious remarks which made me blush very much because I had not thought of them, but there is a good deal more to be said about it. This, for one thing, is something that Pauli did not discuss correctly at all in the first edition of his article in the 1933 or 32 handbook, and it's one of the things that's considerably changed in the 1950 or 1951 edition. I can't remember these years exactly. What one finds here and there in the literature mainly stems back to the incorrect discussion Pauli gave in the earlier version. Professor Merzbacher knows all about this.

Merzbacher: It's published in an equally obscure journal <sup>as</sup> ~~of~~

the Elisha Mitchell Society.

Furry continues: Not quite so obscure, in the sense that all libraries have it.

Merzbacher: Well, I don't have very much to add.

Podolsky says: Well, this last point wasn't clear to me.

Merzbacher: I think I can make this clear. It is ~~the transition~~ <sup>representation</sup> ~~of~~ the unitary matrix that takes you from one <sup>^</sup> to the other. You will see that it is extremely reasonable. There are many possibilities. It is ~~not unique~~  <sup>$i\sigma_z\theta/2$</sup>  I mean the Pauli spin matrix ~~in the usual representation.~~  <sup>$-i\sigma_z\theta/2$</sup>  where  $\phi$  and  $\theta$  are the following coordinates:  $\phi$  is this angle, the azimuthal

~~angle, and  $\theta$  is the polar angle.~~ Depending on where your

point is on the sphere, you have a different transformation. It's just a unitary transformation, and it has this singularity at the origin. But anything that deals with spherical coordinates must have a singularity at the origin, of course.

Merzbacher: When you apply this transformation  $S$ , this operator.

~~for instance, you get the  $A_z'$  say,  $SJ_z$ . I'll call it prime now to make it a distinction--S-1. You apply it to this familiar total angular momentum operator, then you get  $J_z$  in this very much simpler form.~~  <sup>$\frac{1}{2}\frac{2}{2}\phi$</sup>  <sup>We are used to see the orbital angular momentum in this form, only.</sup> Then when you ask yourself, "Well, how do

I solve the eigenvalue problem, how do I get the magnetic quantum number?" You just go through this differential equation and you

come out with ~~this~~  <sup>$e^{im\phi}$</sup> . And then usually now you say, when I start

from  $j$  augment by  $2p$  then I should demand single valuedness, I should get back to what I had before. And, if you do this,

of course, you come out with the wrong answer--that ~~in which it is the total angular momentum,  $J$ , not  $L$ , that should be an~~ <sup>would</sup>

integer and we know very well that that's not so.

Podolsky: Well, that part I understood perfectly well before.

Merzbacher says: ~~well~~ <sup>How</sup> How do you get the half-integral values, what was the mistake that you made? ~~in going from here to here?~~ <sup>And</sup> <sup>It</sup> It is, if you wish, a condition of single valuedness which

is nothing more than a boundary condition. The thing that you have overlooked is that you have a new coordinate system. In

the old coordinate system where Z was fixed, this was all right as a requirement of single valuedness. When you go around in a circle behind here like this and come back, then you should return to the same value of the wave function that you had before. But now in the new representation where we're using the different coordinate system to describe the spin, we are using a coordinate axis of quantization which points in the radial direction wherever we are. So as we travel around the point and return to the original place, we must change our axis of quantization. We are rotating the coordinate system. Now in rotation of the coordinate system we know that as we go around by  $2\pi$  there is <sup>a</sup> this change of sign in the spinor.

Podolsky: This is the point I didn't know. That is what seems to you perfectly obvious but not to me.

Merzbacher: Well, it's because the spin follows the half integral representation of the rotation group, ~~and the quantities which come in half integral values, have half-values just like these things have.~~ You see one-half of the angle, so as we go around

$360^\circ$  there's a change of sign. This is the famous sign change

~~that occurs when you move the system. Suppose you have a system~~  
~~of~~ an electron with a spin one-half, and it is known to have

this spin in this direction, it's in an eigenstate. When you take this electron and bodily move it around, rigidly, as it were, and bring it back to it's original position, physically nothing has changed, of course, but the wave function has changed

Comment on back of page 9  
(referring to page 10)

(Dr. Herzlacher's notation):  
Editor:  
I suggest you omit this  
repetitious exchange if  
Kunz agrees.

sign. Equivalently when you rotate the coordinate system by  $360^\circ$  you have a new wave function which has a change in sign. This does not mean that the wave function of the particle with spin in the usual old representation is not single valued. It is. But when you bodily move the system you must take this change of sign into account, and that's exactly what we have to do here. When we do go around  $360^\circ$ , we must change the sign. The very single-valuedness requires us to put in this condition because as we go around there will be two changes of sign. Let me write down a typical state:

$(e_0^{\frac{j}{2}})$  I claim, is an eigenstate of angular momentum with  $m$  equals one-half. There are two changes of sign, and this is a single valued wave function. Why? It doesn't look like it. When you change  $\phi$  by  $360^\circ$  there is, of course, a concomitant change of sign because the geometrical properties of spinors require an additional sign change which just compensates for this change of sign. So we are

the half integral values are the right ones. ~~(He writes)~~ Does this help you, Dr. Podolsky?

$$\boxed{\psi(\phi + 2\pi) = -\psi(\phi)}$$

Podolsky: Yes.

back to the conclusion that

Kaiser Kunz says: Students are going to have a little trouble with this shrinking to a very small circle around the Z axis.

Merzbacher says: Well, it has the usual singularity properties. As I say of the ...

Kunz continues: No, I meant ~~of~~ the sign, <sup>change on going</sup> twice around the Z axis.

Merzbacher: Yes, it has a singularity. over



Kunz says: It <sup>does here</sup> ~~has~~ a singularity.

Merzbacher says: Yes, it does, it does, that's true.

Furry: It has a singularity, doesn't it?

Merzbacher says: Yes.

~~Kunz says: I know.~~

Dirac: I think this wave function is similar to the one...

Merzbacher: ~~Probably, yes. There, of course,~~ It's these very considerations in ~~previous~~ <sup>of</sup> single-valued ~~considerations~~ <sup>ness</sup> that led ~~him~~ <sup>you</sup> to the proper value of the magnetic monopole. Right?

Dirac: That's right.

Merzbacher: In fact, if you write down  $J^2$  in the new representation the eigenfunctions, the operator belongs to the symmetric top—

and that's exactly what comes in the magnetic monopole, as M. Fierz ~~Professor Dirac~~ has shown. More generally my point is simply

that the single-valuedness of the wave function is not an artificial boundary condition that must be brought in afterwards somehow to get the right answer, but is quite deeply embedded in the principles of quantum mechanics. Why should one demand this single valuedness in the old representation or the change of sign in the new one? Why <sup>not</sup> adopt some other boundary condition?

This has puzzled people and, as Professor Furry pointed out, was a source of puzzlement to Pauli, who certainly thought about this a great deal and made very different statements about it. Furry says: Well, Pauli, of course, did not hesitate to make statements even though he did not understand it, and this then was accepted at face value. There were some assertions in the

1933 edition about currents flowing from pole to pole in the spherical coordinate system and strange inadmissible singularities. One finds these statements duplicated elsewhere, but if you just sit down and try real hard to find these strange things happening, they aren't happening. This is not the reason that one excludes these half-integral things for the ordinary Schrodinger electron. Now you see we may perhaps gradually get Professor Merzbacher to tell us more about it.

Merzbacher: Will there be enough time?

Dirac: I think there will be.

Merzbacher goes on: This remark was just about the spin, where things are complicated. It came to mind because of Professor Dirac's remarks about the fact that if one sat down and wrote out the radial equations in this representation, one might say something. I don't know that one would, but the problem of single-valuedness, of course, faces you even if you have a

particle without spin. *I remind you of* ~~just the same, you reminded me here of~~  
 what you see in a great many quantum mechanics books: ~~as well.~~

*T*he wave function has to be single-valued, so you must get these

integral values of angular momentum. What do we mean by single-valuedness? I want to be quite specific. I mean that given a wave function which is a function of the coordinates, as you follow it from point P on any closed curve back to the point,

the value of the function must return to its previous value.

Now, of course, *if* a wave function ~~like this one~~ does not have that

property, so you exclude it. Now why would you want to demand

this? People in the old days played with double-valued wave functions a good deal. I think that Eddington had some ideas, too. (To Dirac) You, perhaps, can correct me on this. His had something to do with the positive and negative nature of

~~charge, or something like that. There is some obscure reference in his work to the problem.~~ I would suggest that the fundamental

principles of quantum mechanics drive us to the assumption of single-valuedness. There is really no choice, because these fundamental principles, as I understand them, include one which says that for a particle without spin there is some such thing as a probability amplitude at a given point in space. Once you have said this, there is no question about single-valuedness or double-valuedness. You cannot possibly have double-valuedness anymore at a point. At a point there is, by definition, only one amplitude. There cannot be two. You can have two only if there is some additional degree of freedom that you have neglected in this description. Then you might have two. In other words, saying that when you go from this point P back to it and come up with a different value, it somehow means that you are no longer talking about a particle having just X, Y, Z as its complete set of dynamical variables.

Aharonov: This quite certainly is not satisfactory. If only the wave amplitude changes sign when you go around with no change in any probability, there is no physical meaning that can be

*I suggest omitting [...] if it is OK with Aharonov.*

connected with this change of sign. So it might appear, at least at first sight.

Merzbacher: No, you see that the phase changes by  $180^\circ$ , but it's not an actually observable change. You can just say that everything changes sign, and that it's unobservable.

Aharonov: That's the question.

Merzbacher: Well, maybe you should do the following. You see, you have a choice. You can say I go around and I return to this point. The wave function has become minus  $\psi$  and therefore, what I'll do is just cut here. What I...

Aharonov says: You don't have to cut, <sup>a</sup> You may just say that a wave function has more than one variable at ~~this~~ point.

Merzbacher: ~~I don't know. Well, you see,~~ I think you don't have

quantum mechanics as we know it anymore, if you so say that.

Aharonov: No.

Podolsky: I think you are begging the question.

Merzbacher says: No, I don't think I am.

Podolsky says: You're questioning the assumption of single-valuedness. You're saying that the single-valuedness comes in because we assume the probability amplitude to be single-valued. Essentially, that is what you are saying.

Merzbacher: No, I am saying ~~that because we understand~~ that a

general state can be expanded in terms of probability amplitudes that pertain to a particular point. That is, for every point in space there is a certain probability amplitude for a given state.

Dirac: I think you might say that if the coordinates  $X$ ,  $Y$ , and  $Z$  are observables, ~~that~~ then the wave function has to be single-valued.

Merzbacher: That's what I am saying. I think there is really nothing else to be said about it.

Dirac: If they are just mathematical parameters...

Furry: There is a great deal more to be said about it. I've never heard you use this argument before. I don't think it's in your paper, and I don't particularly like it myself.

Merzbacher: You see, there are arguments which some people consider stronger. I consider them weaker, actually. I'll present one.

Von Roos: Maybe the argument would be all right if you say a particle is a simple representation of the rotation group. A spinless particle is a scalar, and a scalar can only be single-valued.

Aharonov: It's like saying a wave function has to be single-valued.

Merzbacher: I agree with Professor Dirac.

Podolsky: No, let's not get away from this point. You could pretty well say that probability is single-valued, instead of saying probability amplitude is single-valued.

Merzbacher: Quantum mechanics does not say that. It is a separate assumption.

Von Roos says: In the Dirac equation you can represent the spinor by a scalar and make the gamma matrices vectors. This has been

done, for instance by Sommerfeld, and ~~it's~~ *The wave function is then* single-valued. You

can see it really is an assumption.

Merzbacher: Yes, it is an assumption, but I think it is implicit in these postulates that there exists a probability amplitude. You can't have that and then still admit double-valuedness.

Dirac: I think I would agree. If you take any representation in terms of any observable quantities, then the wave function has to be single-valued.

Merzbacher: There is no question any more. Now, people have...

Aharonov says: Now wait a minute. What you understand is not clear. You want to say that if we only specify that all the observables have to be single-valued, we shall wind up with a single-valued wave-function?

Dirac: If you are dealing with observable quantities, yes.

Aharonov says: I would like to...

Dirac: If you have it expressed in terms of any observables  $q$ , you can infer that  $\Psi$  as a function of  $q$ , has to be single-valued.

Aharonov: This I don't see.

Dirac: Well, otherwise you can't add together two states in an unambiguous way.

Aharonov: If one of the states is...

Furry: All the states are double-valued.

Aharonov: Then I can't see how it's possible that...

Dirac: No, you don't have a unique sum for two wave functions, if there is an ambiguity in sign attached to each of them...

Aharonov says: Right. It depends upon what kind of theory you are taking. If there is more than one sheet in space then are you allowed to add all these functions on the same sheet.

Dirac: Then you are bringing in further observables.

Merzbacher: That's right. This is my point. ~~As soon as you go to the second Riemann sheet to a given...~~

Someone says: You have said that there is, in addition to  $x$ ,  $y$ , and  $z$ , another observable. Then you say: I have a particle which has but three observables  $x$ ,  $y$ , and  $z$  which are a complete commuting set, then you have it. There is no choice anymore.

Dirac: Yes, I think you both have the important point. If a set of observables is complete, then the wave function in terms of those observables has to be single-valued. Otherwise, you don't have a unique process for the addition of wave functions.

Merzbacher: May I add a sort of philosophical point to this?

I think that all of physics is this way. You make a model, ~~and then you have to have a physical model.~~ As long as our physical model of <sup>a</sup>~~this~~ particle has  $x$ ,  $y$ , and  $z$  forming a complete set,

there is no choice anymore. Then if Furry would be convinced by the mathematical arguments...

Furry: I already know those mathematical arguments and found them convincing. This is a very interesting way to say it and

I'm beginning to be convinced.

Dirac: The basic assumption is that there is a unique sum for two wave functions.

Furry: Yes. One does find, of course, immediately from the requirement that operators be Hermitian, that if just one wave function out of all the ones you are using is double-valued, then they must all be double-valued. This is indeed the case.

Dirac: Then you really have another variable coming in.

Merzbacher: That's right. In fact there is a real physical example of this that's a model of such a situation, namely, when you talk about the quantum mechanics of rigid bodies. A truly rigid body has an additional degree of freedom as it were. The difference between a point particle and rigid body in this situation is that when you go around in a circle and come back, you express it in terms of representations of the rotation group because you use group-theoretical language. You can contract this loop to a zero loop continuously, and there can surely be no particular significance to having the z axis stick out here. This is why the half integral values of angular momentum are excluded normally. I'll write down a wave function and you'll see it very quickly.

Furry: I like this argument. It's one I even thought of myself.

Merzbacher; This is an eigenfunction for the differential

equation. ~~It solves the differential equation for  $D_2$ .~~ It is

$$\psi = \sqrt{\sin \theta} e^{i\phi/2}$$



an eigenfunction of <sup>the</sup> this operator,  <sup>$L_z$  and</sup> an eigenfunction of the  $L^2$  operator. ~~With~~ <sup>The</sup> eigenvalues (no spin now)  $M$  is one-half here,

of course, and  $L$  is one-half. This looks like a description of

a particle with spin, but without using spinors, ~~and this satisfies Laplace's equation, well, the  $L$  operator.~~ It ~~is~~ <sup>is</sup> an eigenfunction of <sup>The proper differential equations,</sup> ~~this~~, and yet we must exclude this, you see. ~~Well,~~ I would exclude it simply because it's double-valued. I

think it has no place in the theory. But, then if you don't

like this you can exclude it, ~~uh...~~

Furry says: For many other reasons.

Merzbacher: For many other reasons. Pauli excluded it because

when you apply the  <sup>$L_+$</sup>   ~~$L$ -plus~~ operator to it, or the  <sup>$L_-$</sup>   ~~$L$ -minus~~...

Furry: With the  <sup>$L_-$</sup>   ~~$L$ -minus~~ you get in trouble.

Merzbacher: Yes. When you apply any  $L$  operator to this, the result on the right-hand side should be a linear combination

of functions which have the same  $L$  value, <sup>although</sup> ~~they~~ can have different  $M$  values. This function does not have that property. When you apply  <sup>$L_-$</sup>   ~~$L$ -minus~~ to it, it takes you to the  <sup>$L$</sup>   ~~$L$~~  equals three halves <sup>?</sup>

sub-space so to speak.

Furry: Well, I think it works out worse than that, Eugen, I think it becomes singular.

Merzbacher: Well, it becomes singular but I think that is no

objection because it is square-integrable still, ~~I think~~. Sooner or later as you apply  <sup>$L_-$</sup>   ~~$L$ -minus~~, it stops being square-integrable.

Furry: Yes, and this was Pauli's final way <sup>of</sup> ~~to~~ talking about

it. One has to have these things as representations of the rotation group.

Merzbacher says: That's right.

Furry: You have to have these things as representations of the rotation group. You will not be able to apply any finite rotation.

Merzbacher: That's right, which means that ~~L<sub>-minus</sub>~~ <sup>L<sub>-</sub></sup> is not an

admissible operator.

Furry: You will not be able to apply any finite rotation. When you express a finite rotation as an exponential containing an angular momentum operator, it means you have an infinite series. But that means you must be able to apply arbitrarily high powers of the angular momentum operator, and you never can apply arbitrarily high powers here. As soon as you go to some modest power this function becomes inadmissible.

Merzbacher: I think it's cute, but I think it's unnecessary as an argument.

Aharonov: May I add some side thoughts to this argument? It is probably true that if one wants to have some meaning for a non-single-valued wave function, one has to add an extra degree of freedom. Take the following cases a force,  $\vec{F}$ , that has

$\nabla \times \vec{F}$  different from zero, and is not a magnetic force. You cannot represent such a force by a potential. Now can we get

a quantum theory corresponding to such a thing? Well, it's really something that is not given by the usual theory. So we have a case that can be solved classically but not quantum mechanically. But perhaps by being willing to discuss non-single-valued wave functions one might do it. Let us take a case

in which  $\nabla \times \vec{F}$  is different from zero on one line and then

everywhere else you can describe it by a potential which is not single-valued. You have a new degree of freedom which appears only in the quantum case, and which tells you how many

times the particle has rotated around the line of  $\nabla \times \vec{F} \neq 0$ .

One can then quantize it with a non-single-valued Hamiltonian and find non-single-valued solutions that will have an extra degree of freedom. The lesson is that when quantizing a system like this, one finds not only the points of space that are observable but also the number of times the particle

has rotated around the line of  $\nabla \times \vec{F} \neq 0$

Rosen: I think your first argument is the most convincing one. The wave function has to be single-valued, because

otherwise you could shrink <sup>the curve down</sup> it ~~back~~ to a point, which means that at a point you don't know whether  $\psi$  is plus or minus. It ~~is~~ <sup>is</sup>

ambiguous. However, if you have a multiply-connected region, I see no reason why you could not have a double-valued function,

just as, for example, in the case of potential flow <sup>of</sup> for a fluid, ~~you have flow~~ <sup>a</sup> around the cylinder, ~~where in a multiply-~~ <sup>where the potential</sup>

*may be multi-valued.*  
~~connected region the potential~~ I see no contradiction.

Merzbacher: I certainly accept that. That is, if you have a space where a cylinder will be cut out so that you can never penetrate it. This, of course, is no longer the space in which we have defined  $x$ ,  $y$  and  $z$  or a complete set of variables. That's quite true. This is an additional specification. That this is an additional degree of freedom for the cylinder, I don't deny at all. We have such a space in the quantum mechanics

of rigid bodies. I want to stress that we <sup>must</sup> always work in terms

of an understood model. In this case we agree that there are such things as truly rigid bodies which are not made up of particles which you could in principle squeeze together. Then we understand that this rigid body knows the difference between going around the circle once and going around twice. There is a physical way of distinguishing whether a rigid body has rotated  $360^\circ$  or whether it has rotated  $720^\circ$ . Do you want me to make a model?

Furry: Well, I just don't understand it. It gets back to the same condition, doesn't it?

Merzbacher: Let me say <sup>this</sup> again, maybe there's somebody here

who can say this much better.

Furry: Be classical if you want to. I'm just stupid.

Merzbacher: (to Dirac) Do you know it?

Dirac: You have a rigid body in any shape you like. You have strings fastened to different points on it which go out to

fixed points at some great distance away in space, quite long strings, of course. If this rigid body is rotated twice, you get the strings tangled up, but you can disentangle them without cutting them. If you rotate it just once, the strings are tangled up in such a way that you just cannot disentangle them

without further motion of ~~this~~ <sup>the</sup> body, or ~~without~~ cutting them.

Merzbacher: This was known to Hamilton and he gave a fine description of it.

Podolsky: The strings of it are attached at the ends?

Merzbacher: What I am saying is this, Professor Podolsky.

When you have a rigid body and make a  $360^\circ$  rotation, it comes back to its original position, of course. But there is no way of shrinking this operation to the null operation. It is not possible. Whereas, if you rotate it by  $720^\circ$ , there is a way which I cannot describe, of shrinking that twofold rotation to the null rotation, no rotation at all.

Aharonov: Of course, somebody who did not know it before will not be clarified about it now. What does it mean to shrink something to nothing?

Merzbacher: O.K., well what would be a quantum mechanical model? There is a model that is used all the time in modern physics, and that is the collective model of the nucleus, where

you need half integral quantum numbers. That's what ~~the~~ <sup>an odd A</sup> nucleus has, and ~~it's sort of a rotating thing for which that's~~

~~what you need.~~ You have to use the famous <sup>D</sup> functions as wave

functions with half-integrals... I wish Professor Wigner were here, he'd straighten us all out on this. Anyway, now you have

a rigid body and you attach a particle with spin one-half to it, <sup>held</sup> by some <sup>forces.</sup> ~~field.~~ If this body rotates  $360^\circ$ , then, as explained

before, the wave function of the spin one-half particle will change sign. Therefore, the relative phase will have changed between a rigid body and a single particle wave function. This is actually observable. When you rotate twice you have restored the sign and you can no longer distinguish that from doing nothing at all.

Furry: But now it's the spin half particle that's to blame for this?

Merzbacher: Well, I'm just giving you one possible conceptual way of making a physical measurement of this. It would be a globe to which is attached a spin half particle. Then when you rotate the globe around there is a relative phase change, and we know from our earlier discussion that such things are observable.

Dirac: Well, it's really because of these topological properties that spin half exists.

Merzbacher says: That's right.

Dirac: And no other fraction of spin exists besides the spin one-half.

Merzbacher: I'll point out one thing that Pauli said very nicely about the difference between the half-integral and the integral ones. Again it's the same sort of thing really, but it's amusing. Suppose you go from a coordinate system like this to a new coordinate system with a  $z^1$  axis and with  $x^1 y^1$

here somewhere. Then you know ~~a wave function for the eigen-~~<sup>that an</sup> function of the  $z$  component of angular momentum. ~~When it has this direction, it~~ can be expanded in terms of the new angular momentum eigenfunction, ~~that belongs to this direction.~~ This is, of course, well known. We have spherical harmonics with respect to the old coordinates, and ~~we can expand,~~<sup>and new</sup> having an addition theorem ~~that can be expanded in terms of something,~~<sup>by the</sup> ~~that goes from minus  $l$  to plus  $l$ , and there are  $Y_l$ , and it's~~<sup>we can expand the old spherical harmonics</sup> ~~the same  $l$ .~~ ~~That's the crucial point here.~~ ~~There's a  $Y_l$ .~~ ~~It's the same  $l$ .~~ ~~That's the crucial point here on both sides.~~ ~~But there's an  $M$  here and this, of course, is a...~~<sup>in terms of the new ones, using</sup> ~~There are these rotation matrices over here, something like~~<sup>this</sup> ~~this,~~ that describe the rotation that takes you from one

coordinate system to the other. Now Pauli points out that such a relation cannot hold when  $l$  is half integral, so there are no spherical harmonics for half-integral spins. And the way he points it out is very nice, I think. He says suppose you go around a loop (here we have our famous loop) and circle the  $z^1$  axis. That will change every sign over here, but it

will do nothing over here because we can draw a loop which does circle the  $z^1$  axis and not the  $z$  axis. Therefore, this relation cannot hold and there is no representation of the half integral quantum numbers.

Furry: And besides this, there is the tangling of the strings which also tells us about the difference between  $360^\circ$  and  $720^\circ$ , and which is good enough for the not very erudite.

Merzbacher: Well, there are many places where this is explained. One way of seeing it comes from Professor Wigner's talk. Did you hear it?

Furry: I did.

Merzbacher: Have you looked at Professor Wigner's book?

Furry: I have looked at Professor Wigner's book.

Merzbacher: Oh, I ~~didn't~~ <sup>did you look</sup> mean you ~~hadn't looked~~ at the small print? ~~somewhere~~.

Furry: Drehgruppe Topologie?

Merzbacher: That's right. ~~And there it's semantic of course.~~  
~~Once you have made that mapping, you can see it.~~ There are many different mappings that show you this. ~~It's~~ <sup>one is</sup> connected

with the fact that you know the three-dimensional rotation-group has something to do with the surface of a four-dimensional sphere. Two rotations which are at opposite poles correspond to the same ultimate result, but you have to draw the strings on the surface of a sphere, you can't collapse the mapping. That's



one way of looking at it, Apparently the most physical way of looking at it is by using Hamilton's, what were they called?

Someone says: Three-point variables?

Merzbacher: No, I don't think so. Are you familiar with it Professor Dirac? He gave a physical picture of this and, in fact, he applied it to the point.

Dirac: I'm not familiar with Hamilton's, but there is a model which was given by Miss Ehrenfest, Professor Ehrenfest's daughter, showing how two rotations can be continuously shrunk up to no motion at all. Suppose you have two cones, one of them a fixed cone and the other one rolling around the fixed cone. The two cones have the same vertical angle. (He draws on blackboard) . This is the fixed cone and you take a second cone like

this, with the same <sup>Vertex and same</sup> vertical angle  $\alpha$ , which is moving and rolling around the fixed cone without slipping. The moving cone has <sup>rolls</sup> ~~rolled~~ completely around the fixed cone once and <sup>is gets</sup> ~~is~~

back in it's original position. Here we have a motion of a rigid body which brings it back to it's original position, whatever  $\alpha$  is. Now let us suppose that  $\alpha$  changes continuously

from naught to  $\pi$ . In the original case where  $\alpha$  <sup>is close to</sup> ~~equals~~ naught,

this cone will be very thin, like this, and the moving cone just makes two revolutions about an axis, because it's just like two pennies on the table rolling one around the other. It's made two revolutions when it goes around, not just one. So

when  $a$  equals naught we have two revolutions about an axis.

Then when  $\alpha$  is <sup>close</sup> equal to  $\pi$  we get something like this. The

moving one now makes just a very slight wobble when  $a$  is nearly  $p$ , and when  $a$  is equal to  $p$ , it makes no motion at all. This is a continuous way of passing from two revolutions to no motion at all. It is impossible to pass in any continuous way from one revolution to no motion at all.

Merzbacher: That's a beautiful example.

Dirac: If it was possible to pass continuously from one revolution to no motion at all, there wouldn't be any half-spin.

This is a general <sup>argument</sup> reason which comes from the fundamental

principles of the single valuedness of the wave function. It comes from our accepting as a general principle that our states correspond to vectors in Hilbert space. Any two vectors have a unique sum if we have any representation of these vectors, provided it is a complete representation involving single valued

functions, in order that they <sup>may</sup> have a unique sum. ~~That is,~~ <sup>T</sup> two valued wave functions occur only when there is some <sup>in</sup> completeness

in the representation. We might now have a short break.

Conferences October 1-5, 1962

Friday Morning - October 5, 1962

Podolsky: Mr. Shimony can tell us something about his ideas on the theory of measurement. He tells me that it's more speculative than the other things we heard, and I rather like that fact.

Shimony: Bohr and Heisenberg pointed out that the peculiar problems that come up in interpreting complementary phenomena force us to be aware of epistemological problems in the foundations of physics in a way in which we, perhaps, were not so aware before. I think this is true, but I think the emphasis on epistemological problems, in the present foundation of physics, is partial wisdom. I think it's very important but it's not the whole story, because we have very good reason for thinking that human beings are part of nature, and that if we want to have a thorough understanding of human beings, as capable of knowledge, we have to know where these particular creatures fit, in the natural scheme of things

Now, philosophers have a word for theory of being, as contrasted with theory of knowledge. It is called 'Ontology.' I use the expression from time to time. If there were nothing in the world but physical entities, then ontology would be physics. But since there is some reason for believing that there are mental entities,

~~and who knows what other spirits are around, so this is more~~ <sup>ontology</sup>

comprehensive. This is the study of what things there are -- in view of the fact that human beings are just one set of creatures among many in nature. I feel that a thorough-going epistemology

presupposes some sort of ontology and vice versa. That is, I think that one isn't going to have a complete ontology without understanding the conditions of knowledge. There is a sort of mutual pre-

supposition which I hope can be made free from ~~singularity~~<sup>Circularity</sup>, or free from vicious circles, as one might say.

I find passages in Bohr in which he speaks as if these two investigations are complementary in some generalized sense; that one can look into human beings as knowers (and that's one investigation) or one can look into them as physical creatures in the world (and that's another investigation) and they can't be done simultaneously; there's complementarity between them. A sort of fanciful historical note is that complementarity in this sense can be found in Kant, who has a Critique of Pure Reason and a Critique of Practical Reason. In the Critique of Pure Reason there's epistemology without ontology. In the Critique of Practical Reason there is a consideration of human beings as real entities.

Well, with this general point of view, what I am interested in, in my own work, is to explore the various possibilities of quantum mechanics as it is now formulated, and to see if any of them are in principle capable of being understood, not just as epistemological theories, but also as ontologies.

That is, ~~I~~<sup>Suppose</sup> in particular, ~~if~~<sup>if</sup> one has an interpretation of quantum mechanics of the sort that Professor Wigner was talking about, the sort proposed by von Neumann, in which an observer plays an essential

role. <sup>Can one</sup> Imagine or sketch out even in the loosest way an ontology in

which one understands the role of the observer? That is, can one have a kind of generalized psychology, if you will, which incorporates the data we now have about the psychological behavior of human beings, and also the attributions of the power of an observer to

reduce a wave packet? It would be very nice if one had a sort of systematic classification of different interpretations of quantum mechanics, <sup>in order</sup> to do an investigation of the sort of which I am now suggesting for each of them. NEW paragraph

What we need is a language for each of the different possible interpretations. But I won't try that; I leave it to anyone else who wishes to do it.

All right, ~~well~~ let's look then at one particular interpretation, the one that Professor Wigner was suggesting, the one which takes quantum mechanics absolutely literally, which says that even when one is dealing with macroscopic physical objects the formalism

of quantum mechanics literally applies to it. One can use <sup>the</sup> supposition that a superposition is, in principle, different from a mixture, <sup>no conclusion</sup> in

that the reduction of the wave packet does not come at the time of the interaction of the physical instrument with a system, but at the time that the observer intervenes.

Now, let's ask, can one sketch out in general terms an ontology in which one understands physical things, and also the observer as

performing this particular activity? <sup>this question</sup> well, I'll look at it from

two different points of view. One, what we know about single observers. Do we know anything about the ordinary activity of the

human mind which makes it reasonable to think that it can perform

this activity of reducing a wave packet? ~~Well,~~ <sup>W</sup> what characteristics

of mind can one think of that one might refer to? Well, one thing that one might think of is the fact that under certain conditions human perceptions are vague, and under other conditions they are distinct. And one might suppose that vagueness is roughly comparable to a superposition in which there are eigenstates corresponding to different values of macroscopic observables. And precision in per-

ception, ~~well,~~ ~~that~~ corresponds to the situation in which there is

only one eigenstate of a macroscopic observer.

This sounds somewhat plausible until one starts asking ~~about~~ what are the conditions for ~~precision of observation,~~ precision of perception, <sup>and</sup> what are the conditions of vagueness. Well, conditions for precision of perception are conditions like having the lights turned on, being attentive, being in a fairly good emotional state, etc., and conditions for vagueness are just those in which these conditions, or one or the other, is missing.

If <sup>the superposition  $\sum u_i(x)v_i(y)$</sup>  ~~this~~ (he writes) is somehow the input into the consciousness of the observer, we know nothing else than that he will get <sup>one of</sup> a number

of sharp perceptions if the ordinary conditions for perception are good, and a vague perception if the ordinary conditions are bad.\* (see footnote)

The only difference between that and ~~say this~~ (he writes more)--  <sup>$u_n(x)v_n(x)$</sup>  ~~considered as~~ <sup>in the former</sup> ~~this is~~ the input into his consciousness--is that there are a number of differently <sup>possible</sup> sharp reports; not that in the one case there is vagueness and in the other, precision. No correlation, that we know

Addition to page 4

\* Footnote:  $u_i(x)$  are states of a microscopic system  $X$  in which an observable  $A$  has different values  $a_i$ ;  
 $v_i(y)$  are states of a measuring apparatus in which a macro-observable  $B$  has different values  $b_i$ .

of, appears between the first input and vagueness, and the second input and precision. Nothing at all.

So let's try another possibility. This is even more fanciful.

Do we know, or are there, any psychological theories like <sup>the</sup> superposition <sup>principle</sup> and the resolution of superpositions under the right con-

ditions? Well, I'll mention Freud's theory of the dream world. In

the preconscious he claims that one can have <sup>a</sup> superposition of <sup>two</sup> images,

in which both are present, both are clear, there is no blurring,

there is no contradiction. But somehow, they are both present.

Very attractive as an analogy to the <sup>physical</sup> superpositions, but it doesn't

hold at all. For one thing, the order is all wrong. Take a case

in which presumably the input was of the second sort. The input

was not a superposition, ~~that is, say~~ <sup>our conscious of</sup> perceptions of a parent <sup>in some</sup> ~~on~~  
~~the one hand,~~ <sup>cases,</sup> <sup>in</sup> and other cases <sup>of</sup> ~~were~~ perceptions of a spouse. And

the output, that is, the psychological production, was the superposition; instead of having a resolution, one has a compacting of

the superposition. ~~It's all wrong.~~

And so here are two possibilities. Now, let us mention a third, which is of a quite a different sort. Various philosophers and some physicists in speculative moods, Schrodinger, for example, in his book, Mind and Matter, and Bergson, Creative Evolution, and others, suggested that, mind is precisely that aspect of nature in which there is spontaneity.

We wish to say, there is a stochastic element. And certain arguments, some of which are quite crude, I think <sup>are used</sup> ~~but~~ crude data <sup>(dash)</sup>



*are*  
~~is~~ still data ~~are~~ used. For example, Schroedinger argues, that

our circulation has become mechanical beyond our control. Therefore, for the most part we have no consciousness connected with the operation of circulation. Our musculature is pretty much under our control. There is a certain amount of spontaneity, and therefore, consciousness is connected with it. Some processes are somewhere in between. Breathing, for example, is somewhere in between these; presumably if breathing became more mechanical, it would lapse

from consciousness. ~~On~~ *H* he also mentions the fact that when one is acquiring a new skill, one has to concentrate on it; one is conscious of what one is doing at the beginning, and after the skill has become very deeply ingrained, it has left consciousness.

Well, there are troubles with ~~this~~ *attempting to explain the reduction of superposition in this way* too. I think there are

troubles of two sorts. One is the difficulty that maybe there is

a kind of spontaneity connected with mind, but, at least our intuitive feelings *show* give us no *special* more spontaneity, *in dealing with superpositions*. When this is our input, ~~then~~ **I** if our introspection is any guide to what's going on in the

depth of our mind, introspection reveals no more spontaneity, no more chance elements, no more creativity, when our input is of the

form of a superposition ~~of this sort~~, *The input is not superposed with respect to macroscopic observables.* than when ~~it's this way~~ (he *points*). That's one consideration; I think not the best, because

introspection is often very deceptive. And the other is a biological argument, namely, we have evidence which is mounting and mounting, that the properties of large scale entities and large scale organisms can be explained in terms of properties of small scale

things, like an electron charge. ↗

*No new paragraph*

Recent progress in microbiology, of course, is marvelous in this way. Now suppose that creativity, or a spontaneous element, or a stochastic element is a characteristic of large scale organisms; how could this be the case if it weren't already in some minor way characteristic of small scale things? It could be if this creativity were a structural property. When one builds a television set out of condensers and so on, to say that the characteristic of being a television set isn't to be attributed to the components, is trivial. This is because the characteristic of being a television set is structural, whereas a stochastic element, the property of behaving somewhat spontaneously in no way appears to be structural. So if one expects to find this property in large organisms, there is no reason for expecting not to find it in their very small components.

If this is so, then one would guess that the Schrodinger equation, which is a deterministic equation for the evolution of a state when <sup>*the system is*</sup> ~~it's~~ not being disturbed, is only approximately correct.

And this in turn leads me beyond the theories which I am considering. That is, I am considering only interpretations of quantum mechanics which leave the formalism intact, which don't say that the formalism

is approximately correct <sup>*but that*</sup> ~~and~~ there are small non-linearities, or what

have you, which modify the content.

~~All right, so,~~ **I** it seems to me then, for these various reasons

we have no present account of the nature of mind which in any way incorporates known psychological evidence, plus the extra character-

istic of reducing wave packets. Now, let me mention just one other type of consideration, namely, what happens when you ask, not about a single observer but, about a community of them. That is, we would be very unhappy if the formalism of quantum mechanics did lead us to solipsism or to something bizarre like a society of solipsists. I think there is a kind of gregariousness in human beings, but, carrying gregariousness to the point of forming a society of solipsists would be something which I wouldn't understand very well.

Dirac: What are solipsists? I don't know what you mean.

Shimony: Well, a solipsist is one who believes that there is nothing in the universe but himself and his own perceptions? a society of them would be a rare thing.

Podolsky: (to Dirac) If I were a solipsist I would think you are only a product of my imagination.

Aharonov: Therefore, you wouldn't mind destroying him, because it's only an effect on the imagination?

Podolsky: That doesn't follow.

Aharonov: No? (Chuckling)

Furry: I have some times thought the traffic in Harvard Square seems to be made up of solipsists in the background, driving all the cars. (laughter)

Shimony: Well, let each one of us try to wish away the others.

Guth: We consider a solipsist to be extremely egocentric.

Shimony: Very well, let's think about the problems that come up.

If we don't assume solipsism, there are, I think, some rather severe ones. I'll now mention a kind of gedanken experiment which Professor Wigner has talked about. Some of you may have heard it before, but I hoped he would talk about it here. Suppose there is nothing in the formalism of quantum mechanics which says the instrument you use has to be a particular kind of electronic or a physical device. Why not use a friend as an instrument? Namely, you suppose that a photon if it's right circularly polarized passes through an analyzer, and that if it's left circular it does not. If it's in a state of linear polarization, it half does and half

doesn't: <sup>a</sup> certain superposition. And the friend sees it if it passes, and does not see it if it does not pass. Then <sup>the friend,</sup> ~~he~~ linked <sup>a</sup> up in this way, will be in ~~the~~ state of superposition, if the photon

is initially in a linear polarized state. Fine. Now, how do you use your instrument? You use your instrument in the way you use any macroscopic instrument; you look at it or you ask the right questions, and in particular in the case of a friend, you ask him the question, "did you or did you not see the flash?" If he says, "yes," then there is such a transition. For you, the wave packet—or if you prefer it, the state—which was a superposition of polarization states of the photon plus correlated states of the apparatus including analyzer and friend, is now reduced by this answer. Fine. Now you might ask one further question, "Did you see it before I asked you?" The friend says, "But don't you believe me? I told you I saw the flash!" But, you insist. He says, "Of course I did see the flash long before you asked me." Now how are you to interpret

his answer? There are a number of possibilities, all of them troublesome, all of them leading to some sort of doubt as to whether we have an adequate sketch of an ontological theory which incorporates observers. One possibility is, no matter what answers the friend gives to you, you simply treat them behavioristically, you merely treat him as an apparatus. You don't endow him with any feelings.

In that case his answer to the second question, "did you see it before I asked you?" is ~~precisely the same as~~ <sup>analogous to</sup> the ~~answer~~ <sup>result</sup> you would get from a camera with which you ~~can look at a plate and also look at an~~ <sup>checked your first reading of your apparatus</sup> alternative registration of the apparatus which gives the same answer that it gave by the first way of looking at the apparatus. There is no difference. That's one possibility but <sup>it</sup> ~~this~~ is strictly <sup>behavioristic.</sup> ~~solipsistic.~~ Another alternative ~~is~~ to say, that before you asked

him, he had already made up his mind; that your asking was not what reduced the wave packet. Now, if this is so, then we have something

peculiar. ~~We had that before the asking, a mixture from your point of view is already in a mixture corresponding to this superposition but with the phase relations lost.~~ <sup>(omit)</sup> ~~So~~ Here would be a case in

which, prior to the ultimate reduction of the wave packet in the ultimate observer, there would be a reduction of the wave packet in the apparatus. Well, this seems to indicate that somewhere or another, a non-linearity has crept into the quantum mechanics... either there is a non-linearity in the sense of a limitation on the superposition principle, or there is a non-linearity in the Schrodinger equations which governs the propagation of states.

Someone asks: Why did you assume that it had not collapsed completely when he decides?

Shimony: Well, when one talks of a mixture, ~~mixture is a situation in which~~ there is some ignorance, there is something less than total possible knowledge. That is from ~~your standpoint, that is from~~

the ultimate observer's standpoint. He's describing the situation with a mixture. He doesn't know all there is to know. He knows with probability half the friend observes so and so, with probability half that he didn't. But the objective situation is one in which one or another of the situations envisaged in the mixture is the case. This we know is quite different.

Someone: In other words you know that he has already decided this but, you don't know what he saw;

Shimony: That's right, what I'm saying now, is that if you take his report literally, if you believe that he saw before you asked him the question, then from your point of view he is in a mixture, while from his point of view, he is already in a pure state. The question is, which one.

Aharonov: Will you discuss, in relation to this, Everett's lines?

Shimony: I think that this is an entirely different analysis from Everett's.

Aharonov: According to Everett there would be no difficulty.

Shimony: Right, but look, this is quite a different analysis from Everett's, because Everett really doesn't make reduction of the

wave packet a part of the formalism.

Aharonov: What about the problem of reality.

Shimony: Oh, I'm willing to talk about ~~it~~ *Everett's proposal,* but, I don't think this

is one of the possible ways of analyzing this Gedanken experiment.

That's all. Let me mention one or two more and then we have them all before us and then I have very little more to say, except that any choice among these several alternatives seems equally bad.

Guth: What is this specific reason for the difference between you and Everett?

Shimony: Let me get to that later. Let's just survey the possibilities now, there aren't many more anyway. One possibility is that we deny any attribution of feelings to the friend, we treat him purely behavioristically. That's certainly a possibility and we won't rule it out, but it certainly is in conflict with many of our instincts and our presuppositions. Another possibility is, that the reduction of the wave packet has occurred before the ultimate subject entered on the scene. Now this could indicate that...

Furry: That would mean that there would be a mixed state for you, although a pure state for the friend.

Shimony: It would indicate a limitation on the formalism of quantum mechanics; that some non-linearity has crept in. Therefore, our initial premise that the formalism of quantum mechanics is to be kept absolutely intact, has been violated.

Furry: Well, it has been violated only because a sentient observer...

Shimony: Yes, a sentient observer as contrasted with an instrument enters

upon the scene, a reduction of the wave packet occurs, and this is ~~so~~  
~~the case rather than that~~ <sup>whether</sup> it is the ultimate observer, oneself in

this case, or the friend. That is certainly a possibility.

Furry: It promptly became a pure state.

Shimony: That's right.

Furry: It promptly became a pure state for the friend.

Shimony: That's right.

Furry: It also promptly became exactly a mixed state for you.

Shimony: That's right.

Furry: And then you ask him the question. It was the reduction of the Gibbs ensemble not the reduction of the wave packet.

Shimony: Yes, exactly. And this gets into quite a different line of troubles than the ones we have been talking about before. Here the trouble becomes one of causal ordering of the operations of the various observers. And here I think the situation is very similar to the one viewed in the Einstein-Podolsky-Rosen paradox when the two parts of the system were separated so far that the act of observation of the two parts are outside each other's light cone.

~~so that for the first observer to come on the scene, reduces the wave packet and it is then reduced. Then one can imagine two people taking a photograph of the same apparatus, say X taking it first and Y taking it afterwards? they go apart, and Y looks at his film before~~

X looks at his. But Y having looked at the film first, <sup>he is</sup> the one <sup>who</sup> ~~that has the precipitation into the  $\psi$  and  $\phi$  and then there are~~ <sup>has reduced the superposition of the apparatus and of</sup> ~~both his film and the film of X. There are here~~ serious questions of how Lorentz invariance can be maintained.



Shimony: It looks as if there is...

Furry: I don't see why we should have Lorentz invariance in this case, because there are many, I mean all the good old popular examples of relativity sound very non-invariant in the experiences of the people. They always say, A and B are in relative motion, A sees B's clock running too slow and B sees A's clock running too slow, you can very well have these people's eyes observing these systems far apart and also being themselves in different states of motion, and each one could honestly say that he observed it first.

Shimony: Yea, but here there is a causal connection...

Furry: There is not a causal connection, there is only a correlation. They are both observing the same thing.

Shimony: Maybe in Einstein-Podolsky-Rosen there is, but, not in this. In this there is a causal connection.

Furry: But both observed the same thing.

Aharonov: Suppose one observable is sigma X and the other is sigma Y.

Furry: Oh, in that case it doesn't matter, they'll have no correlation and nothing to check. Either there is a correlation upon which everybody can agree and which it really doesn't matter who observes first, or else there is no correlation and again it doesn't matter who you say observes first. So let the observers have their different opinions.

Podolsky: I think we should let Mr. Shimony tell us what he has in mind before we go on to something else.

Shimony: This is worth following further. My argument is that if the first observer finally has some report register on his consciousness he is the one who is responsible for the reduction of the wave packet, and then there is a causal connection between the two observers; and if we are to take this connection in the same sense at the causal connection in special relativity, it gets us into real trouble because ~~by choosing axes, I should say~~ by choosing X's coordinate system I can make his observation earlier than Y's observation, or choosing Y's I can make his observation <sup>earlier;</sup> well then,

who's the one who caused it?

Furry: Each can say that he is first, or each can say the other is first. Each can think that he is first, or each can think that the other is first.

Aharonov: Well, then, you claim that this reduction of the wave packet is something that will never be observed.

Shimony: Then I don't think you can attribute a causal action to the first observer.

Furry: I have grave doubt, as to whether one can say, that there is anything for which the word causal can properly be used.

Shimony: Well, look, I don't like this alternative either. I'm just exploring the various possibilities, and all of them seem to be troublesome. Let me mention just one more alternative, namely, that the reduction of the wave packet does not occur, that the superposition remains. This is one of the cases which Ludwig talks about, where from the standpoint of the ultimate observer, there is

just no way of telling the difference between a superposition and a mixture. He thinks the right way of describing the friend's state is a mixture. He's wrong, it's a superposition, but he's not badly wrong, because it doesn't make any difference. I think I don't like this because I think again this means changing, giving up one's literal belief in what the friend said. That is, what you are attributing to the friend's state of mind is a kind of indefiniteness corresponding to a superposition. He was saying, no, there's no indefiniteness, and you're not taking his report seriously. So I conclude that if one is willing to give up our ordinary premises regarding inter-

subjective communication, <sup>we can</sup> and keep the formalism of quantum mechanics utterly intact, ~~if one wants to keep it intact,~~ <sup>if</sup> but one wants to trust the ordinary premises of intersubjective communication, ~~it~~ looks <sup>as if</sup> like a small change has to be introduced into the formalism, and this small change can be crucial. Maybe we <sup>had</sup> better look for small changes elsewhere, in our formalism, than in this peculiar

case where a human observer intervenes. Well, anyway, I summarize by saying that if you ask, as I did in the beginning, for a kind of ontological theory in which one not only uses the observer

as a black box, to do certain things, but wants to have an

<sup>of</sup> outline a theory of the conscious observer, ~~even wants to have an outline of a theory of a conscious observer,~~ there are many, many

blind alleys; and I, for one, do not see the way out, and I would be very happy for anybody to sketch ways out. There is one possibility, of course, that is, not to be so rigorous. Let's change the

formalism of quantum mechanics at some point. Bad or good, that's a possibility, but it's not one about which I want to talk about. That's going in an entirely different direction.

Aharonov: You will say a few words about what Everett makes the reduction of the wave packet?

Shimony: Everett simply doesn't. Everett makes the reduction of the wave packet not an ultimate thing. That is, ultimately the universe has one state, and its propagation is governed by the Schrodinger equation. What seems like reduction is really only appearance versus reality. Namely, at one of the crucial junctures where reduction seems to occur, or appear, one has a branching of the relative state, that is, the state having left out part of the universe, in various directions. Now as to that, there are various questions which one can ask. One is, is awareness associated with only one of these, but not with all of them? That's certainly a possibility. Everett's answer was no, so maybe we shouldn't even consider that. He says, no, if there is awareness, it is equally associated with every possibility.

Aharonov: In that case then, each possibility doesn't know about the others, each possibility has no way to know the others.

Shimony: That's right, and if this is the case, well, it seems to me that the thing to ask is how is a situation as visualized in one of the branches to be distinguished operationally, or by any other way, from a situation in which you don't suppose that the other branches are real, but only suppose that there is one branch

where a stochastic jump has occurred. In other words, what are the differences, if any, between one part of it which is enclosed in one branch and one part of it which is enclosed in another branch? What is the difference from that standpoint between his theory of multiple branching, and the theory which has only one

branch, but has ~~changed elements?~~ *Chance events?* And his answer is that there is

no difference observationally, there is only a difference logically, and his claim is that the theory he is proposing is more logical.

Well, I don't know what this means. I think that if you have two

statistical theories ~~equally logical~~ each equally consistent, you can't claim one is more logical than the other, ~~neither has more predictable possibilities than the other.~~ It seems to me that in some sense ~~there~~<sup>s</sup> are equivalent ~~ways of talking about these things,~~

ways of talking about the same thing. One way is more elaborate in its terminology than the other. I think one should invoke Occam's

razor: Occam said that ~~any distinction to be produced~~ *entities* ought not to

be multiplied beyond necessity. And my feeling is that among the entities which aren't to be multiplied unnecessarily are histories

of the universe. One history is quite enough. ~~This is simple.~~

~~That's essentially my analysis of Everett, but it's a very different one than one gets from looking at all the possible answers.~~

Aharonov: I don't see that you point to any inconsistencies. The question is, are there any inconsistencies?

Shimony: I think that my answer is that either there is not a very apparent equivalence between his way of talking and a way of talking

which is, ~~well, you know,~~ much more intuitive. ~~The hidden equivalence.~~

you know, the equivalences are often buried when one makes transformations, ~~so that's one possibility.~~ That is, if you analyze very carefully the meanings of all terms, there is ~~such a relationship.~~ ~~There is~~ such an equivalence. The other possibility is one that I mentioned, that ~~o.k.,~~ they are somehow different, <sup>but</sup> ~~this~~

is one place where it's certainly reasonable to invoke Occam's razor.

Podolsky: Dr. Band.

<sup>says</sup> Band: ~~Something about spontaneity and that in which friends do take~~ *If your friend gives you information spontaneously without being asked, does this cause jumping from one branch to another?* ~~part and something about jumping from one branch to another.~~

Shimony: No, there seems to be a possibility that when this branching occurs most of them are dead and one is alive, but he doesn't want to say this; he wants to say that in the other branch he made a foolish decision or in the other branch he made a wise decision, whatever comfort that would be to you. And in the other branch you were aware of your faulty decision.

Podolsky: There seems to me to be a possibility that when you have two observers simultaneously observing an instrument, that both of them produce reduction of a wave packet, but not the same wave packet. In other words the wave function may be a sufficiently subjective sort of a thing, so each observer produces a wave packet for his own consideration.

Shimony: Fine, that's one of the possibilities but, then I ask what is it in the nature of things that allows intersubjective agreement? Is it what Leibniz has called, 'pre-established harmony?'

Well, that's a desperate and quite ad hoc answer. Ordinarily we believe in an agreement between us when we make an observation, that certain physical conditions for the observation are the same for us. That is, there is something there that we are both observing, and there is similarity enough to describe it.

Podolsky: I see the difficulty.

Shimony: If you are leaving that out, it's truly hard to see what guarantees the intersubjective agreement, that is, if you make your wave function subjective for you, and my wave function subjective for me.

Kaiser Kunz: That isn't so bad because if you wanted to find out,

~~to study it you would say...~~ you would study Dr. Podolsky's <sup>Wave function</sup> and I would have my wave function and you would study that; ~~and~~ Dr.

Podolsky would have his wave function which he would study and I

would have my wave function which I would study. ~~And~~ I would <sup>of course be</sup> make <sup>say</sup> a subjective study of <sup>my wave function.</sup> that. Is it true that we superpose <sup>should somehow</sup> another <sup>of course be</sup> wave function <sup>one</sup> when we study <sup>The wave function obtained by other individuals,</sup> it, or ~~some such?~~ <sup>something of that sort?</sup>

Shimony; I go back now to my original philosophical supposition,

~~that is, pre-supposition, a credo which someday I may have to give up,~~ but it's the credo that you can't do this <sup>investigation</sup> thing entirely in the <sup>small.</sup> ~~smog.~~ <sup>do not want to</sup> That is, this is an answer in which you resign the

responsibility of giving an overall picture of the world in which

there are independent observers interacting. You are saying, here's my answer from my point of view. ~~My point of view is sort of privileged but,~~ if you say that is in addition to doing physics, which is

what we are primarily interested in doing, <sup>B</sup> but in addition to doing physics, I want at least a sketch about ontology, or of an ontology in which, you know, observers with their faults, <sup>and</sup> observers with their full psychological capacity are included. ~~Then~~ I think <sup>we</sup> you have a responsibility <sup>for</sup> of sketching a theory of many observers, interacting with each other more or less on a par. ~~That I don't see where you went.~~

Kunz: I'm glad you mentioned it because I think it ties in with

something else. ~~It seems to me it ties in with something else.~~  
<sup>Off hand</sup> Mathematically you would expect <sup>there</sup> it to exist <sup>a complete</sup> everywhere mathematics <sup>describing the entire situation</sup> of controls. Still we know of no theory which is so complete.  
<sup>are</sup> There ~~is~~ a question; it seems to me. Questions to be asked outside

the theory.

Shimony: May I give Goedel's answer. His conclusion is that mathematics is not merely a matter of axiomatization. Which means

that mathematics in <sup>its</sup> ~~its~~ present form cannot be completely <sup>confined</sup> ~~defined~~ by any amount of axiomat<sup>ion</sup>. Many mathematicians reject Goedel's

interpretation of his own proof, of his own results; but, I'm citing Goedel.

Furry: Physics certainly includes enough mathematics to include the postulates of Goedel's theorem.

Shimony: For a theory of inscriptions anyway, and these are macroscopic problems.

Furry: I should think that in the admittedly woefully incomplete state of our knowledge we are to accept his conclusions with con-



siderably more excuse than the mathematicians could accept it. That is they are dealing with something which ought to be under the control of their mind, and we're at the mercy of the new experimental facts, so we have more excuse for having a theory which is sort of open-ended than they have.

Shimony: Well, anyway I don't know how much more there is to say here. I try to do this with each of the interpretations that I

know, and in each case ~~or every case~~, I find that there may be

consistency, just as classical mechanics is consistent as long as one doesn't try to push it too far. I think there is consistency in many of these interpretations but, if you ask the question, "Is there a consistent extension of them to other than physical reality, to reality embracing conscious beings?" I just see many blind alleys.

Perhaps the moral of this is that there is a kind of complementarity of Bohr's form

between the investigation ~~of Bohr's form~~, or maybe the moral is that, ~~well, one should introduce~~, one should start being speculative about unphysical types. *of things.*

Furry: May I make a brief comment?

Podolsky: Sure.

Furry: I hope this will be made available somehow for our perusal.

Shimony: Well, I.

Furry: If these proceedings are going to be published,

Shimony: I have a paper entitled "The Incompleteness of the Philosophical Framework of Quantum Mechanics," \* (see footnote) but, as is the physics,

my paper is incomplete. (Chuckles in the background)

Addition to page 22

\* Footnote: actually published under the title "Role of the Observer in Quantum Theory", *Am. J. of Phys.* 31, (1963), 75

Furry: I was out of the room when you started your talk, and I don't know just what the postulates were, but it sounds as if the postulate is, that some human or at least sentient observation is the only stage at which...

Shimony: No, no, no, no.

Furry: At which the wave function is...

Shimony: The rules of the game are only two. The rules of the game are, let's take quantum mechanics as it now stands absolutely literally—that's one. The other rule of the game is, don't introduce any black box or ad hoc device, such as an observer, ~~that is a~~ *to account for the* reduction of the wave packet, without a sketch of a theory about

that black box. Those are the only two rules of the game. Then I said I would like to look at all possible interpretations of quantum mechanics consistent with these two rules, but I only looked at one, the only one which I took up in great detail, namely, the one Professor Wigner was sketching, in which the reduction of the wave packet occurred in cognition.

Furry: Professor Wigner's statement of it sounded, to me, quite anthropocentric, and I would certainly be inclined to say that, whatever the fields in which there is controversy, the greatest importance is attached to questions as to what difference there is between a human being or a sentient being, and some other very complicated physical-chemical system. This doesn't seem to me to be one of them. I should say that the essential feature of the human observer as the thing that reduces the wave packet is that

he is a very complicated system which we are not able to analyze in complete detail, and many other things such as a photographic plate, geiger counter complete with amplifier and so on, and many, in fact, all our large scale pieces of apparatus have this in common with human beings, and many, in fact all the usual large scale pieces of apparatus have this in common with humans.

Shimony: Well, that's essentially Ludwig's answer. And one of the various ones Bohr gave, I think. I read Bohr at different times in different ways but I think this is one of his answers.

Furry: Now, the transition would seem to me to be somehow like the step from a completely detailed kinetic theory in which one keeps track of all the particles and uses detailed description of just exactly what goes on in the system, and a statistical mechanical or thermodynamical treatment in which one uses fewer parameters. Now the difficulty that seems to arise is that we have no sketch, as you say, of a theory of just how this change goes but, do we have exactly such a sketch of a theory in the case of a change from detailed kinetic theory to statistical mechanics? Of course, we can show how to get the same answers mathematically, but it seems to me we do not have any such an epistemological theory of the change from one to the other.

Shimony: I think there is an essential difference in the case of the relation between statistical mechanics and thermodynamics, but we do have some pretty good ideas of why most of the  $3n$  conserved quantities are observable, or not observable, on a large or a small

Addition to page 25

Postscript on Dr. Shimony's letter of April 1-64: ↙

an alternative possibility regarding my answer to Prof. Band is to leave the original answer but to add a new note:

At present I do not see any implication of Gödel's theorem regarding the relation of mixture and pure states.

That might be fairer to him, and would be a suitable response to his reformulated question.

scale, whereas, the few in which thermodynamics is interested are; this is largely a physical theory, not an epistemological theory.

Furry: It is a mathematical truth that you will get right answers if you use statistical methods. But, aren't those results proof enough that we'll get right answers if we use statistical methods here? Maybe, it's a simple method here.

Aharonov: After you can use a mixed state and you can get the right results. I mean there is...

(There is extremely loud explosion outside; Bang!!! followed by fifteen seconds of silence.)

Aharonov: Are we all agreeing that there was something, an explosion here, or (laughter)... Is everybody here on this same branch (referring to Everett's theory).

Furry: Whether it's necessary for such a loud bang to be associated with that observation, or whether a smaller one would suffice...

Podolsky: Dr. Band, you wanted to say something.

Band: Yes, I'd like to comment on the difficulty about mixtures and pure states. They have different transformation properties. Generally a theorem true for pure states is not necessarily true of mixtures. You can't just write mixtures into the result.

*Further  
correction  
by  
Dr. Band  
(letter  
4-29-64)*

Shimony: So far I haven't written a mixture there, but you used

it a little bit in ~~this~~ assumption. There should not be a sum but

a functional, a function of functions. A mixture is a function not of the wave function or maybe a function all of the wave function. (note by Shimony concerning the last paragraph:

"Perhaps not this. It makes no sense.")

Furry: It's a bi-linear form and the other is a linear form.

Band: Just look at one system, a pure state or a mixed state. You know less about it, you want to describe this state as a pure or mixed state. If you describe it as a mixed state, you should describe it as a functional of the wave function and not as a function.

Furry: I don't think I can agree with you, Professor Band, I mean after all, there may be theories in which one could establish this but, if one accepts the usual, powerful assumptions used in making the mathematical theory of quantum mechanics—well, though there have been criticisms of these powerful assumptions, no one has ever made the theory without them. Then one proves very definitely that this bi-linear form in the wave functions here, is the most general statistical situation.

Shimony: There's the mixture, as I take it, using projection opera-

tors, <sup>It is</sup> ~~at~~ the ~~corresponding~~ mixture <sup>corresponding to the superposition</sup> when one has neglected phase

relations.

Podolsky: But, such a mixture, of course, is not a wave function.

Shimony: ~~Could I,~~ I don't know how much time we have left, ~~but~~ ~~this I think is a sense,~~ <sup>y</sup> your situation is sensible and intuitive;

everything's good about it except one thing and that's that a ...

Furry: Except that I can't prove it logically.

Shimony: No, I think...

Furry: It only agrees with all the facts and that's its trouble.

(Chuckles in the background.)

Shimony: Except I think the trouble comes in <sup>with large bodies. Either</sup> ~~either~~ you say

that quantum mechanics is not literally correct, that is, when one goes to the large body, ~~there is some~~, there is no longer a superposition; or you say that it is, but you can't distinguish epistemologically between a pure state and the corresponding mixture. Then you can say, "but it doesn't make any difference; after all, I only observe ensembles, I only observe large numbers of cases," and that's the strength of such a position. But I think the weakness comes in a theory which does not tell you in principle what happens in individual cases. That is, in principle, the ordinary quantum mechanics says that the reduction of the wave packet occurs not when you have a great number of identical electrons in the beam, but it occurs for each one of them.

Furry: Oh, yes.

Shimony: Now, your theory would say, well, we don't really care.

Furry: No, no, I don't say that. There can be just one electron. If that one electron has been coupled in the measuring manner with a photographic plate as a macroscopic system in which there are an enormous number of particles—but, I don't even know how many particles there are, or its exact detailed structure, or its isotopic composition. That photographic plate is just as good and as new a thing as I am for this purpose of calling the state a mixed state. For making predictions about that single particle, of course, we won't get much of a pattern on our photographic plate, and no experimenter would ever do it. But, the only differ-



ence between the beam business and the single particle business that can ever be distinguished experimentally is checked when one does the experiment, which has been done, running at such a low intensity that one particle comes through at a time. You add them up, and one gets as a result the statistics that one can predict for a single particle. That is, for a single particle, one cannot predict what will happen exactly, one cannot say exactly what will happen, one can only give probability. What one gets piled up out of this business of sending one through at a time, when one has sent through a million, is just the accumulation of these. I don't know about Ludwig's stuff, Ludwig's doctrine which I haven't read, but I don't feel what I am saying retreats from any experiments done with single particles, except this, that the experiment done on large numbers is recommended. But, you never get much of a check of any statistical relation, because you never get much of a check of any statistical relation if you only take one case.

Shimony: Even a theory regarding only individual cases is checked

by *statistical data.*

Aharonov: Here is the view crucially as far as cognition, namely, as far as the observer, in order to get over your difficulty, right... Anyhow, this is how we describe the relation between macroscopic things and microscopic things in ordinary statistical mechanics. There you have the same thing that you have to give some kind of quantum mechanics.

Shimony: I can understand putting a limitation ~~on quantum mechanics,~~

on the formalism of quantum mechanics.

Furry: I am asking for something that the formalism doesn't contain, finally when you describe a measurement. Now, classical theory doesn't contain any description of measurement. It doesn't contain anywhere near as much theory of measurement as we have here. There is a gap in the quantum mechanical theory of measurement. In classical theory there is practically no theory of measurement at all, as far as I know. Now, quantum theory does an awful lot more for us than classical theory. And I have a suspicion that this is the point in which we should stop making demands on the instruments of classical theory, and as Professor Dirac says, "There are other problems too hard for us." They really are the ones we ought to be thinking about.

Podolsky: There is no way of telling what path we have to take in order to get the kind of a theory we want to have. Possibly by examining these difficulties we may get some clues as to what kind of a theory.

Aharonov: Class one difficulties?

Podolsky: Yes, class one difficulties.

Band: This thing you wrote up there says something about a mixture operator? The result of the operation is a pure state, which is another state, that is still a pure state.

Furry: Oh, yes, you apply this to a wave function. You see this is not a combination of kets, this is a bra and ket back to back.

Band: Yeh, I know this. It's a quantum mechanical operator, some kind of operator. I wanted to have the concept whereby you say, as far as my knowledge is concerned, I don't know what state it is in, but I allow it to be in a mixture of states. This is his condition of my knowledge of the system. I don't represent that con-

dition by an operator. I represent it by ~~possible state and this~~ *Possible states and this means I need the probability of each wave functions.* ~~is what I mean by the presence of a wave function.~~ Well, this is

what I mean by a function of the wave function. It may be something that's missing in quantum mechanical descriptions. It may be the conclusions have to be statistical.

Furry: Well, it may be that with the restrictions on the postulates. Many people, including very distinguished people, have said we really ought to make a more general description of what one really means. But, no one has ever given it, and if one does use the powerful postulates, as many people have used, everyone uses them to derive the complete formal theory and in a formal mathematical way uses the powerful postulates including that famous one, of course, that any Hermitian operator that does not have certain pathological characteristics, is an observable, then you prove that this is the most general theory. If there could be a more general one, somebody somehow or other has to find it.

Band: I don't get the connection between the mixture operation and the state of the system whose condition is not given.

Furry: Oh, this, as I see it, you must realize that this quantum argument I gave the other day is, this formal argument I gave the

other day, is not usually given. The usual way to introduce the mixed state is the way that I have always done it in a class. It is just to say that you ordinarily do not measure a complete set of observables. "The measurement is ordinarily fragmentary concerning the ones you haven't measured, you can only make certain guesses, that is probabilistic guesses as to what they might have been, what the relative probabilities of different values are, and one then puts in these estimated probabilities for the different wave functions which the system might have, if the complete observation had been made, and had come out different ways. One puts those in, these are to be established by the principle of insufficient reason, or by whatever other evidence is available, and then one goes ahead.

Conference: October 1-5, 1962

Friday Afternoon - October 5, 1962

CLOSING REMARKS

Professor Podolsky: It seems to me that we have exhausted the questions that Dr. Schwebel is prepared to answer at the moment. Before closing this conference I would like you people individually, if you so feel like doing, to express your opinion about the desirability of this kind of a conference, a panel conference is different from most ordinary conferences. We would appreciate expression of opinion.

Aharonov: The question is not clear enough, do you mean this type of conference from the point of view of topic, or from the point of view of the number of people?

Podolsky: From the point of view of number of people, organization and everything else that went into it. Did you like the conference?

Dirac: I think it's much better to have a small conference like this where people can really have time to think about things. In the larger conferences you get a paper every ten minutes. Therefore, it's pretty hard to follow after a while.

Podolsky: Thank you Dr. Dirac. Well, this is the kind of opinion I would like to hear from other people too.

Carmi speaks: \_\_\_\_\_.

Podolsky: Thank you, Dr. Carmi. Anybody else want to say something about it?

Band: At this conference, I really learned something, whereas, at

other conferences I really don't learn much. Here you have plenty of opportunities to ask questions and get into discussions. It's good to be able to sleep on it over night, and come back and talk about it the next day.

Aharonov: And we are certainly grateful to Mr. Hart for his help he gave to all of us in everything we have to do. (hearty applause)

Podolsky: Thank you, gentlemen. I do believe that Mr. Hart was more responsible for this conference than anybody else.

Aharonov: I think we should also mention the other people that were all the time around here to help us.

Podolsky: Oh yes, we had plenty of help from these other people. Would you like, Mr. Hart, to mention the names of all the people that helped you, just for the record?

Hart: Well, for the record I would like to mention the immediate people in the room, first of all, starting with Dr. Podolsky. This could not have been done without his great help contacting Professor Dirac, Professor Wigner and Professor Aharonov. I would like to thank Dr. Werner for his tremendous enthusiasm for this type of conference, and for helping to sustain me in some of the effort that we had to go through to bring this about. I would also like to mention in our immediate group at this University, Mr. Fisher. I appreciate all the work that he has done recording these sessions and I particularly hope that he was able to record Professor Dirac's comments as well as Dr. Aharonov's, in their mentioning of the fruitfulness of this type of conference. I would like to thank

Mr. Towle for help in handling the cameras, and Mr. Robert Podolsky, who is not here at the present time, for helping to record some of the material put on the blackboard and taking notes also. There have been a lot of people who may be considered, as Dr. Furry mentioned at one time, I believe, our part of the hidden variables of this conference. We have Mr. Weber in our development office, who went to a considerable amount of trouble in trying to secure and actually obtaining the necessary funds for the conference, and also our Public Relations Department, Mr. Vonderhaar and Mr. Bocklage. I know that there are others, and it's dangerous to list people by name because, I almost of necessity will have forgotten to name people explicitly. I would like to offer my tremendous thanks to the main participants who honored us with their presence at this conference. There is no doubt about it, that without them it could not have been put on and would not have been a success at all, and without the tremendous enthusiasm that all these people manifested during the past week. Wow last but not least, I think we should be tremendously appreciative of the National Aeronautics and Space Administration, the Office of Naval Research, and also The Judge Robert Marx Foundation for contributing the necessary funds to make this possible. Now I would like to mention, although he is not here now. Dr. Jack Soules, of the Office of Naval Research. He was the first man in any government agency who, without qualification or hesitation just took it upon himself to say, "This looks like such a good conference, yes, you will get the money." He was

among us here for a time. He just left last night. Well, I think at the present time this is all that I have to say? I do hope that perhaps within the next couple of years or so, if you are willing and the agencies are willing, we might possibly duplicate this and make it much better, because I have learned from mistakes I have made this time. Thank you very much.

Podolsky: Anything else anyone wishes to say before we close the conference?

Werner: I just want to say on behalf of the students of the University, and also some of the people of the community, who for a while were students at the University here, who came to the lectures, that they certainly have indicated a great deal of appreciation for the stimulation that has been given here. All of those who came, who came and helped to have this conference go on, the students both regularly enrolled and ones who came especially to the conference, express their deep appreciation to you who gave so much inspiration. I think your work here will continue in ways that perhaps go beyond where you may ever see fully in detail how much you have given.

Podolsky: Thank you, Dr. Werner. I now declare this conference closed.



# THE FOUNDATIONS OF QUANTUM MECHANICS

*A conference report by F. G. Werner*

What are the leading problems of quantum physics today? Where does reduction of the wave-packet occur? Why single-valued wave functions? To what extent have relativity theory and quantum theory really been united consistently? Does it make sense to speak of "quantum mechanical action at a distance"? What is the significance of electromagnetic potentials in the quantum domain? What does a leading quantum physicist have to say about the physicist's picture of nature?

Yakir Aharonov, P. A. M. Dirac, Wendell Furry, Boris Podolsky, Nathan Rosen, and Eugene Wigner engaged in vigorous discussions of these questions at a special five-day conference called by Professor Podolsky at Xavier University in Cincinnati. The Conference on the Foundations of Quantum Mechanics was sponsored jointly by the National Aeronautics and Space Administration, the Office of Naval Research, and the Judge Robert S. Marx Foundation. Although the meeting took place during the week of October 1-5, 1962, the writing of this report had to wait until the entire conference proceedings could be transcribed and submitted to the participants for approval.

Years ago, when the number of physicists at a meeting was so small that all could fit easily into a single room, the spirit of free discussion so vital for the progress of physics was characteristic of most conferences. Today, with the large meetings attended by hundreds of people and with many sessions going on simultaneously, it is difficult to create an atmosphere conducive to free and thorough discussion. The prime purpose of the Xavier conference was to recapture some of that earlier spirit of intensity and depth in the exchange of ideas.

The heart of the conference was a series of limited-attendance sessions designed to provide ample opportunity for the six participants to discuss among themselves questions concerning the foundations of quantum mechanics, and to do so at

sufficient length to establish clearly which issues are most in need of further clarification. In order that each main participant might feel free to express himself spontaneously in the spirit of the limited portion of the conference, Chairman Podolsky adopted the policy that references to remarks made by the participants during the conference were to be checked with the persons who said them for approval prior to publication. These limited-attendance sessions were also attended by about twenty observers, who were expected to speak only when called upon by the chairman.

While at Xavier for the conference, four of the participants delivered lectures which were open to the public. Aharonov spoke on the significance of potentials in the quantum domain. Furry lectured on the quantum-mechanical description of states and measurements. Wigner discussed the concept of observation in quantum mechanics. Dirac addressed visiting physicists and students on evolution of the physicist's picture of nature.

Aharonov, in the first part of his public talk, summarized some previously treated effects of potentials in the quantum domain connected with interference and energy shift caused by potentials in field-free regions. Here he emphasized three general points: (a) the effects of potentials are all peculiar to quantum theory in that they all disappear in the classical limits; (b) they all make themselves evident only in nonsimply connected regions, in which freedom from finite field values does not ensure that potentials may be gauged to zero; (c) all these effects of potentials in quantum theory depend on the gauge-invariant line integral of the four-vector potentials around a closed loop in space-time in a manner not affected by the addition of integer multiples of  $ch/e$ . Aharonov suggested that these results peculiar to quantum theory be taken as a hint that we do not yet fully understand all the most characteristic consequences of quantization of the electromagnetic field theory.

In the second part of his "talk, Aharonov questioned whether there might not be some residual quantum effects of potentials in simply connected

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F. G. Werner, the author of this account of the proceedings of the five-day conference on the foundations of quantum mechanics, is associate professor of physics at Xavier University in Cincinnati, where the meeting was held.

regions. Although classically defined vector potentials may always be gauged away in any field-free simply connected region, this may not necessarily be the case for  $q$ -number potentials. To see the difference between the quantum and the classical case, said Aharonov, "Remember that both theories distinguish between canonical and kinematical momentum. Nevertheless, it is only in quantum theory that canonical momentum acquires an independent significance, in particular through uncertainty relations and the demands of single-valuedness of the wave function. Thus in the quantum theory we might have a situation in which both canonical momentum and vector potential are uncertain in such a way that their difference, which depends on the kinematical velocity, is still certain." He illustrated the possibility of observable consequences of this distinction in "a possible residual correlation between electrons moving in a simply connected region with a well-defined velocity and the quantum-mechanical source of uncertain vector potential"; the attempt to remove such vector potentials in a simply connected region through a ( $q$ -number gauge-transformation, he pointed out, would not leave this correlation invariant and therefore this will have an observable consequence.

Aharonov went on to discuss the importance of this aspect of potentials and of its relationship to quantization of magnetic flux in superconductors verified in recent experiments. He also discussed the state of experimental verification and experimental work in progress.

Furry, in his public talk in the afternoon, described the regular formulation of the theory of measurement in standard quantum mechanics in order to provide a background for various further discussions. He discussed the generality of the Gibbs ensemble and the "realistic interpretation" where "we could think of many systems, some prepared one way, some prepared another way, and the experiment consists of measuring on a system drawn from this ensemble". He emphasized that a mixed state, which is the outcome of a measurement, does not mean a state which has a wave function formed from a linear combination of some other wave functions. "It has *no* definite wave function at all," Furry stated. "It has instead a list of probabilities for different wave functions. In applying it, one appeals to the principle of insufficient reason in precisely the same way that one does in classical probability theory. But there is another source of dispersion in quantum mechanics—and it has no classical analog. It is something entirely different from the Gibbs ensemble and has nothing whatever to do with the Gibbs ensemble. But it is true that

if you work in the most general possible way, you can build the Gibbs ensemble situation *on top of* the quantum-mechanical situation, which is quite important for some purposes. Within the context of quantum mechanics it is not possible to ascribe this second form of dispersion to hidden parameters."

In discussing the description of measurement, Furry showed that the orthodox theory of quantum-mechanical measuring processes assumes choosing the interaction between the microsystem and the apparatus so cleverly that after their interaction, the system (apparatus plus microsystem) has a wave function of the form,

$$\mathbf{y}(q, x, T) = \sum_n c_n(T) \mathbf{m}_n(x) \mathbf{f}_n(q).$$

Here,  $\mathbf{f}_n(q)$  is an eigenfunction of the dynamical variable being measured,  $\mathbf{m}_n(x)$  is an eigenfunction of the apparatus-pointer position, and  $|c_n(T)|^2$  is the probability of obtaining the result numbered by  $n$ . Thus, by observing the state of the apparatus  $\mathbf{m}_n(x)$ , the state of the microsystem can be inferred. Furry remarked that in both classical and quantum theory we don't say what we do when we make a measurement.

"In the so-called Einstein-Podolsky-Rosen paradox," said Furry, "we have a situation which theorists cannot ignore, and where the realistic interpretation fails completely. It is just not available. The property of wholeness of the quantum state can apply to systems in which the parts become widely separated and in which one deals with only one part." This is analogous to the wholeness of the quantum state which London has emphasized in the theory of superconductivity and superfluidity. Furry pointed out that for *macroscopic* systems covering *macroscopic* distances—and in that case with a great many particles in them—one has the *essential wholeness* of the quantum state giving special properties to the *macroscopic* system.

In his public talk, Wigner began by declaring most emphatically (three times) that "there is no logical flaw in the structure of orthodox quantum theory". But in quantum experiments "the instrument may even be in a state having no classical analog. . . . How we eventually get the information is not described and cannot be described clearly by quantum mechanics." He noted that on entering science we are filled with idealism concerning the wonderful nature of science and how much it will accomplish for us; but in quantum mechanics only the probability connections between subsequent observations are meaningful. Questions about the process of observation, he said, presently lead to answers such as "We learned that as children," which brings home the fact that "we cannot make

science without being unscientific. . . . This teaches us a little humility in our science."

In discussing the implications of relativistic in-variance in quantum field theory, Wigner questioned how realistic the theory is, since measurements of field strength at points accurate enough to detect quantum effects have not been accomplished because of "very grave difficulties". He also wondered why we almost exclusively measure positions, when the theory says that every self-adjoint operator can be measured. "Nobody really believes that everything is measurable. It is absurd to think of it. . . . [But] I feel terribly uneasy about it. . . . A really phenomenological theory would not only say that there is a measurement but would tell how it should be carried out." Wigner said that one way to do this would be to reduce every physical problem to one of collision, and to perform calculations using the collision matrix, but, he added, "there are, in this world, other things of interest in addition to collisions."

In concluding his talk, Wigner returned to the question of how knowledge and understanding are acquired. Although this question is crucial to physics, he indicated that we must also look elsewhere for the beginnings of an answer. "Science," he said, "has taught us that in order to understand something we must devote a great deal of careful and detailed thinking to the subject in question." He noted that physics has little to say regarding the acquiring of knowledge, which "teaches us a great deal of humility as to the power of physics itself. It also gives us a good deal of interest in the other sciences. . . . I think that an integration [including] more than physics will be needed before we can arrive at a balanced and more encompassing view of the world, rather than one which we derive from the ephemeral necessities of present-day physics."

Rosen took charge of a panel discussion for an entire afternoon. The group, which also included Wigner, Podolsky, Furry, and Aharonov, discussed questions developed that morning at a question workshop, to which the public had been invited, conducted by William Wright, Dieter Brill, and Frederick Werner. The workshop offered those in attendance an opportunity to receive technical help in formulating their questions. The individuals who did so were invited to stay for lunch and to join the other observers in the afternoon to hear their questions discussed by the panel members.

The first such query asked, "What is meant by the statement that an operator is observable? How does one distinguish which are observable?" The ensuing discussion by the panel participants might be paraphrased as follows:

Furry: "As Professor Wigner and I remarked, it's nice to have powerful mathematical weapons if you are making a mathematical theory. If you're interested in powerful mathematical assumptions to make various deductions easy, you make the assertion that every Hermitian operator has a spectrum that can be measured. On the other hand, very eminent physicists have held strongly to the position that one should regard as measurable only things for which we can describe, at least in principle, an actual physical arrangement for making the measurement. And such one finds in the descriptions that Pauli worked out in the early part of his Handbook article. (This adds a little bonus, I might say, for the old custom of learning to read German, which was universal among graduate students when I was one, and is not so universal today.) These measurable quantities include, of course, position within certain limits, and momentum, energy, and angular momentum. As Professor Wigner said, that is just about the end of the list. Time, of course, is not an operator in nonrelativistic quantum mechanics. Time measurement is just a procedure for tagging things with a parameter. Now if you arm yourself only with positions, it is much more difficult to prove all the theorems." Wigner: "How can you measure position?" Furry: "Well, with Heisenberg's gamma-ray microscope." Wigner: "You don't measure position with that! At what *time* do you measure position?" (meaning: the measurement took place at what definite time, if any?)

Aharonov: "What about separating shutters?" Furry: "Yes, that is the method Bohr ordinarily used. One can plan ahead but the experiment might fail."

Aharonov: (referring to the statement above that in practice only positions can be measured) "One can measure energy jumps and thus—if the energy is a sufficiently quantitatively detailed function of momentum and position—from the spectrum find the value of operators which are, in general, complicated functions of momentum and position. So life is not so bad after all."

Furry: "That's right, a single measurement of energy will get you quite a lot of different operators associated with it."

Gideon Carmi, a conference observer, asked: "What is a measuring apparatus, and what is the relationship between observables and dynamical invariants of the system? Some people feel that there is much more to this relationship than appears on the surface." Wigner: "I'm afraid I am one of those people. I

think that it is a very useful thing to analyze in detail what you really measure with a gamma-ray microscope. But Dr. Furry withdrew from the gamma-ray microscope, with good reason. Then he said, 'Let us erect barriers separating the space into many regions, and then we can leisurely investigate in which one the system is found, converting position into a stationary state.' What is measured at all with ease are stationary properties. Arake and Yanase found that only those operators can be measured without approximation which commute with all conserved quantities. Now one of the conserved *additive* quantities is energy, so that they must be already then stationary quantities. It is also evident that in the relativistic theory, if it commutes with the energy, it will have a very hard time unless it commutes with momentum also. Furthermore, the measurement of position, which Dr. Furry mentioned, destroyed the invariance of the system by erecting the barriers. It isn't a bona fide measurement because it does not leave the system alone. It changes the wave function very considerably. It is very difficult to measure something that is really easily measurable that is not stationary. It follows from general theory of observation that unless the measured quantity is stationary, no such measurement is possible. The interaction between instrument and object must be consistent with the principles of invariance." (Note: Further discussions are taking place between Wigner and Aharonov, who has a different interpretation of this point.)

The queries from the question workshop continued: "Is it justified to make a theory ignoring at the outset questions of the measuring process, and then expect to obtain, by means of that theory, a description of the measuring process?" Aharonov: "The point of view that measurement theory is something very special seems to me a very subjective one. There are only special kinds of interaction taking place in nature anyhow, and interactions with human beings are no more special than any other. We don't have to put in a foreign interaction for the measuring process. We believe the theory should be valid also for considerations of measuring processes." Podolsky: "That assumes, however, that the measurement process involves nothing but interaction. But actually, it involves a good deal more. It involves the question of reduction of a wave packet. You say at a certain point you read a pointer or something like that. You have the object on which the measurement is performed. You have the measuring instrument. You establish a correlation through the interaction at the appropriate time.

Then we say we read these measurements and ignore the others. As you pointed out, Professor Wigner, we cannot separate the measuring instrument from all the other objects. In order to measure something about the electron, we have to measure something about this measuring instrument. How do we do it? We've got to have another measuring instrument, unless we can somewhere say, 'Well now, I know what this measuring instrument is doing.' And that is an additional assumption in the theory."

Aharonov: "I think it is inconsistent to say that it should collapse suddenly, only when we human beings are coming and looking at the thing. Suppose we consider such a large system independently of the fact that we call it a measuring process, but consider simply that this kind of interaction is going on. There the collapse should happen independently of whether we call it a measurement process or not, or whether we prepare it as a measurement process. If the theory is consistent, independent of questions of measurement theory, it should also answer problems of measurement theory, because measurement theory serves only to point to some special difficulties of the theory. But these are independent of the question of measurement."

Podolsky: "I disagree."

Wigner: "There are perhaps two points of view on this subject. Ludwig, who made use of exactly the point of view of Dr. Aharonov, said that quantum mechanics is not suited to describe macroscopic objects, because there the contraction of the wave packet takes place under all conditions. The other point of view is that quantum mechanics applies even to macroscopic objects, and the *collapse of the wave packet takes place only through the act of cognition*. This is entirely tenable. It says that quantum mechanics gives us only probability connections between subsequent observations or cognitions. I never succeeded in finding out what Dr. Dirac thinks about it, because he dodges the issue. But there are two points of view and I think we must admit we don't know, with absolute certainty, the answer. I agree with Dr. Podolsky's opinion."

This interchange is indicative of the nature of the debate between the main participants which continued throughout the afternoon. It is clear that no complete agreement among the panelists was reached as far as the first questions were concerned.

The next question from the workshop was: "Today, what would you consider to be the best reply to the arguments of Einstein, Podolsky, and Rosen?" After much discussion, the panel agreed that the topic is still as challenging as ever, and

that, although the mathematical formalism of quantum theory is perfectly consistent, it is still very difficult to find a way to picture, by a model, some of its subtle consequence, such as the so-called Einstein-Podolsky-Rosen paradox. Some went so far as to say that perhaps this is an indication that at some point a modification may be necessary in the formalism to overcome these difficulties.

Another interesting problem, raised by Carmi and discussed by Wigner and Aharonov, was the question "What would be, from the point of view of quantum theory, the best way to define a classical object?" They concluded that this is another difficulty: macroscopic measuring devices cannot be treated fully by quantum theory. Since any such macroscopic object is built from single electrons and other elementary particles, it seems reasonable to assume that this difficulty may be reflected even in the treatment of single particles.

The last main question considered at the afternoon panel discussion concerned the question of potentials and gauges in quantized theory. Aharonov replied with a further discussion of quantum gauges, which he put forward in his public talk.

The lively spirit of the extended discussions on problems of quantum mechanics, so evident in the panel discussion, carried through to the limited-attendance sessions. Hugh Everett flew to Cincinnati from Washington to present his relative-state formalism. Some of the observers also offered interesting comments concerning various related problems. Merzbacher discussed the important question of the single-valued character of the wave function, its necessity, and its consequences. Guth discussed, among other things, a formulation of a nonrelativistic Schrödinger equation for a particle moving in an electromagnetic field, and showed that one can transform it to an equivalent equation dealing only with local gauge-invariant quantities. Nevertheless, one could show that the Aharonov-Bohm effect can be incorporated in such a theory. Schwebel reported on a reformulation of quantum electrodynamics without photons (published elsewhere), and Rivers spoke on an interpretation of metric which he was preparing for publication. Shimony discussed the general state of affairs in measurement theory, giving some challenging thoughts of his own.

The high point of the conference was P. A. M. Dirac's talk on "The Evolution of the Physicist's Picture of Nature" (which was subsequently published in the *Scientific American*, May 1963). In keeping with the idea that the development of general physical theory is a continuing process of evolution, he gave a brief account of some past

achievements and discussed, in more detail, present difficulties and a few of his ideas on possible future developments.

Dirac emphasized that progress in theoretical physics sometimes crucially depends on having beauty, based on sound mathematical insight, in one's equations, rather than only having them agree with experiments. Present difficulties suggest that we are in a transitional stage, and present theories are stepping stones to better stages in the future.

"The hostility some people have to [the giving up of the deterministic picture of nature] can be centered on a much-discussed paper by Einstein, Podolsky, and Rosen," Dirac noted. He left this as essentially a problem of describing quantum uncertainty and indeterminacy in a way satisfying to our philosophical ideas. But, since evolution goes forward, "of course there will not be a return to the determinism of classical physical theory." Physicists, he said, are most concerned with difficulties stemming from the fact that present quantum mechanics is not always adequate to give any results.

Dirac indicated his belief that separate, unexpected ideas will be needed for each difficulty, even though most physicists "are inclined to think one master idea will be discovered that will solve all these problems together." After mentioning several examples of these problems, he presented some ideas that he has been developing recently: introducing "something corresponding to the luminiferous ether" of the 19th century which would be subject to the quantum uncertainty relations, discrete Faraday lines of force, and a finite-sized electron. Also, since the description of nature sometimes gets simplified when one departs from four-dimensional symmetry, Dirac expressed doubts as to its overriding importance in future theories. He said "The physics of the future cannot have  $h$ ,  $e$ , and  $c$  all as fundamental quantities." If  $e$  and  $c$  are fundamental (as he suggested) then  $h$  will be derived, and "one can make a safe guess that uncertainty relations in their present form will not survive."

In conclusion, Dirac said he thinks ideas more drastic than his may be needed to make any real fundamental progress. To describe the laws of nature, we need "a mathematical theory of great beauty and power. One could perhaps describe the situation by saying God is a mathematician of a very high order, and He used very advanced mathematics in constructing the universe. Our feeble attempts at mathematics enable us to understand a bit of the universe, and as we proceed to develop higher and higher mathematics we can hope to understand the universe rather better."