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STATISTICAL CONCEPTS IN EINSTEIN'S PHYSICS

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It is particularly appropriate that we are gathered together not so much to analyze the work of Albert Einstein, but to undertake a critical appreciation.

In the Indian tradition it is said that in fact if there is any ultimate entity, that ultimate entity is the Master: master in relation to disciple. And if there is a master in physics, Einstein certainly commands that place: there are so many areas of physics that he has contributed to so decisively and with such clarity. In fact it is almost impossible to believe that one human being could have done all that he did.

I have chosen to talk about a part of Einstein's work which has fascinated me for a long time. My teachers taught me two things about Einstein's contributions in this area: (1) that he had contributed significantly and systematically to statistical physics and (2) that he simply did not agree with the contemporary interpretation--statistical interpretation--of quantum physics. It interested me greatly to examine why it is that the person who has contributed so significantly to statistical and to quantum physics is unwilling to accept a statistical interpretation of quantum physics.

But in the course of these studies, thanks to the help of many of my distinguished colleagues who know a great deal more about the history of science and about statistical physics in particular, I have come to realize that in fact much of what Einstein has done was done with the motivation which was always very clear, namely to elucidate the unknown through statistical assessment of the physical situation.

Albert Einstein's statistical physics contributions were of fundamental importance in the elucidation of atomic and quantum phenomena. It is still curious that Einstein, who contributed so definitively to the foundations of quantum theory, took exception to the generally held interpretation of quantum mechanics.

The first group of statistical papers of Einstein began at the turn of the century, 1902, with a paper on the quantum theory of statistical equilibrium and the second law of thermodynamics. This paper dealt with a method of trying to relate statistical concepts, and quantitative statistical mechanics to thermodynamics. Of course this general idea was underlying the derivation of the Maxwell distribution of velocities: a description of a statistical state, an imprecise (mixed) state of a collection of particles being put in correspondence with a state which was described in an entirely different language in terms of temperature and pressure. But this correspondence was always made with regard to a very specific and particular system, not with regard to dynamical systems in general. Einstein's treatment here is very curiously parallel to the work of the great Josiah Willard Gibbs who had done this work about a year earlier; and obviously the two men worked quite independently of each other. The general idea here was that instead of dealing with the mechanical system as it was, you dealt with many, many identical copies of this entity--the so-called ensemble--and the ensemble, that is the system with many, many identical copies, this surrealistic entity, became a substitute for the original system. This new system was then put into one-to-one correspondence with the system which was the object of the thermodynamic description. We now have a connection with the mechanics of the system: the evolution of the system was completely governed by the equations of mechanics. On the other hand, we had something that conventional mechanical systems did not possess, namely, a variety of physical states--variety of mechanical states--<u>all at the same time</u>. It is not one after another one, but all of them at the same time. So that the thermodynamic system was imaged by <u>not one system but many</u> copies of the same system.

This was followed the next year by another paper which is also now standard knowledge--so standard in fact that at the present time it is a shock to think that there was a time when this was not known--called the theory of the foundations of thermodynamics. In it the notion of ensembles of states was reexamined and one considered the possibility that an ensemble could be thought of for certain purposes as the same system in passage through time. Since the system was in statistical equilibrium there was going to be no long-range change, but there could be short-range fluctuations. The assertion was that, taking the time average of a (sufficiently complicated) mechanical system over a long period of time was more or less equivalent to taking the ensemble average by taking many copies of the same system.

But all these papers were, in a sense, the prelude to the real work, and the culmination was in the beautiful and celebrated paper on Brownian motion in 1905, that extraordinarily miraculous volume of the Annals of Physics in which there were the three famous papers by Einstein.

This paper and its sequel, called "On the Theory of Brownian Motion," in the following year are of particular interest to the physicists who are gathered here because one might say that this was one of the earliest examples of the direct application of symmetry principles to dynamical evolution and dynamical configurations. In talking about Brownian motion, unlike usual mechanical systems where the forces acting on a mechanical system have a definite direction and a definite magnitude, here we have a situation in which particles (pollen grains or other particles) are suspended in a liquid; the liquid is supposed to consist of a lot of restless molecules, and therefore they are jostled around all over the place. But no direction is preferred to any other direction, and therefore, the forces on the suspended particles due to the molecules--the net force--should have no preferred direction.

Now in a pure mechanical situation, it is impossible to deal with such a treatment. If you don't know anything, then you don't know anything further. That is too bad, but it cannot be helped. But when you come down to dealing with ensembles, the situation is quite different: If you don't know which way things go, you make one to go each way! To be on the safe side, you take out insurance. You deal with every direction--send one member of the ensemble in each possible direction.

While it looks like a very wasteful and very expensive way of doing things, in the long run such a system eventually reaches a certain equilibrium in which the ensembles do not change any further--and at that time we say an equilibrium has been reached. So the theory of Brownian motion may be said to be a step in the direction of application of symmetry to a mechanical system.

You will hear from Professor Wigner and Professor Dirac about symmetry with much more authority and much more elegance. We do know that in quantum mechanics symmetry considerations play a more fundamental role because a (pure) state can be symmetric without having to resort to ensembles. It was a fortunate thing that we didn't discover quantum mechanics too early: we may not have discovered the proper treatment of Brownian motion as an example in which two apparently contradictory aspects were reconciled.

One aspect which was the aspect of symmetry said that for the forces that are to be exerted on a particle buffetted by myriad molecules of a liquid no direction is preferred to any other direction. The other aspect is the law of mechanics which states that

the change of the momentum is in the direction of the applied force and proportional to it. How are we going to talk about the motions in mechanical systems with a force in which the direction is unspecified? The answer is: Let us invent a mechanics which goes outside the usual framework. When two natural principles cannot be reconciled, it is a signal that something is missing from our perception of the system. We must enlarge our model so that a new model may come about.

Probably people were more courageous and inventive in the 1900's, because it appears that Einstein had no hesitation in inventing an entirely new model, the new model being one in which you had both the symmetry and the mechanical equations satisfied at the same time.

If symmetry (which has the popular association with Einstein's works in relation to what we now call the Poincaré group or the theory of special relativity and the theory of general relativity) could be related to statistical concepts I'm certainly not going to let you go without trying to tell you that all the other important work that Einstein did at that particular time was also very closely related with statistical concepts.

Boltzmann had found some decades earlier that there was a relation between the possible arrangements that were associated with a certain system (what is called the statistical probability, a statistical mechanical concept about configurations in a mechanical system) and the thermodynamic concept of entropy or disorder--that the entropy was, apart from a certain unit of measurement, (which we now call Boltzmann's constant), proportional to the logarithm of the statistical probability. There is a bridge between the two: if you knew the statistical configuration and the number of possible complexes that you could make, then you could find out how much disorder there could be in the system--how much entropy there could be in the system.

That's of course not very different from our conviction that if there are ten ways in which your books could be disarranged, then anyone who tries to clean up your books, obviously, disarranges them in all those possible ways.

We could now ask: Instead of starting with the statistical mechanical end, and then trying to find out the thermodynamic correlates like the entropy of the mechanical system couldn't we invert the order? After all an equation could be read from left to right, but it could also be read from right to left. If we do that it suggests a means of finding out something about the mechanical aspects of the system from the thermodynamic properties of the system.

In the case of a collection of gas atoms of the kind that we had talked about earlier (Maxwell distribution of velocities, etc.) we know what the mechanical system is and therefore we are anxious to find out about the thermodynamics of the system. But there are other systems in which we know more about the thermodynamics and we did not know or do not know much about its mechanical structure. And honorable mention must be made of the blackbody (a heated cavity from an enclosed area of space which is kept at a steady temperature). You peep into it through a small hole or allow the light to come out through a small hole. The blackbody has a familiar pattern of radiation, of light and heat and other electromagnetic waves inside it, which is characteristic of the temperature and independent of the kind of cavity. Max Planck, at the turn of the century, had at last solved the problem of the distribution of energy--what energies were available, what was the energy in each special range of wavelengths and frequencies and also how it changes as a function of temperature.

In developing his theory Planck introduced us to the concept of the quantum. The discovery of the correct law of blackbody radiation is synonymous with the quantum revolution. That blackbody radiation could be explained is an interesting, but not very compelling news item! But the birth of quantum theory, the introduction of the quantum: that was of course big news!!

Planck had taken the first step in this direction by observing that you had to get a radiation law, which was intermediate between two laws which were known to be approximately true at two ends of the spectrum--the law of Rayleigh and Jeans which was derived from the equipartition law and the Wien radiation law which was derived from entirely different considerations: both of them are approximately true in the two extreme ends of the spectrum. Planck had tried to find something in between, and he had made use of thermodynamics as a means of generating insight into this particular structure.

Einstein made use of a similar path to find out what exactly are the quanta of Planck. Are they real entities, or are they a means of deriving certain results? Are they only manifesting results when there is an exchange of energy between the oscillators and the field, or were the quanta substantial entities in the sense in which we said atoms and electrons were there, or was it only there in the sense of somebody writing down some equations. Einstein set out to find out. If it consisted of a collection of particles, there would be certain properties of the system that one could calculate; he calculated the probability of the radiation contained inside a volume being compressed to a fraction of the volume and found that this particular probability was proportional to a power of the fraction of the volume. If the volume was 1/2,

it was a certain power of 1/2, and the power was given by the total energy divided by the energy of a single quantum. So if there were quanta, this ratio was the number of quanta.

Now that sounds as if these quanta are really there. Because each quantum could have been anywhere, with 50% probability of being in one half and 50% in the other half, if you have two of them, then it would be the square of 50%, that is 25%. If there were three of them, it would be 12½%, etc. Having found this result, he was more or less convinced that these quanta were there. Therefore it was only a natural step to say, if the quanta are there, not only with regard to the exchange of radiation with oscillators and giving rise to the Planck radiation law, but also with regard to the fluctuations of energy density inside the cavity, then these quanta are really there and when they come out of the cavity, they come out as quanta. In that case we must find manifestations of these quanta doing something in the interaction of radiation with matter.

It is very natural that these particles of light must manifest themselves in the photoelectric effect, and this led to the very celebrated paper of the application of quantum theory to the photoelectric effect. It's also in the same issue of Annals of Physics in 1905.

Einstein did not stop at this point. Planck's quanta were for radiation, they were manifesting themselves in various aspects of radiation, but there was a question: Is this a quirk of radiation, or is the appearance of quanta valid for all kinds of matter?

If it were something which is manifesting itself in phenomena which involve radiation, as well as in entities which did not involve radiation, then it was a general principle of physics, and therefore something which was going to revolutionize all physics. So it was natural to search for a process in which radiation was not directly important: a good clue to fruitful areas would be to look for unusual and unexplained phenomena.

One such was the behavior of specific heat of solids. The Rayleigh-Jeans law was good for sufficiently long wavelengths for the blackbody radiation but not for short wavelengths, and was based on the equipartition law. If you applied corresponding ideas one deduced that the specific heat of all solids, when expressed in molar units had to be a constant, because the number of degrees of freedom that were associated with the solid could be easily counted and if you took the same number of particles of different solids, they had the same number of degrees of freedom. Since the energy associated with each degree of freedom at any particular temperature was fixed by thermodynamic considerations, when you had the same number of particles, same number of molecules, in the solid, you

should have the same specific heat--the so-called Dulong-Petit law. All solids seem to obey this particular requirement at room temperatures or higher. When the temperature came down, the solids seemed to show a remarkable decrease of the specific heat, as if certain degrees of freedom were being frozen. How do they freeze? What was the mechanism of this freezing of the degrees of freedom? If the quantum had anything to do with this one, then the quantum better appear. So Einstein applied Planck's theory of radiation to the theory of specific heats of solids in a paper published two years later.

In this paper, Einstein showed that the specific heats decrease rapidly with temperatures below a certain value even though the actual result that Einstein derived for the behavior of specific heats of solids showed too fast a decrease; but for the first time it showed that the quantum was something that cut across the whole domain of physical phenomena, not just for radiation.

We see a revival of this particular idea--this particular theme--of recognizing that if the quantum is there, it better be relevant for everything. (Very much like the simple but powerful point of view that we would like to propagate at the present time, namely that physics being the all-encompassing science there should be no domain of human experience in which physics is not relevant!) Accordingly Einstein decided that it should be possible to be able to apply the notion of the quantum to study the induced and spontaneous emission of radiation by atoms. If atomic systems were excited and in the process of excitation they de-excited, and emitted radiation, the same physical process of course could be reversed so that the atom in the ground state together with a little bit of radiation could get excited and go to the higher state. If both these processes are continually taking place eventually we would reach thermal equilibrium; and then we not only find out what is the probability of the atom being excited and how much of it being in the ground state, but also how much radiation there is. But we already have a theory of the radiation, the Planck distribution law. Question: How shall we reconcile the Planck distribution law with this particular dynamic mechanism? Planck's method was a static method, a method in which you considered the equilibrium state, but not the dynamic processes which are going on. Here you ask the question: How do we derive the Planck law from considerations about the dynamic equilibrium? Einstein discovered that for a two-level atom one had to include, in addition to the process of emission and absorption stimulated by electromagnetic radiation, also a spontaneous emission process enabling the atom in the excited state to decay spontaneously. Therefore an excited atom has two ways of decaying: (1) All by itself without paying any attention to anybody else (spontaneous emission) or (2) stimulated emission in which there is already existing radiation and this

radiation makes the excited energy come down as part of the interaction of the radiation with the atom.

Classical theory of course was quite good at considering the second process: a charged particle could change from one of its configurations to another one of its configurations. And that process has the reverse, namely that if it is in the ground state--the lower energy state of the atom--the atom can pick up energy from the radiation that is existing and go to the upper excited state. But in addition to this one there was the new process of spontaneous emission, and Einstein was able to derive Planck's law of radiation by deducing the ratio of the probabilities for spontaneous emission and induced emission. Though this demonstration was carried out for a very limited case, and though we knew the Planck law already, it was important to demonstrate that the quantum principle was applicable not only to radiation or to solids, but also to atoms in interaction with radiation.

This work was completed about a decade or so later by S. N. Bose who generalized it to dealing with an arbitrary number of material bodies, arbitrary number of collisions, and arbitrary spectrum of energy levels. Thus the Boltzmann method of dealing with dynamic equilibrium could be generalized and adapted to the case of radiation interacting with matter. The result is the Einstein derivation (as a special case of the Bose derivation of general radiation equilibrium).

But the important thing by this time was the fact that the quantum had been shown to be playing the central role not only in the process of blackbody radiation, but in several domains of physics.

Lest you think that I am simply being carried away by the auspicious occasion of the Centennial, and think that all the things that could possibly be done and some more were done by Einstein, allow me to mention that I do notice a curious lacuna in the developments around this time. It is curious that the applications of the specific heat of solids which Einstein started was completed only a decade or so later by Debye, who introduced what we would today call the phonon picture of a solid (namely that the solid really could be effectively replaced for many purposes by a collection of sound waves and that these sound waves were the elementary low-lying excitations). I find it very strange that none of the physicists at that particular time seemed to look at the very close similarities between the blackbody spectrum which had an energy density which is proportional to the 4th power of the temperature (and therefore a specific heat or a heat capacity--thermal capacity--which is proportional to the 3rd power of the temperature); and the behavior of solids which also had the same

temperature dependence. If you look at Debye's theory, which was done in the 1920's, and Planck's theory, which was done in the 1900's, you see that in fact the mechanisms are quite similar. In fact, people very often write the same letter "c" for the velocity of light and velocity of sound so that they look even more similar. I am puzzled why this close connection was not recognized by anyone in the first two decades of the twentieth century!

A major group of contributions to statistical physics was the theory of the ideal Bose gas. The fundamental idea for dealing with the ideal Bose gas came from the Indian physicist Satyen Bose, in a very sketchy but remarkably personal letter to Einstein. Bose pointed out that if one were to think of the photons (the light quanta) as particles, they were not ordinary particles, but particles which were so strictly identical that they were indistinguishable; we must pay proper attention to the indistinguishability by saying that only complexions (arrangements) of photons were important, not which photon was where. If two identical photons exchanged places, it would not be a different state. This method of counting is somewhat different from the counting that we normally use; but Bose showed that if we used such a method of counting distinct states we could derive the Planck law of radiation.

Going back to the analogy that I mentioned about books being disarranged, somebody who did not know what you are working on or what your habits are or what ideas are important, would see a lot of sheets of paper distributed on the table and collect them all nicely together and arrange them all in one neat pile, little knowing that in fact there is information on those sheets: not only on the writing on the sheets, but the manner in which they are displayed. They are supposed to inspire you by being an ensemble of sheets, rather than a pile of sheets. For a person who does not see the distinction, there is in fact no difference. It would be foolish for anybody to have a collection of blank sheets of paper or sheets which are identical--almost everything written the same way--which were displayed on the table and then complaining that by someone tidying his desk by piling them up you have lost the order in which they were placed. If on the other hand they were distinguishable in that some had more interesting doodles than the others, some had formulae or references or jokes written on them: then of course we can say that there has been a disturbance of the order of the papers; but if the sheets are strictly identical, the order of them should not make any difference, because nobody can tell them apart. This was the idea that Bose had brought up.

Einstein immediately recognized not only that this was a new method of derivation of Planck's law but that it was also a new property of light quanta--that light quanta were particles but they were not ordinary particles because there was a new law of counting the statistical probability of any possible configuration. By

recognizing this difference, one finds a new order of matter, not in the composition of the components, but in the interrelationship between the components. In a true sense of the term one may say that this is the beginning of the appreciation of the notion of correlation between things which do not interact with each other. What could be more uncorrelated and independent than a collection of particles?

Previously whenever we had a collection of particles we recognized that if they are interrelated then they must be interacting--they must be exerting forces on each other. But the quantum particles are doing something quite different. They are correlated, but they are not interacting. Their interrelationship was not by interaction--not by forces--but by a certain bond of affinity, a certain kind of complexion, a certain kind of correlation between the particles. Einstein recognized that this particular affinity could be made the beginning of a general theory applicable not only to light quanta, but also for all matter. Because the quantum was not restricted to radiation, the quantum was cutting across all of physics; if identical particles of light were to be treated in a certain fashion, we should try to treat other identical particles also in the same fashion.

The generalization from the treatment of light quanta to the treatment of a general gas, required two more identifications. One was that light quantum being light quantum was very light--it has no mass, and therefore it had a certain relation between energy and momentum. The energy was equal to the magnitude of the momentum multiplied by the velocity of light. But if you wanted to apply it to some other Bose gas--helium molecules--then the law that you use should recognize that the helium atoms were rather heavy and moving rather slowly, and that the relation between energy and momentum for such a system was different.

The second thing was that light quanta had the property that there could be creation and destruction of photons. There was no particular reason in arranging for equilibrium to make sure that the number of all the particles was preserved, but when you deal with something like helium gas, the number of helium atoms are to be preserved. You may rearrange their energy, but you have no business to make them disappear. Otherwise it would be like cleaning up a desk by throwing some papers away. You are told, no, no, you can't throw anything away, you can't hide it, you must keep all of them, you must really rearrange them!

This modification is introduced by a technical device called the chemical potential. Whenever we consider variations, we have to say that if some particles with high energy are moved out to lower energy, then the number of lower energy particles would

increase, but the total number of particles remaining constant. Introducing the chemical potential and introducing the finite mass and the corresponding relation between momentum and energy of these particles, we may generalize Bose's idea to construct a theory of the ideal gas. This theory is now of course quite naturally known as the theory of the ideal Bose gas; and the particles as Bosons.

In all these things, the probabilities that one dealt with were classical probabilities, even so it was the harbinger of quantum theory. Much of the notion of probabilities, except the idea just mentioned which Einstein adopted, enlarged and generalized, nevertheless were still classical probabilities. By this I mean that they obeyed all the things that probabilities were supposed to obey: Probabilities by their very nature must be nonnegative quantities, usually nonzero quantities, the sum of all the probabilities should sum up to 1, and no probability could be larger than 1, nothing can be smaller than 0.

This meant that of course any bifurcation, any uncertainty with regard to the lack of determinacy with regard to the forces acting on the system or the dynamical processes was going to make the ensembles, the collection of realizations, larger and larger. I could make them split into many possibilities, but I could not recombine. So all the statistical processes had a certain direction; and being sensible people we always take the directions from the past to the future that disorder and disarray always increases. A very disheartening way of looking at things, if we thought that all human experience is subject to this law that the total disorder always increases!

It appears in all cases in which the probability was applied to a complex system which has many, many component parts for which the underlying dynamical law of the pure mechanical system was purely deterministic, it is only as a simplification of the complex dynamics that we use statistical methods. It appears to me that Einstein believed that physical law and physical states were respectively causal and precise. Physical law was precise; physical states were precisely specified, and the statistical mechanics that was applied was therefore a device for dealing with this particular complex system. Therefore statistical methods were to be used to deduce properties of the complex system which were hidden from us, which we could not directly perceive by looking for the regularities of the large entity that we have. It seems to me that Einstein would not have indulged either in the idea of intrinsic probability of all processes nor the notion of quantum probabilistic dynamics. Both would appear to him to be playing dice.

This mistrust of quantum probabilistic interpretation has been with him apparently since the early days. For example at the Solvay

conference of 1927, his remarks seem to indicate that he was deeply disturbed by this. But the clearest exposition and the pinpointing of this critique of the standard interpretation, the statistical interpretation of quantum theory is in the paper titled "Can quantum mechanical description of physical reality be considered complete?" by Einstein, Podolsky and Rosen. It is written somewhat late according to this calendar of events that I am describing to you, namely in 1935. The questions raised there are fundamental and demonstrate the nonlocal character of quantum mechanics and have been restudied extensively during the past few years. It seems to me that there is a great revival of interest in questions of this kind, because there is a suspicion that one had misread what Einstein had written. He was not objecting to the existing theory, but was pointing out simply that we had not drawn enough conclusions from it. It would be as if we had been given a Christmas gift but we simply looked at the wrapping paper and did not bother to open the present and find out what was inside.

For those of you who are not familiar with physics of the quantum systems let me just make a two-minute presentation of what is the problem with the statistical interpretation. It is believed that a quantum mechanical system is a system in which you cannot simultaneously specify all attributes that it potentially possesses. For example, in ordinary physics when we talk about a particle which is moving it is almost axiomatic that we must be able to measure both its position and its velocity, both its position and its momentum. If we cannot measure them, it is because we are either not very good at measuring things or that we don't have the right kind of apparatus (or we don't have enough research funds to deal with it), but that in fact if all these things were available there should be no difficulty of measuring it to arbitrary degree of precision: that in substance there exists a thing to which we are approximating: that the system could, in principle, be measured.

Quantum mechanics seems to introduce a system where this is no longer possible, that the dynamical attributes of position and momenta are there for the quantum particles but that you could not make both of them measurable at the same time. If you tried to measure the position very accurately, then the experimental arrangement, the interaction with the system and the method of measurement make it difficult to measure the momentum. If you measure the momentum precisely, you cannot measure the position accurately.

If you want to say that you have a certain amount of fog, we must have the fog extend over a certain region. We cannot say the fog is only at one particular point. By the very nature of fog, it is rather foggy. It must extend. If you talk about a wave, or a traffic jam, they too involve an extended object. And it is not that you cannot measure this more precisely, but that in the very

nature of things you could not make the entity more precise. It is not like a treasure mark in which there is a cross saying the treasure is here. A fog is not here or there, a fog is all over the place. The traffic jam is not at this particular traffic light, but all around it. The quantum system seems to be something of this kind; therefore the natural conclusion is that a quantum system is an extended system. But when you try to detect the quantum particle, say an electron, or alpha particle, or a light quantum, by a suitable apparatus, you can find it at one point and one point only. So if we were to think that it is extended, we are wrong: It is at one point. When you think it is as if it were at one point we are wrong: It is extended. You can't win; either way you are going to lose.

Therefore we say that a quantum mechanical system is intrinsically uncertain, the uncertainty quantified by Heisenberg's principle of indeterminacy. Very often we can think of quantum experiments which possibly have only spectacular outcomes which can depend not only on the initial conditions that we set up, but also on chance. Sometimes it may happen; sometimes it may not happen.

There is a very deep difference with regard to the quantum probability: they come about not through the mechanism that we had in classical theory of taking an ensemble, seven of them going this way and three of them going that way, but by introducing the notion of what may be called the probability amplitude; you square the amplitude to get the probability. But the great advantage of a square root of probability is that it does not have to be positive. If you take a positive number and square it, you get a positive number; if you take a negative number and square it you still get a positive number. Therefore a square root can be positive or negative. Actually in quantum theory it turns out that the square is really the absolute value squared of a complex number; the probability amplitude needs not even be real. But being a complex quantity, you can have two probability amplitudes, either one of which by itself would have given you a distribution, but the two of them added together produce a probability in which everything is concentrated at one point. And therefore the quantum probability is a new kind of probability. It is this kind of probability that Einstein was unwilling to accept, because it appeared to him that either the lessons of the quantum were not fully understood or quantum theory was a provisional theory, a theory which was describing only a limited level of the description of the system. In statistical systems when we employ the probability densities, etc., we were making a simplified description of a complex system, with an aim to elucidate some properties of the complex system which we could not handle in its entirety. But quantum theory as we understand at the present time is not a simplified but incomplete description; Einstein said that completeness of description and probability do not go together. Either the quantum is not a final

and complete theory--but only an approximate and provisional description, or we must not talk about probabilities in the manner in which we talk about it.

It is interesting to note that the use of probability amplitudes furnishes new possibilities and problems of interpretation. In classical probability it was always the density of ensembles which was averaged over; for example, taking forces which are completely symmetric was not possible except by employing ensembles. So you had to get a nondeterministic situation. You had to get a distribution before you could get a group average which is rotationally invariant, something which is the same as seen by all different people with their different orientations. The price of democracy in this case was impurity. But quantum theory with its probability amplitude provides us with a new possibility. We could have a state which was invariant under rotation, which was the same looked at from all possible directions, or a state which was rotationally invariant, but which was nevertheless a pure state. For example, to the extent that you can neglect the spin of the electron, the ground state of the atom is one which is rotationally invariant, the same when viewed by differently oriented observers. Yet it is not analyzable into simpler entities. In fact it is the simplest possible entity. The state is rotationally invariant.

So group integration, the summing over contributions over all possible directions, the averaging is now done for the probability amplitude. There is a new probability amplitude, but the state is now a pure state, it is the most deterministic state of the system. It is both tantalizing and curious. It appears to be a mixture if you try to produce a classical picture of it, to try to see it as if it is a classical system.

Another aspect of the vector space nature of pure states in quantum theory is that if you took a composite system, a system which had many parts, either two particles or one million photons, for a classical system it means that you know the motion of every particle. Every particle had a definite position and a definite momentum. Even if you decided to look at a subsystem and concentrate your attention on the subsystem, the subsystem would be in a pure state. Therefore, every pure state of the composite system viewed as the state of a subsystem continued to be pure. But in a quantum system described by probability amplitudes, this is no longer true. One could think of pure states of the composite system in which the states of the subsystem were not pure states. The reason is the same old notion of correlation, the interrelationship between things not describable in terms of forces, but describable in terms of a connection, in terms of a phase connection between the particles. It is an understanding between the particles, not an imposition upon each other. Correlation is lost when you fragment the total system into subsystems.

This direct contrast with classical physics creates lots of problems and the paper of Einstein, Podolsky and Rosen talks about the appearance of an independent correlated probability distribution when you would have been led to expect uncorrelated behavior for spatially separated subsystems. We find that we don't have consistency.

This correlation has nothing to do with how far the particles are, because correlation is not a force which is acting from one entity to another one, not an interaction which is propagated; a correlation is preserved however far you go. So two particles which constituted part of one system in a pure state, however far they go, still belong to the same system, and the correlation is something that is lost if you talk only about the subsystem. It is something which is inherent in both of those subsystems taken together and something which is not lost when they move apart.

I would like to believe that Einstein was a very sophisticated person, a very clear thinker, a person who read what other people were writing and talking about. His objections to the standard interpretation of quantum theory in this question of the separation between two particles, was couched very carefully: He said that either certain ideas about quantum theory are wrong or a subsystem cannot be considered as separable from the rest of the system. I would therefore like to suggest that perhaps he was saying that we must look at the holistic nature of the entire system; that perhaps quantum theory is suggesting to us that correlation cannot be ignored. In classical theory we could ignore correlations: out of sight, out of mind. But in the quantum theory this was not so; the total perception of the system was essential. The total perception was not something that was expressible in terms of the fragmented components.

There is a consistent view that underlies all his statistical work including the photoelectric effect, the Brownian motion, the radiation equilibrium, the specific heat, and finally the work criticizing the standard interpretation of quantum theory. Whenever you see a statistical theory and you deduce conclusions from it, it should be the way of uncovering something which is covered up, rather than a way of covering up things which are embarrassing! He seemed to feel that in quantum theory we were not entirely honest in uncovering something, and he seemed to be bent on calling our attention to it, even after he is no longer amongst us in life: calling our attention to the fact that in fact there are lessons from quantum theory which have to be uncovered.

To Einstein statistical theories were tools to elucidate the simple but important features of dynamics of complex systems, and it is not difficult to share with him the conviction that

probabilities, even in quantum theory, should lead to the elucidation of a new picture of physical reality.

Professor Wigner has for many years tried to continue in the same vein, to remind us that perhaps we have left something out. He reminds us that maybe we should remember that all of us are conscious, and consciousness is a very crucial part of our human experience, and if we consistently study scientifically all of our human experience, consciousness cannot be left behind. In a situation like the quantum theory where the machinery works perfectly, but somehow or other we cannot agree what it is that works, maybe what we have left out is the role of consciousness with regard to it. Obviously, it is a very difficult problem.

Allow me to conclude this tribute by saying that it is very difficult for us to conceive of a person with the same degree of versatility, clarity, productivity and abiding impact on physics as Albert Einstein.

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