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The Trieste Lecture of John Stewart Bell

Delivered at Trieste on the occasion of the 25th Anniversary of the International Centre for Theoretical Physics, 2 November 1989

The video of this lecture is available here.

General remarks by Angelo Bassi and GianCarlo Ghirardi

During the autumn of 1989 the International Centre for Theoretical Physics, Trieste, celebrated the 25th anniversary of its creation. Among the many prestigious speakers, who delivered extremely interesting lectures on that occasion, was the late John Stewart Bell. All lectures have been recorded on tape. We succeeded in getting a copy of John's lecture.

In the lecture, many of the arguments that John had lucidly stressed in his writings appear once more, but there are also extremely interesting new remarks which, to our knowledge, have not been presented elsewhere. In particular he decided, as pointed out by the very choice of the title of his lecture, to call attention to the fact that the theory presents two types of difficulties, which Dirac classified as first and second class. The former are those connected with the so-called macro-objectification problem, the latter with the divergences characterizing relativistic quantum field theories. Bell describes the precise position of Dirac on these problems and he stresses appropriately how, contrary to Dirac's hopes, the steps which have led to a partial overcoming of the second class difficulties have not helped in any way whatsoever to overcome those of the first class. He then proceeds to analyse the origin and development of the Dynamical Reduction Program and draws attention to the problems that still affect it, in particular that of a consistent relativistic generalization.

When the two meetings *Are there quantum jumps?* and *On the present status of Quantum Mechanics* were organized in Trieste and Losinj (Croatia), on 5–10 September 2005, it occurred to us that this lecture, which has never been published, might represent an extremely interesting historical record for all the participants who certainly shared with us a great admiration for this outstanding scientist and deep thinker. Accordingly, with the permission of the Abdus Salam International Centre for Theoretical Physics, and with thanks to the financial support of the Consorzio per la Fisica of the Trieste University, we have produced from the original record a DVD which has been given to all participants although, unfortunately, the video tape of the event was not particularly good.

Taking into account that the participants to the meetings represented only a very small subset of those scientists who might be interested in hearing what John Bell said in probably his last lecture, we considered that it would be useful for the scientific community interested in foundational problems to publish the text of this lecture in order to make it accessible to everybody. The lecture was preceded by a presentation by the Chairman, Alain Aspect, which we have also included.

Due to the aforementioned low quality of the recording it has not been easy to pass from the tape to the text we are presenting below, and we have to thank, for her precious collaboration, Dr Julia Filingeri who did most of the work, as well as Mrs Anne Gatti from ICTP, Professors

Detlef Dürr and Sheldon Goldstein, and the staff of IOP Publishing who contributed in an essential way in deciphering some particularly difficult passages. Obviously, we take full responsibility for any possible inappropriate rendering of the original talk. We thank the Abdus Salam International Centre for Theoretical Physics for authorizing IOP Publishing to publish this important document.

Some final remarks are in order. Firstly, we have put in square brackets parenthetical remarks that John made while reading sentences from his transparencies. We have also indicated by parenthetical ellipsis (...) very short parts of the speech (usually one word) which we have not been able to decipher. We have included a picture (figure 1) shown by him, taking it from the tape image which is of rather poor quality (we apologize for this) and three figures taken from his transparencies. Moreover, to help the reader in grasping the various points John Stewart Bell brilliantly raised in his talk we have divided the paper into six sections whose titles have been chosen by us to summarize the most crucial points of his argument.

Presentation by the Chairman, Alain Aspect

It is a great pleasure and an honour to introduce Professor Bell. When looking to my old papers I discovered that this 25th anniversary of the ICTP also coincides with the famous paper in which appeared, for the first time, inequalities that are now known as Bell's inequalities so it's a very good opportunity to have a talk by John Bell here. Many of us have been strongly influenced by this work of John Bell because he has shown us that quantum mechanics is much more difficult to understand that we thought it was. I am sure that today he will again raise some questions which are very embarrassing but that we have definitely to face.

First Class and Second Class Difficulties in Quantum Mechanics
John Stewart Bell

This is the picture of Dirac and Heisenberg which was taken here at the ICTP, although not in this room, in a smaller room, in 1973 when there was a meeting here celebrating Dirac's 70th birthday. And I, to my great regret, did not know either of these two men and in fact this picture shows the closest that I got. I am grateful to ICTP for the opportunity.

Figure 1. The picture shows Heisenberg (right) and Dirac (left) in the first row and Bell (left) in the second row

At that meeting many Dirac stories were told and some of them are very relevant for what I have to tell you. I'll start with one told by Heisenberg. He says:

> 'Paul always thinks about his formulations very carefully. He does not like to answer spontaneously at once, he first thinks about things. We were on the steamer from America to Japan, and I liked to take part in the social life on the steamer and so, for instance, I took part in the dancing in the evening . . . he asked me "Heisenberg, why do you dance?" I said: "Well, when there are nice girls it is a pleasure to dance." He thought for quite a long time about it, and after about five minutes he said: "Heisenberg, how do you know beforehand that the girls are nice?"'

Now this illustrates very well the difference between the two men both in life and in physics. Heisenberg was a sunny optimist. He was rather confident that physics will continue along the line that he and the other founding fathers had directed it in 1925. Dirac was never sure that the next turn would also be nice and also not so sure that physics would continue in the same direction. And he tended to think rather that there would be big changes. He said at the meeting in 1973:

> '. . . I think it might turn out that ultimately Einstein will prove to be right, because the present form of quantum mechanics should not be considered as the final form. There are great difficulties . . . it is the best one can do up till now.'

This is very different from the attitude of the other men of the Copenhagen school.

1. First class difficulties

Dirac divided these difficulties into those of the first class and those of the second class. The second class difficulties are rather practical but the first class difficulties appear immediately when you look at the fundamental axioms of quantum mechanics. Here I am taking from Dirac's own great book *Quantum Mechanics*:

- '... if the measurement of the observable ... is made a large number of times, the *average* of all the results obtained will be ... [such and such]
- ... any result of a measurement of a real dynamical observable is one of its eigenvalues...
- ... a measurement always causes the system to jump into an eigenstate of the dynamical observable which is being measured.'

And you see here in this word, *average*, already the appearance of statistics in quantum mechanics. It's not the intention here to predict what really happened on a given occasion but only probabilities of what will happen and average results of the many repetitions of the experiment. Dirac said about that:

> '... if you feel uncomfortable about having indeterminacy in the basic laws of physics, you are not alone in that feeling. Very many people do. I do. Schrödinger and Einstein have been very much against it all along. But one has to accept that it is the best one can do in the present state of knowledge ...'.

Well, as well as indeterminism, and in fact in my opinion more important than indeterminism, there's another feature that emerges very clearly from these physics axioms and it appears in the repeated use of the word *measurement*. You will think from this that physics has become dedicated, restricted to piddling laboratory operations. Why all this interest in the laboratory? What about the big world outside the laboratory? Are measurements going on there? If not, we have nothing to say about what is happening there. Moreover, this concept of measurement on analysis becomes extremely slippery.

Why was it that the founding fathers retreated from any attempt to describe the quantum mechanical system in itself in realistic terms, and fell back on this idea of describing only the results, interventions from outside the system? Interventions from a world in which things like apparatus and experimental results have meaning, from a world, in fact, which is a classical world, so that this approach involves the division of the world into a quantum world on the one hand, and a classical world on the other hand, and the division between them, which is far from clear, a division which I call the *shifty split*.

The root of the problem which led to this retreat from realism is in the superposition principle of quantum mechanics. If A and B are two possible states of a quantum system then you can form a whole family of states $\alpha A + \beta B$ where α and β are complex numbers. Now this combination is not a trivial one; in particular it is not the sort of combination you meet in probability theory where you combine two probabilities into a single probability distribution. It does not mean either A or B; in some sense it means both A and B at the same time and that is evidenced from the existence of interference phenomena. If one of those states is a plane wave going in one direction and the other a plane wave going in another direction you know that such a superposition has interference minima; for particles that could get here with either A or B cannot get here with $\alpha A + \beta B$. In some sense the two states are there simultaneously and not in any trivial way because of the interference.

But superposition is absolutely essential in atoms. The Schrödinger equation describes the quantum structure and has the superposition principle built in it, but it is embarrassing for large objects. What could it possibly mean, when A and B are big systems like pens or tables or chairs, a state in which we have both one configuration and another configuration at the same time?

This difficulty, this obscurity, was dramatized by Schrödinger in his famous 'cat' example. He imagined the situation where you have such a state $\alpha A + \beta B$. This A might be a radioactive nucleus which has not decayed and this B is a state in which it has decayed, and he coupled this up into an apparatus of some kind which looks in one way or another to see whether the decay of atoms has not occurred, and that apparatus is thrown into a state T associated with A or F associated with B: $\alpha AF + \beta BT$. T and F are two different states of the apparatus, and in fact they are Thin and Fat states of Schrödinger's cat. This apparatus contains Schrödinger's cat and in such a way that it will be fed or hungry according to whether or not a certain given atom has or has not yet decayed.

This is the experimental setup (figure 2):

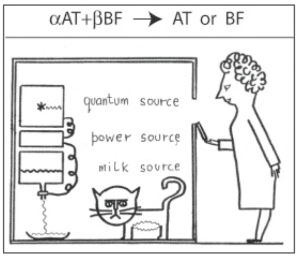

Figure 2. Bell's version of Schrödinger's cat problem.

The radioactive source is here; it may or may not decay, if it decays in some different period of time, about supper time, the signal is taken to an amplifier which is taken to a tap which may or may not release some milk which the cat may or may not have to drink and therefore may or may not afterwards be hungry or fed.

The quantum mechanical analysis of this situation depends on where you put the shifty split. If the split is put here[1] with the cat on our side, so to speak, there is no particular problem: the cat is like the classical one that is thin or fat; but if the split is put here [2], this state is described by the superposition of the thin cat and the fat cat; both are present in this case, and that's a thing that the cat knows very well whether it is either thin or fat. From the cat's point of view we have either this (AT) or that (BF), whereas from the wavefunction point of view we have this (AT) and that (BF), and the step from 'and' to 'or' is a non-trivial one because interference phenomena disappeared. In the original version of the cat experiment, we know it was poison that was released and not milk, and I have always been against that version because in that case even the cat does not know what is going on!

The situation with this *shifty split* is further illustrated in this picture (see figure 3).

Here imagine an experiment in which a so-called electron is passed through a small aperture and falls upon some kind of detector, say a photographic plate. Now, we know that the dynamics of this situation simply cannot be described in terms of classical particles moving along classical trajectories; we have to solve here Schrödinger's wave equation. But when this wave impinges upon this detector, what we see is not in the least like a wave: we see isolated spots, one at a time, so this picture builds up in the course of time, one there … one there … one there … one there … as the experiment is repeated. We see spots not waves, and here you see this terrible division of the world into a wavy part and a spotty part, and this awkward shifty division of the world has to be made.

In this level of analysis some measurement occurs[3] here, at the interface between waves and events. But if you look at these spots with the magnifying glass, you see they are

[1] At this point Bell draws a line separating the apparatus from the cat.

[2] Obviously at this moment he draws a line between the cat and the rest of the figure.

[3] In pronouncing the following sentence Bell indicates one after the other, as his argument is developed, the four stages of the processes represented in the figure.

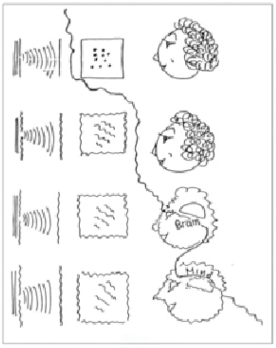

Figure 3. A picture illustrating the problem of locating the shifty split when human perceptions are involved.

not mathematical points: they are highly structured objects. The photographic image is a complicated thing, an agglomeration of little silver grains. If you want to know more than the rough location of such spots, you must refine your theory into a theory of the photographic process, and the only way to do that in our present theoretical scheme is to displace this shifty split, to take the photographic plate into the wavy part described by Schrödinger's wave and leave out all the rest of the world. But of course you cannot stop there: the eyes are also making their contribution to what you see, they are also physical systems and so you would say we want to take into account the role of the eye in controlling what you see. You must displace it still further beyond the optic nerve towards the brain.

The brain also is a physical system, we know a great deal about the electrochemical interactions going on in there, and if we want to give a serious account of those, the split is displaced still further between the brain and the mind; and there perhaps we can really rest, because the mind we do not think of as made of atoms evolving by quantum mechanics. It is something different. And so here is the ultimate location where such a split might occur between a wavy part of the world in which you have potentialities rather than events, and a classical part of the world in which you have events, because although most of us are sometimes in two minds about something, most of the time we are more or less in a definite state of mind.

What physicists have speculated, very respectable physicists have speculated, is that this is what is implicit in the axioms of quantum mechanics, basically their logical conclusion. There is not a shred of experimental evidence for that; we all know that somewhere out here you find that you can make this split and calculate the results and get agreement with experiment.

So this is the first class problem of quantum mechanics, the ambiguity:

> Nobody knows what quantum mechanics says *exactly* about any situation, for nobody knows where the boundary really is between wavy quantum systems and the world of particular events. This is the first class problem and, as we all know, it does not matter at all in practice.

As with Schrödinger and his cat you learn where to put this split in particular situations: at one side of the photographic plate or at the other, depending on how interested you are in details. And we know we can get away with that. It doesn't matter in practice and it is very much a question of debate among physicists whether or not you should worry about a question of principle before it becomes a question of practice.

2. The Copenhagen interpretation and complementarity

A standard resolution for these difficulties, at least the standard resolution when I was a student and perhaps even now, went under the name of the Copenhagen Interpretation. Now, the Copenhagen Interpretation is a bit like the Church of England! There's room for everybody in the Copenhagen Interpretation, that is one of its merits! On one side you have simply the practical rules of quantum mechanics. Many physicists these days belong to the Copenhagen position; they simply mean that, like everybody else, they know what the rules are and they apply them to particular situations in a professionally approved way. They are pragmatic men and I think that in that sense we are all of the Copenhagen school; but then there is a whole spectrum which ends up in something called the philosophy of Complementarity. And complementarity does not so much tell you where the shifty split is, but is designed to reconcile you to the ambiguities of the subject, to make you feel good about the ambiguity, to make you feel even sophisticated in tolerating ambiguity. And that is another aspect that is very successful and it allowed a whole generations of physicists to get on with their job rather than to sink into a state of permanent anxiety about the problem.

I cannot explain the philosophy of complementarity, I never got the hang of it, and I will content myself with reading you some things from some of the masters that may give you some hint of what it is. This is Bohr:

> '... the situation met with in modern atomic theory is entirely unprecedented in the history of physical science ... for a parallel to the lesson of atomic theory ... we must in fact turn to quite other branches of science, such as psychology, or even to that kind of epistemological problems with which already thinkers like Buddha and Lao Tse have been confronted, when trying to harmonize our position as spectators and actors in the great drama of existence.'

You see here that physics is touching on very deep questions and this theme has been developed by other people, notably by Capra, into the idea that indeed physics, modern physics, has to some extent been anticipated in ancient oriental philosophies. However, Bohr was not of that persuasion. In lecturing to public audiences he would say things like that which spice up his discourse, but he would immediately pull back. He says:

> 'still the recognition of an analogy does in no way imply acceptance in atomic physics of any mysticism foreign to the true spirit of science, but on the contrary it gives us an incitation to examine whether the straightforward solution of the unexpected paradoxes met in the application of our simplest concepts to atomic phenomena, might not help to clarify conceptual difficulties in other domains of experience.' (Bohr, 1958)

So you see that Bohr was not looking for help from the ancient oriental sages, he was proposing a solution to their problem.

This is Rosenfeld who thought of himself, I think, as a chief disciple of Bohr:

> 'Bohr had great expectations about the future role of complementarity. He upheld them with unshakeable optimism... on one of those unforgettable strolls during which Bohr would so candidly disclose his innermost thoughts, we came to consider that what many people nowadays sought in religion was a guidance and consolation that science could not offer. Thereupon Bohr declared, with intense animation, that he saw the day when complementarity would be taught in the schools and would become part of general education and, better than any religion, a sense of complementarity would afford people the guidance they needed.' (L Rosenfeld 1963 *Physics Today* 4 October)

I believe that this has not happened! Even though, when I was at school, I heard a great deal more than I wanted about complementarity, it did not go so far.

What did Dirac think of that? Well, no one knows more than Professor Mehra what Dirac thought:

> 'Though Dirac appreciated and admired Bohr greatly, he told me that he did not find any great significance in Bohr's principles of correspondence and complementarity because they did not lead to any mathematical equations . . . '.

I absolutely endorse this opinion. I think that what is needed in quantum mechanics is not philosophy but the missing equation, which defines mathematically where the split is or replaces the need for the split. Continuing with Dirac's opinion:

> 'These class one difficulties do not really worry the physicist [This, by the way, dismisses Bohr and Einstein from the community]. 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.'

> '. . . 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 . . . '

He saw obstinate transitional stage even as regards the fundamental concepts of quantum mechanics (...):

> '. . . 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.'

Now that's his opinion on the first class difficulties. He gives this much comfort to those people who are worried about them, he sees that they exist and are difficult. Many of the founding fathers would not have admitted that.

> '. . . 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.'

> '. . . the trouble is that certain quantities that ought to be finite are actually infinite. Physicists have found that there is a way to handle these infinities according to certain rules, which makes it possible to get definite results ... the renormalization method.'

3. The second class difficulties and the attempts to overcome them

I am not going to describe these second class difficulties. They arise when you try to make a relativistic version of quantum theory. You find that you are obliged to start not with the particle, the classical particle, but with the classical field. The classical field has infinitely many degrees of freedom. And so you find that when you do physical calculations the result is very often given by an infinite sum, and the trouble is that this infinite sum very often diverges, so that the result seems to be infinite. Physicists have developed a way of very delicately balancing these infinities one against another in order to get out finite results for experimentally observable quantities. That's called the *renormalization scheme*.

This is what Dirac thought of the renormalization scheme:

> '... with these rules for subtracting infinities you do not really have a correct mathematical theory at all ... just a set of working rules ... '

For him, the first class difficulties he could very happily leave to another generation, but the second class difficulties he was extremely disturbed about and felt that we should all address ourselves to the resolution of them. Dirac said:

> '... The surprising thing is that in the case of electrodynamics one gets results that are in extremely good agreement with experiment ... It is because of this ... that physicists do attach some value to the renormalization theory, in spite of its illogical character ... '
>
> '... 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.' [It is a very strong word!]
>
> '... on the same footing as the successes of the Bohr orbit theory applied to one electron problems.'

We know that Bohr was extraordinarily lucky in getting the right answers for the hydrogen atom and the orbital quantization; and Dirac had the strength of mind to suggest that the (...) Lamb shift, g^{-2} for the muon, and a whole list of probable successes until the 7th or 11th decimal places, are a fluke.

There have been developments since Dirac formulated his opinions as regards both the first and the second class difficulties, and they are not in the direction that he anticipated. Renormalizability, for which he had very little respect, was the guiding light informing the Glashow, Salam and Weinberg theory of electroweak interactions. As far as we know, it is perfectly successful, set the fashion for constructive gauge theories and the QCD and the present standard model, which is enormously successful in coordinating a great deal of data.

At the same time as this development in the renormalizable theories, which proved very relevant for experiment, there have been developments towards finite theories of the type that Dirac loved. Unfortunately, they have not proved relevant for experiments, at least so far.

I quote here a summary from our director, Professor Salam who, along with his colleagues, played a leading role in that development. Abdus Salam says:

> '... it now appears that there is, indeed, a class of field theories which are perturbatively finite to all orders ... (certain) non-Abelian gauge theories of supersymmetric type. Whether such theories are physically relevant is not yet known.'

I think this was written in 1987 and I believe it is still not yet known, in the sense that no relevance has yet been found. Continuing with Salam:

> '... Even more important ... there is the promise—brought to a near proof—that closed-string supersymmetric field theories, whose long range excitations must contain quantum gravity as well as electro-nuclear interactions, may give rise to finite matrix elements. If this is really proved ... and if such theories prove to be physically relevant, Dirac would be fully vindicated.'

There has not yet been time for this near-proof to become a proof. As far as I know, it remains a near-proof. There has not yet been time for it to become clear whether the string theories are or are not relevant for physics. I think that the ultimate fate is much less promising now than it was one year ago because people have found out that string theories are infinitely flexible, if you will excuse the words. At the beginning, it seems that there are really one or two to choose between, and we would just have to decide which, but as often happens when people look harder, they find more possibilities, and now there are really infinitely many, and I have the impression that people do not know precisely where to go next in that area, although the mathematics is indeed very complicated.

And this is an interesting remark, that Dirac would be fully vindicated if these theories proved relevant and finite. It was even an enormous mystery why was it that renormalization proved such an infallible guide to port. That would be a great mystery; many looking backward could see that renormalizability was in some way an anticipation of finite field theories, although precisely something that we do not see at this moment.

4. Overcoming the first class difficulties

There have also been developments on the first class side. Again they do not fulfill Dirac's expectations in this sense. He thought that technical development in quantum theory would eventually illuminate the first class difficulties. And they haven't. The developments that I just told you about on the second class side have not touched at all on the first class side, and the first class developments are separate. And it is a particular pleasure to me to talk about them here, because the essential idea there comes from here: it comes from Ghirardi, Rimini and Weber. The idea goes back at least to 1984; perhaps the most accessible early paper was this paper in *Physical Review* (1986 **34D** 470).

I have written down here a certain number of other people who have been associated in one way or another with these developments.

- Benatti and Grassi are collaborators, now, of the original group;
- Gisin and Diosi were pioneers in applying stochastic differential equations to the measurement problem and that has proved very relevant;
- Pearle is a man who has devoted almost his entire career to trying to eliminate the first class difficulties and so he was very well prepared to join these people to collaborate with them.

Berkeley has perhaps the greatest priority of them all. Berkeley was an eighteenth century Irishman. He was the Bishop of Cloyne in Dublin. He was also a noted philosopher of that day, and rightly or wrongly he became famous for the idea that 'to be is to be perceived'. He was very concerned about the relation between the observed object and the observing subject, and he convinced himself that something could not exist without being observed. And so you see, he is the forerunner of those modern physicists who insist that the quantum system must not be spoken about as something in itself, but only as something which may or may not be behind the results of measurements, of observations. The philosophy of Bishop Berkeley was most succinctly summarized by Ronald Knox. The point is that Berkeley remembered something which the founding fathers of quantum mechanics, those who were inclined to the idea that observation is the essence, they forgot something which Berkeley remembered, perhaps because he was a bishop.

Berkeley:

'There was a young man who said: God
Must think it exceedingly odd
If He finds that this tree
Continues to be
When there's no one about in the quad.'

[A quad is an open space in an English college around which the buildings are built.]

'Dear Sir,
Your astonishment's odd
I am always about in the quad
And that's why the tree
Will continue to be
Since observed by
Yours faithfully
GOD'

So, the thought of Ghirardi, Rimini and Weber, although they may not recognize it immediately, is that God is always looking, and we should apply the axioms which Dirac said are of quantum theory to the observations continually being made by God. We have to decide then what is the problem with that and we have to make hypotheses there because this is not actually written in the Holy Scriptures.

We suppose that God observes the positions of particles and does so randomly, choosing this particle or that at random and at a random time; and He or She is not very interested in any particular particle. He looks only upon once every 10^8 years per particle. And this look, according to the rules of quantum measurement, causes the jump:

$$|t\rangle \rightarrow j(\mathbf{r_n} - \mathbf{x})|t\rangle,$$

the state jumps into a state which is concentrated somehow at the result $\mathbf{x}$ of this observation. And there is the hypothesis that the probability of that is specially big where the wavefunction in the $\mathbf{x}$ representation is large and that is represented by supposing this probability distribution per unit time, per unit volume, per particle, for the jumps:

$$\mathrm{d}t\, d^3x \langle t||j(\mathbf{r_n} - \mathbf{x})|^2|t\rangle.$$

And what I said about 10^8 years comes from the integral

$$\tau^{-1} = \int \mathrm{d}^3x |j(\mathbf{r_n} - \mathbf{x})|^2$$

being about 10^{-8} per year; and for the width of the function $j(\mathbf{r_n} - \mathbf{x})$ it is assumed that God is not all that interested and does not look very closely so there's still some uncertainty in the position of the localization which is taken to be 10^{-5} cm (see figure 4).

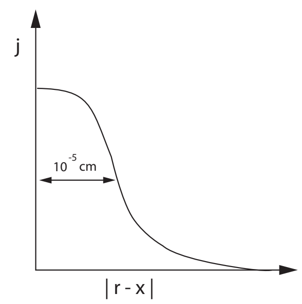

Figure 4. The shape of the function j characterizing the localization process

These numbers, 10^{-8} and 10^{-5} they are new constants of nature like the fine structure constant which have to be fitted with experiments and at present they can only be determined to a very gross order of magnitude.

You see that Divine intervention is rare and mild in small systems, the extension of the localizing function is big compared to atoms and the localizations do not happen very often. This theory does not disturb you as regards those predictions of quantum mechanics which have been tested with high precision, like the theory of the spectrum of Helium. However, it makes cats fat or thin in 10^{-8} seconds. That's because, if you remember the thin and the fat cat, to pin down the position of any one atom in this region does make the distinction between the thin and the fat cat. And there are more than 10^{23} atoms in even a small part of the cat and there are some more of those atoms to pin down by this jumping process in about 10^{-15} seconds and so you see that the embarrassing superpositions go away very quickly. So that's a rather neat solution of the problem.

There have been some developments in that solution which I will mention very briefly. Instead of a few big jumps we have many small jumps. Instead of localizing your particle rather sharply you may localize it rather grossly and again and again and again and you find that that's just as good. And if you carry that to its limit, you have got the idea that the wavefunction does not make jumps but a sort of Brownian motion for which the changes are small and random steps. And this leads to the Continuous Spontaneous Localization version of the theory: CSL, which is represented by an Ito Stochastic differential equation. Here then is the successor to the Schrödinger equation:

$$\mathrm{d}\Psi = -\mathrm{i}H\mathrm{d}t\Psi + \sum (q_n - \langle q_n\rangle)\,\mathrm{d}\xi_n\Psi - \frac{\gamma}{2}\sum (q_n - \langle q_n\rangle)^2\,\mathrm{d}t\Psi;$$

$$\overline{\mathrm{d}\xi_n \mathrm{d}\xi_m} = \gamma\delta_{n,m}\mathrm{d}t$$

The increment $\mathrm{d}\Psi$ in a small time interval is given by the ordinary Hamiltonian term: $-iH\mathrm{d}t\Psi$. Then you have a new term: here is one of the degrees of freedom, q_n (Ψ is a function of the q_n's) minus the expectation value of q_n in this wavefunction Ψ; so you see the theory is nonlinear, not because of Ψ appearing after $\mathrm{d}\xi_n$ but because it is involved in the calculation of the expectation value; $\mathrm{d}\xi_n$ is a small random increment, a small bit of a Brownian motion. And then there's a third term here, which involves the square of q_n minus the expectation value and a constant gamma which is related to the rate of the Brownian motion. The mean value of $\mathrm{d}\xi_n\mathrm{d}\xi_m$ is $\gamma\delta_{n,m}\mathrm{d}t$.

Now it is at this stage that it is easiest to tell you about the repair of a detail that I did not mention. The original GRW theory did not respect the symmetry or antisymmetry for identical particles. But you can now easily repair it because instead of applying this localization process to the position of particles, you can hit something which is symmetric in all the particles. For example you can replace q in this equation and instead of q enumerated by n, you can have the density ρ enumerated by the position x. And you must in fact define a density, you must average over a little region, and that introduces a new parameter into the game, this $\bar{\rho}$ up here:

$$q_n \to \bar{\rho}; \qquad \bar{\rho} = \int \mathrm{d}^3r' f(r - r')\rho(r').$$

This f is an arbitrary function, which takes a mathematical ρ and averages it. So $\bar{\rho}$ goes in the stochastic equation and you have a new equation which has all the same good properties, and which in addition can look after identical particles.

That's, in my opinion, a very good solution for these problems in the context of nonrelativistic quantum mechanics. And if I were teaching nonrelativistic quantum mechanics that is the line that I would take. Instead of all that talk I would have this new equation and you would see that from that new equation the world is entirely represented by the wavefunction, you will see that big objects have definite configurations, the wavefunction is always narrow with respect to the macroscopic variables and you would see that little objects like hydrogen atoms are fully represented by the Schrödinger wavefunction. The hydrogen atom in its ground state would be perfectly spherical; you wouldn't think of finding a little particle somewhere in the cloud: the perfectly spherical wavefunction would be the complete and final representation. Nevertheless, cats would always be either fat or thin at least as we don't look more closely than 10^{-8} seconds.

5. The relativistic challenge

What about relativity theory? Well, if you try to apply the same ideas to relativistic quantum field theory there is one very simple and rather cowardly way out: just define a density. A density averaged over a little region is a perfectly good quantity in quantum field theory, but the averaging is essential. You can do that and then you would have a theory which For All Practical Purposes (FAPP) would be the same as ordinary quantum mechanics, except that the shifty split would be defined mathematically rather than according to the whim of the theorist. But since we know that that split is rather displaceable anyway without changing much the result, the fact that we pin it down mathematically is not going to change much the practical predictions of the theory. So this theory will agree with the ordinary relativistic quantum field theory used in the usual model way. To that degree it would be Lorentz invariant, because it gives the same results for experiments, including the Michelson–Morley experiment. But this theory would only be Lorentz invariant in the sense of Fitzgerald, Larmor, Lorentz and Poincarè. Before Einstein, people had devised a way of understanding that you would not be able to detect the motion of the ether, say in the Michelson–Morley experiment, but nevertheless the theory that these people had in mind had an ether in its fundamental construction.

And of course all coordinate systems were not equal, there was one which was in some sense fundamental. And in this theory there would be such a fundamental frame because when you define an average over a certain region of space you pick out a certain frame as a preferred frame. And moreover, if you try to make the theory invariant by making this averaging over the region go to a point, then you know that in quantum field theory the operators at a point are terrible. In particular, the quantum fluctuations would be infinite, and what does it mean to narrow down fluctuations of something which fluctuates infinitely? When you do that, you must feel that you are doing rather serious damage to the system, not just rather gently taking it. So one can anticipate that there will be troubles when you try to make this theory Lorentz invariant, not just for all practical purposes but deeply, in the sense of Einstein, eliminating entirely any privileged reference system from the theory.

Well, of course these people, Ghirardi, Grassi and Pearle, have started to tackle these problems. Naturally you start with the free scalar field to see if you can make a relativistically invariant Continuous Spontaneously Localized quantum field theory. And in that context there have been much formal progress for generating the Lorentz transformations, and this is very original because the generators of the Lorentz transformation, like the Hamiltonian, must contain stochastic elements. There is a whole new field there for formal work, and much of it has been done. But I think that these people do not yet claim any particular success. I think that they would say that they are in the stage of having interesting difficulties. Now I will start with one of them.

Suppose we tackle this free scalar field theory. To avoid any infinities at the beginning, we regulate it by putting it on a lattice. So instead of a Lagrangian containing an integral we get a sum:

$$L \quad \longrightarrow \quad \sum_n a^3 \dot{\phi}_n^2/2 \; + \; \ldots$$

I take the lattice spacing distance to be a, so there's a volume element a^3 here. And then there's ϕ gradient square over 2, and now I have a discrete rather than a continuous set of variables. Then one applies the GRW jump to this theory, assuming from time to time that, for some of these variables ϕ_n, the wavefunction suddenly acquires an extra damping factor, with a width $1/b$, around some prescribed centre of the jump ϕ'_n:

$$\Psi \quad \longrightarrow \quad e^{-\frac{1}{2}b^2(\phi_n-\phi'_n)^2}\Psi.$$

If you then ask what happens to the mean value of the field over a region, after all we don't care so much for experimental purposes what happens at a point, we only see gross averages. You can calculate what is the effect of that and, since such an extended average will involve many lattice points, in a small period of time there will be many such jumps and we must find how they add up and this is how they add up:

$$e^{-b^2\frac{\ell^3}{a^3}\frac{t}{\tau}(\bar{\phi}-\ldots)^2}.$$

Supposing that the region over which ϕ is averaged is of dimension ℓ there is a factor ℓ^3/a^3, because that's the number of lattice points, the number of possibilities for jumping. There is a factor t/τ where t is the time lapsed and τ is the mean time between jumps per site, and b of course. And then there is this mean field $\bar{\phi}$ and I won't write down the rest of that which tells you where it is localized; I rewrite the previous formula as follows:

$$e^{-\alpha\ell^3 t(\bar{\phi}-\ldots)^2}.$$

You see the bigger the region that you consider, the bigger the time, the more localizations you have. And that's why it is that macroscopic quantities can always be well localized, whereas those on the microscopic scale are not. And so the constant α describes the rate of localization per unit volume, per unit time.

But now look what the Hamiltonian is for this Lagrangian:

$$H \quad \longrightarrow \quad \sum_n \Pi_n^2/2a^3 \ + \ \ldots$$

The quantity a^3 in L plays the role of a mass and when you go from the Lagrangian to the Hamiltonian, you have the momentum square divided by the mass, rather than the velocity square multiplied by the mass, and this $1/a^3$ in H is the source of a difficult problem. So, when you multiply the wavefunction by a factor like

$$e^{-\frac{1}{2}b^2(\phi_n-\phi'_n)^2},$$

you introduce kinetic energy into the system. I didn't tell you that before, that this theory does not conserve energy. When you look at the rate of energy production on a non relativistic model, you find that you produce only a tiny fraction of the three degree background energy during the course of the evolution of the universe. So there is no good looking here for a solution of the energy problem.

But something terrible happens when we try to do it relativistically, in such a model. Squeezing the wavefunction increases the kinetic energy, and if you calculate the rate of energy production per site, you find of course that this is proportional to

$$\frac{b^2}{a^3}\frac{1}{\tau}.$$

Here there is b^2, the degree of squeezing; there is $1/a^3$ because the mass comes into that calculation; and there is $1/\tau$, because $1/\tau$ is the rate per unit time. So this is the rate of energy per site. But this happens at each site, so that the rate of energy increase per unit volue is proportional to:

$$\frac{b^2}{a^3}\frac{1}{\tau}\cdot\frac{\ell^3}{a^3}\cdot\frac{1}{\ell^3} = \frac{\alpha}{a^3}.$$

And if I pass to the relativistic limit, α has to remain finite, because I want these things to be localized quickly, while I have to make a zero, and then you see that the rate of energy production goes to infinity. Now, letting a going to zero is going toward the real quantum field theory where there are infinitely many degrees of freedom per unit volume. And what you see here is the second class difficulties, rising their ugly head in the context of an attempt to solve the first class difficulties.

Now, in the early days of quantum field theory, there were of course things like that and people could be very despondent. I hope you know it worked out in the end, and we can envisage ways it might work out here; maybe someone could find some way of sweeping the infinities under the rug and going ahead nevertheless, or maybe some of the finite field theories might help.

There's a whole line of research here which has been opened up as the Ghirardi–Rimini–Weber jump. And it remains to be seen whether it will work out well or ill. In any case there is a program where before Ghirardi, Rimini and Weber the fields were rather moribund.

6. Conclusion

I end with another Dirac story because I know that many people here are practical physicists that know very well that you can get along without paying any attention to these first class difficulties and many of these people are so confident in their practice that they can even dismiss the existence of the first class difficulties. I am always wanting to argue with such people, but I have learned by experience that there is no hope whatever of converting them. And I think that those people would only really admit the existence of these problems when they are solved, and then when they see a nice definitive relativistically invariant solution, then they would look back and say: 'Yes, there was a problem'. And Dirac was very conscious of how differently things could look according to whether you were looking forward or backward, from one side or another and he had a tremendous ability to dissociate himself from the common perception, from the obvious idea and that should be an object lesson to all of us, and I will tell you another story which illustrates that. There is another Heisenberg–Dirac story which is perhaps even more relevant to my purpose here. It is said that they were once walking together in the country, and Heisenberg noticed that some sheep in a nearby field had been shorn. It was a cold day, and he felt sorry for them. He said 'Look, Dirac, those poor sheep have been shorn'. Dirac looked, and considered, and after a time replied 'Yes, at least on this side'.