The Lesson of Quantum Theory, edited by J. de Boer, E. Dal and O. Ulfbeck


# Measurement in Quantum Theory and the Problem of Complex Systems

Philip W. Anderson

Princeton University
Princeton, New Jersey, USA

## *Contents*



## *1. An approach to the Bohr–Einstein debate*

Every physicist who has studied the quantum theory is familiar with the great debate on the uncertainty principle between Bohr and Einstein which began at the 1927 Solvay Conference but which then simmered on for many years. Perhaps the commonest view is that Bohr simply won hands down; in Einstein's somewhat mistranslated phrase, God *does* "play dice with the world" and the outcome of a quantum measurement is truly uncertain. In a way, this view corresponds to the truth—at least to the pragmatic truth that now, after 60 years, there is no doubt in any quantum physicist's mind that the whole quantum theory, including the original quantum prescriptions of measurement theory, are correct in describing the results of experiment. Why, then, re-open the question?

It may be slightly provocative, but surely in a spirit Bohr himself would have enjoyed, to suggest that many of those who have thought beyond the textbook level have come to the conclusion that the really correct answer to which one was right is —neither! It is not true that quantum mechanics is a purely probabilistic theory, any more so than statistical mechanics, for instance. That it is, seems to have been Bohr's point of view, in so far as we can follow his statements about complementarity as a general philosophical principle. In fact, generations of philosophers have been misled, probably against Bohr's intent, to give a deep meaning to this "uncertainty". On the other hand, so far as I can see, one cannot avoid the probabilistic results of measurements on atomic scale systems, as Einstein wished. Einstein's reasons are clear enough: he believed firmly that general relativity was the appropriate model for future theories, with its fundamental emphasis on the geometry of space-time, and it disturbed him deeply to have that geometry indeterminate and fluctuating—in particular, how can an indeterminate geometry properly maintain causality? The difficulties of quantizing general relativity vindicate Einstein's misgivings to some extent. Whether or not Einstein would have felt comfortable with the present liberties being taken with the structure of space-time is hard to know; one would have thought he would have felt a proprietary interest in four dimensions. It is true that we are beginning to return to his geometrical view and that at some ultimate scale everything may yet change. But really that is a bit of a diversion.

I want to talk here about simple non-relativistic quantum theory, although I believe everything I say is equally valid in relativistic field theory at any scale short of the Planck mass, e.g. during most of the events which took place during the "big bang" including the symmetry-breaking phase transitions.

What I consider to be the correct approach to the Einstein–Bohr debate began very much with one of my own personal idols, Fritz London. Interestingly enough, London trained originally as a philosopher, and one sees, in following his career, a certain philosophical theme: how can we deal with the quantum theory at the level of macroscopic objects? He is most justly famous for his ideas on the two macroscopic quantum phenomena which most severely test quantum mechanics in this regime, superfluidity and superconductivity; but I refer here to a seminal paper in the theory of measurement by London and Bauer (1939), who I believe were the first to take the point of view I advocate here, that the central problem of measurement theory is not the quantum mechanics of atoms, which is simple and easy, but the fact that macroscopic everyday objects are very difficult indeed for the quantum theory to deal with properly. To mis-quote another famous Dane: "The fault, dear Horatio, is not in our atoms but in ourselves."

The sticking point is twofold. The first problem is that the quantum theory is in principle *not* probabilistic but deterministic. The equation for the time variation is a simple deterministic one:

$$\mathrm{i}\frac{\partial \psi}{\partial t} = H\Psi, \tag{1}$$

where $H$ is a given—in principle—function of a maximal set of arguments of $\Psi$ at the present time—or in relativistic theory, at a space-like surface. Of course, there is a mathematical equivalence between eq. (1) and a sum over classical trajectories, which resembles a stochastic process, but every attempt to replace quantum mechanics with a truly stochastic theory ends in failure. Thus in principle, given an initial $\Psi(t=0)$, we know precisely what $\Psi$ is. What is more, if quantum mechanics is a complete theory, $\Psi(t)$ should be a complete specification of the physics including measurements, though not necessarily vice versa.

What London points out to be the second prong of the problem is that the $\Psi$ involved must be taken to be the wave function describing the measuring apparatus and the experimenter as well as the experiment. It is philosophically repugnant to suppose that the prescriptions of quantum mechanics are different depending on whether we include the apparatus and the observer in the system or not. I follow Everett (1957) in his important, if perhaps somewhat incomplete paper, on "relative states" sometimes called "many worlds". Everett gives the following statement of the question: "The wave function must be taken to be the basic physical entity... interpretation comes only *after* an investigation of the logical structure of the theory", hence also only after an investigation of the quantum mechanics of large or rigid or sentient objects. A clear necessity is that $\Psi$ must tell us what it may know about itself, not vice versa—there is no necessity that $\Psi$ be a measurable thing in general, only that it describes what we actually observe.

The large objects which are involved in a conventional measurement behave in essentially non-quantum ways in at least three respects, each of which, at one time or another, has been conjectured to be the source of the anomalies of measurements.

(1) They must be or appear irreversible, so that at least the result of the measurement is recorded after it has taken place, not predicted before; measurement is of the essence of an irreversible process in actual experimental fact.

(2) They must be rigid; in order to make the appropriate kinds of measurements (we will see that "rigidity" can be defined in a much generalized way) they must have the properties of pointers.

(3) They must be sentient, to communicate it. Now (3) involves us in enormous difficulties with the different levels of computational complexity and with unknown aspects of the brain; fortunately, as London and others have pointed out, measurements can be made and recorded by purely automatic machinery, and one hardly sees physics depending on the final process of peeking at the instrument. I believe both (1) and (2), on the other hand, play an important role.

## 2. *Canonical Stern–Gerlach experiment*

It is important to think a little about the actual measurement process at this time. Let us start for instance with the canonical Stern–Gerlach experiment, which has been described as using the position variables of the atomic beam as an instrument to measure the spin of the atoms (fig. 1). But we must realize that just separating the two beams is not a measurement per se; a measurement must in some sense, as everyone agrees, destroy the possibility of interference between the two beams.

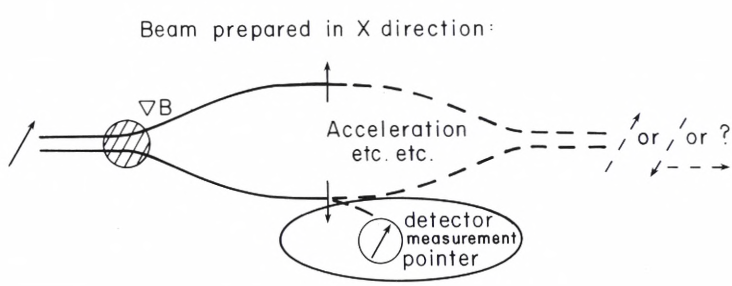


Fig. 1. Stern–Gerlach experiment.

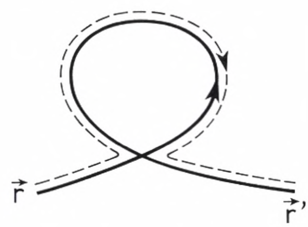


Fig. 2. A self-intersecting Feynman path for an electron to propagate from $r$ to $r'$. Propagation along solid and dashed paths can interfere.

A measurement, according to the old viewpoint which agrees with experiment, definitely sets the relevant quantum number equal to that appropriate to the one beam alone, but interference with the other modifies the values of the quantum number and destroys the property of being in the one eigenstate, so if such an interference experiment remains possible, no true measurement in the canonical sense has taken place. This will be an important observation to the remainder of the chapter. In fact, in a Stern–Gerlach apparatus we very clearly can, even after accelerating the beams and passing them through all kinds of lenses etc., return them to coincidence and observe interference effects. This kind of experiment has been successfully carried out to demonstrate the effects of potentials on neutrons, for instance. (Macroscopic quantum coherence is *not* relevant here. That is enforced by generalized rigidity and minimization of free energy and is irrelevant to measurement theory.) The existence of an interference effect after subjecting beams of particles to all kinds of vicissitudes is the basis of the phenomena of "weak localization" in metals, where we demonstrate interference between electrons travelling in a state and its time—reversed, i.e. precisely backscattered, version. Strong localization, in fact, occurs when this backscattering prevents irreversible loss of coherence entirely (fig. 2). A most striking experiment was carried out first by Sharvin and Sharvin (1981) at the suggestion of Altshuler et al. (1981). In this experiment the interfering pairs of states are carried around an array of holes in the metal, or along an array of wires in a magnetic field, leading to Bohm–Aharanov interference phenomena at half-flux quanta periods between paths for these so-called "cooperons" which take opposite direction around the holes. An excellent review which describes these phenomena is by Lee and Ramakrishnan (1985), from which fig. 2 is borrowed, showing schematically the identical paths of the particle and the backscattered hole which interfere in such a way as to affect the response function which gives the resistivity due to elastic scattering. This response function is closely related to the electron–hole pair propagator. In fig. 2 we see two possible routes for this pair around a hole containing magnetic flux, which can interfere constructively or destructively depending on the flux. Figure 3 shows an example of the delicate microlithographic methods employed to fabricate a network containing many such tiny holes, and an example of such a network (from Bishop et al. 1985); fig. 4 shows some examples of the resulting interference pattern. The amplitude of this pattern measures the degree of coherence and vanishes if the path around the hole is larger

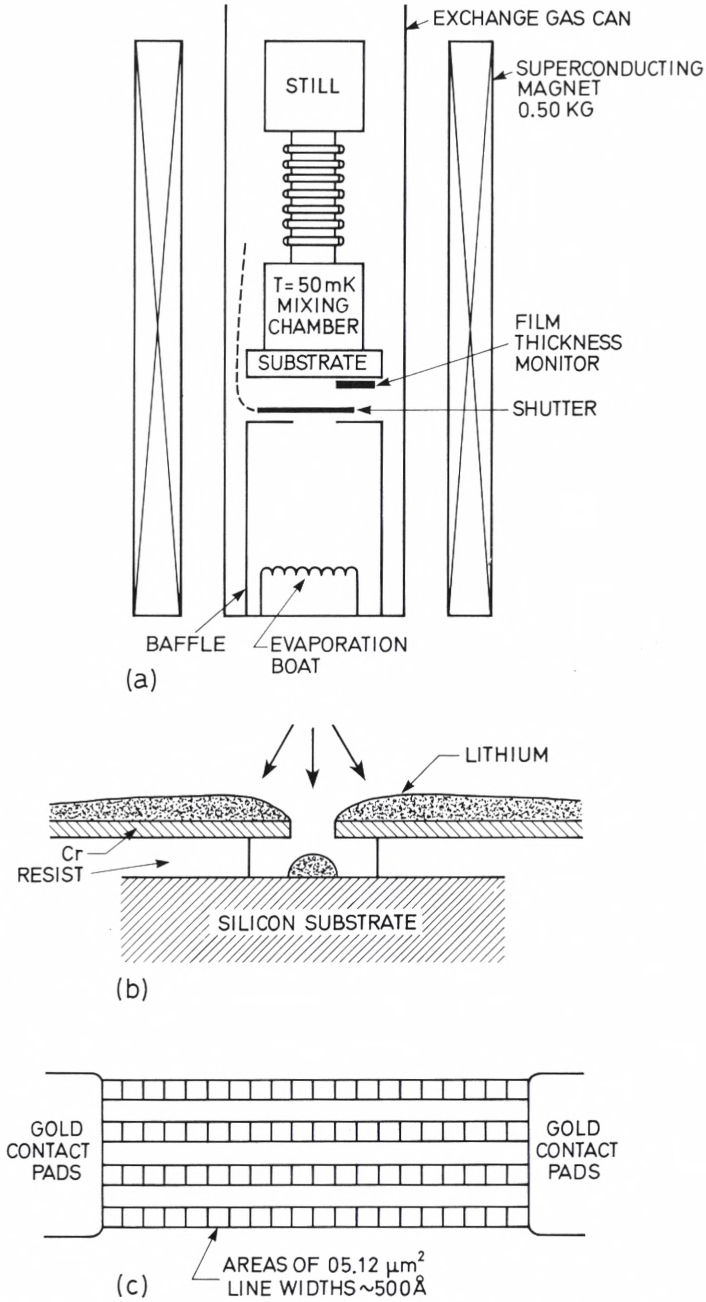


Fig. 3. (a) Apparatus shown schematically; (b) the overhanging photoresist and (c) the grids for our normal metal flux quantisation experiment.

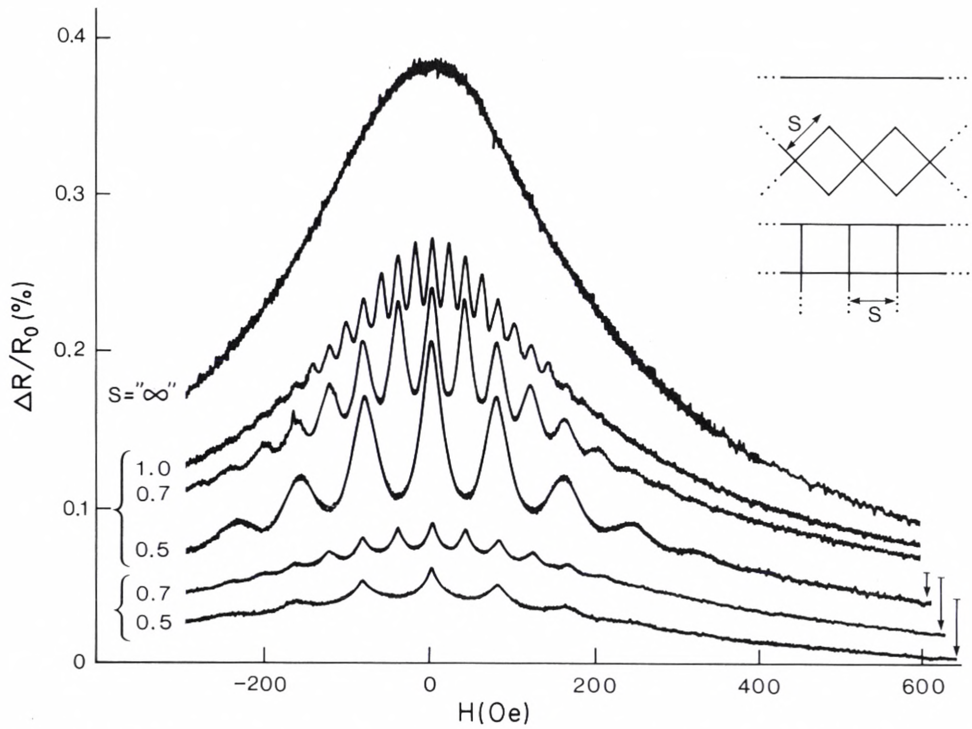


Fig. 4. Relative change of resistance showing interference between particle and backscattered hole as a function of magnetic flux enclosed by the path.

than the "Thouless length" $\sqrt{l_e l_i}$ [$l_e$ and $l_i$ are the elastic and inelastic (coherence-destroying) mean free paths]. This is a particularly clean system for studying the effects of inelastic scattering of the particles on the interference phenomenon. As suggested initially by Thouless (1977), in these experiments it is possible to measure quantitatively the rate of loss of coherence. Fukuyama and Abrahams (1983), Altshuler, Aronov and Khmelnitski (1982) and Fukuyama (1984) have been studying—with some controversy among themselves, but in essential agreement with experiment—the problem of how to quantify the definition of an inelastic collision event which destroys coherence between the electron and its backscattered partner, as opposed to carrying away a given amount of energy or momentum. This is most significant in that, to my knowledge, it is the first example of measurement and prediction of coherence - destroying inelasticity rates, in the true sense. A second interest is that the Russian group has emphasized that the relevent scattering can be thought of as coming from the Nyquist noise, generated by the density fluctuations of the electrons themselves. Because it deals with a completely isolated many-particle system, this work gives the feeling of having finally gotten down to the actual nuts and bolts of the phenomenon of irreversibility itself.

Once inelastic scattering has taken place, we would have to recohere the additional excitations in order to reconstruct the beam, and then each of our

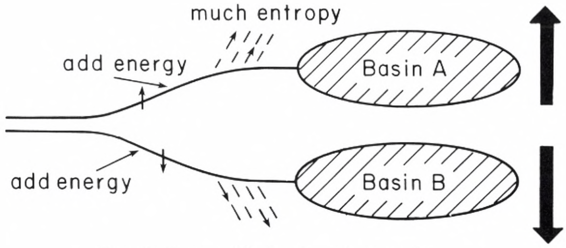


Fig. 5. Two stable basins of attraction.

excitations itself scatters inelastically, etc. Perhaps, in principle, a perfect reversible quantum computer (Bennett 1973) could keep track of every scattering, but in effect the two halves of the beam are by now following different, untraceable paths through wave function space and we must treat the outcome in terms of probabilities and not probability amplitudes.

So far, however, no "measurement" has taken place. For instance, no "particle" has been registered as having gone through a channel. In a general measurement process, we add energy—e.g. accelerate the beams, or cause them to impinge on metastable grains of silver salt, or some other similar scheme. This allows us to enhance the rate of dissipation—i.e. production of entropy—so that the trajectory of each piece of the wave function in Hilbert space can satisfy Liouville's theorem by spreading out in irrelevant variables, but contracting strongly in certain relevant ones which have been coupled to the variables to be measured (see fig. 5). The relevant variables, which are normally the coordinates of some macroscopic, rigid object (a "pointer"), become trapped in a "basin of attraction" which is different for the different values of the original quantum number. By virtue of registering that fact in his neurons, which are themselves switching devices with multiply stable basins of attraction, the observer too becomes entrapped in beam A or beam B, since in essence the Hilbert spaces of the macroscopic objects involved are essentially disparate and no physical operator acting on a few coordinates at a time, expressing the only possible physical processes internal to the system, can connect states in A to states in B. (Of course, an external force can move a rigid pointer by the application of an overall force coherently to every atom of the pointer simultaneously, but this merely redefines "A" and "B".)

## 3. *Phase variable of superfluid helium. Broken symmetry*

These properties of macroscopic systems are much more clearly shown if we look at an example. One example of measurement, and of quasi - degeneracy and of rigidity properties, which has always intrigued me, is that of the phase variable of superfluid helium. This is particularly attractive because this is a broken symmetry variable whose nature is not confused by the presence of many other objects with the same kind of broken symmetry. At this point let me insert a brief primer on broken symmetry in condensed matter physics. [see Anderson (1984) for more details.] This takes the form of five points.

(1) The initial Hamiltonian of our system has a large group G (e.g. local homogeneity and isotropy for a liquid).

(2) The lowest-energy state, or mean field fixed point, has a lower symmetry H (as a crystal, with discrete rotation and translation symmetries).

(3) Hence there is an "order parameter" describing the state, which has "phase angles" free to move in a space isomorphic with the factor group G/H.

(4) Interactions enforce generalized rigidity of these phase angles: there is a (free) energy $\propto (\nabla\phi)^2$. This allows dissipationless "action at a distance" via rigidity, supercurrents, etc.

(5) Hence all atoms are correlated, in highly non-quantum but very familiar behavior—*we* are broken symmetry objects with quasi-degeneracy and rigidity, after all, so we have no trouble in dealing with these concepts except when we try to make them compatible with quantum theory.

Now, in order to explore the relationship of these peculiar—if you are a quantum person—or ordinary—if you are macroscopic—properties to measurement problems, let us do the following sequence of "Gedanken" experiments. [With recent advances in technique perhaps these need not all remain "Gedanken" experiments forever (see Avenel and Veroquaux 1985).]

As a first stage, cool down isothermally a bucket of liquid helium from above $T_\lambda$ to near $T = 0$, starting, if one likes, from a microcanonical ensemble so that the resulting state has a fixed number $N$ of particles. The bucket will subside into its lowest state, which can be written as

$$\Psi = \int \mathrm{d}\phi\ \mathrm{e}^{\mathrm{i}N\phi}\ \Psi(\phi),$$

where $\Psi$ is a quasi-classical coherent state in which the order parameter is taken to be

$$\langle \Psi \rangle = |\Psi_0|\ \mathrm{e}^{\mathrm{i}\phi}.$$

The phase $\phi$ will be uniform everywhere because of the "generalized rigidity" term in the free energy

$$\tfrac{1}{2}\rho_s v_s^2,$$

where

$$v_s = \frac{\hbar}{m}\nabla\phi.$$

I would assert that at this point, already, the different components of the wave function have ceased to interfere and that our bucket is already in the "many worlds" representation; within very fine, calculable limits enforced by the $N$-$\phi$ uncertainty principle, $\phi$ has become a classical variable, and while no experiment can determine what the actual overall value of $\phi$ is—since it is gauge-

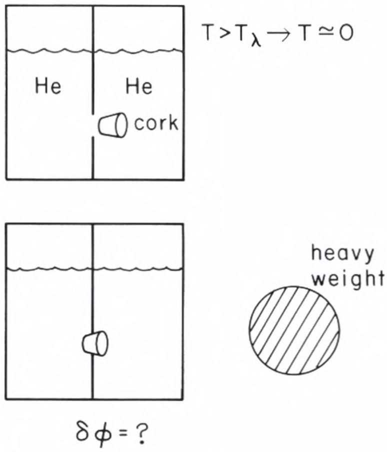


Fig. 6. Change in the condensate phase due to gravitational potential.

dependent—any future experiment will be interpretable as though $\phi$ was fixed. For instance, as a second stage of our experiment, let us suppose our original bucket was nearly divided into two, and at a certain time we close off the septum between the two halves of the bucket, and thereafter subject the two halves to different gravitational histories—e.g. by putting a large object like a locomotive near one half and not near the other half. If the difference in gravitational potential is $\Delta V$, we expect to observe a change in phase of $\Delta\phi = \hbar^{-1}\int \Delta V \, \mathrm{d}t$ (see fig. 6).

Each half of the bucket, we assert, is now in a state in which it has a wide variation of $N$, $\Delta N$, and in which there is coherence between the wave functions belonging to different values of i$N$:

$$\langle N_{1+1} \,|\, \Psi^* \,|\, N_1 \rangle = \Psi_0 \, \mathrm{e}^{\mathrm{i}\phi_1}$$

and similarly for the other half of the bucket. When we reunite them, a measurable interference current will initially flow depending on their relative gravitational histories. This interference current, of course, violates Wigner's "superselection rule" but that is not our problem. What is relevant to measurement theory is that when the two are reunited the experimentalist does not see any interference between different values of the overall phase. This is not quite exactly the case; the phase in each bucket does diffuse slightly, because compressibility forces make the states of different $N$ slightly inequivalent. The time scale, for that, however, is on the order of $10^8$ years.

A third experiment illustrates this point. I would suppose that if the experimenter now cools down two entirely different, non-communicating buckets, each of which initially had a fixed $N$ and no preferred phase, that he, upon opening an orifice between the two, would see initially with equal probability any *fixed* value of the phase difference, and thereafter no experiment he tried could recover the components of the wave-function which started out with different relative phases. He would not see *zero* interference current, which would be the result if he was to average over all of his many worlds.

That is, we have done a macroscopic Stern–Gerlach experiment, dividing the states of a macroscopic object into beams with different quantum numbers.

The above experiments essentially represent the equivalent of a "reference system" for phase. It is rather like the development in the early universe of the first gravitational inhomogeneities—prior to that time, "location" would not have had a measurable meaning.

In another sense this experiment resembles the original big bang. During the earliest moments, present theory suggests that one or several symmetry-breaking phase transitions take place leaving us with a present set of particle interaction symmetries which are much reduced, as well as with values of certain "Higgs" fields which have, in principle, arbitrary phase angles—otherwise, of course, they would have broken no symmetry. Coincidences which, to me, suggest that the whole story is not yet in, leave us with no experimental handles on that phase-angle and no corresponding rigidities; nonetheless we can be quite sure that physics averaged over all possible values of that phase-angle is not what we live with, even though in the initial big bang—corresponding to our helium bucket above $T_\lambda$—the symmetry-breaking field averaged to identically zero and its phase was meaningless, so that below $T_\lambda$ we must be in a linear superposition of all possible quasi-degenerate worlds with different values of that phase-angle. (This is of course, quite independent of the existence of monopoles and the singularities of the symmetry-breaking fields, which are disturbances of the relative, not absolute, values of the symmetry-breaking field.)

## 4. Conclusions

It is symptomatic of the depth of Bohr's thinking that his simple resolution of this deep problem still stands up as the practical way to deal with these hard questions. What I have tried to show is that nonetheless they remain a fascinating subject for subtle experiment and intriguing theory.

Let me close with a quotation from Niels Bohr remarking on the early days of quantum mechanics: "Although the spectroscopic successes of the quantum theory were more spectacular, the explanations of the macroscopic properties of matter were more satisfying and more fundamental". This has always been my view.

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## *Discussion, session chairman W. Kohn*

*Thirring*: Don't you think that some axioms of the orthodox interpretations of quantum mechanics should be modified, namely that the measuring device has to be described in terms of classical variables only. These variables should certainly not be limited to the $p$'s and $q$'s alone because, as you pointed out, the phase of liquid He-II can be used for measurements.

*Anderson*: I turn your question around to a statement and say: I agree with it.

*Weisskopf*: You have connected your remarks with Everett's theory of many worlds, and I am not quite clear how you interpret this. For example: In your experiment with the two buckets one could say that there was an equal probability for any phase difference. But the one observed is the one that happens to be realized in this case. I personally cannot see why one has to assume that all others are also present, but your reference to Everett makes me think that you made this assumption.

*Anderson*: I am afraid I do mean that. I agree with John Wheeler who once said that that is much too much philosophical baggage to carry around, but I can't see how to avoid carrying that baggage.

*Weisskopf*: Why are you forced to carry it along in this case?

*Anderson*: Let's say I carry it along in the philosophical, and therefore not very serious, part of my mind.

*Ambegaokar*: While I thoroughly agree that, for the problems treated by most physicists, many paradoxes are avoided by treating subsystem *and* environment as a single quantum-mechanical system, is it not a rather large extrapolation to apply quantum mechanics to the universe as a whole? Is there some *evidence* that this is a justified or prudent assumption?

*Anderson*: No, there is no evidence. So far there is, in fact, a disturbing absence of evidence in that people have tried to predict a number of singularities in the hypothetical symmetry-breaking field left around: for instance, transition monopoles and things like that, which have not been seen. It is hard to know whether the phenomenon of inflation is to be considered as a confirmation or a further puzzle and, as will be discussed later, there is also the strange question of the cosmological constant. Note, incidentally, that all of this takes place only in the *relative* phases and says nothing about all the other universes.

*Casimir*: I am afraid I don't quite understand the remarks by Thirring and Anderson about a measurement not always having to be reduced to classical, macroscopic observations. I accept that one can use the phase in liquid helium for measurements. But how do you determine that phase? How do you read that instrument? You have got to arrive at something classical. As an intermediate step, you can certainly use things which are not classical, not macroscopic. But I like to maintain that you can always, in the last instance, reduce the measurements to classical phenomena.

*Anderson*: You are, of course, right. I guess I would make two caveats: One is at what point the measurement actually takes place; and the other is that someday we may make a computer entirely out of Josephson junctions, which is itself capable of making the relevant observations.

*Kohn*: You have made extensive reference to macroscopic quantum phenomena. In the last two or three years we have seen this remarkable development of the electron-tunnelling microscope where we see quantum phenomena, namely tunnelling, literally on a one-atom scale. This has been an exciting experimental development. A lot of work has been done since its invention and all kinds of applications have emerged. But I wonder what your thoughts are on the future relevance of that direction of experimental work for measurement theory?

*Anderson*: I haven't really thought about it. The only thing I have thought about a little bit is that it would be amusing to use some of the electronics in the Josephson mode. I don't think that anyone has done it yet. There could be some things that you might be able to do with adjustable junctions of that sort, in terms of turning Josephson currents on and off.

*Ginzburg*: If I understand correctly you have in mind to measure a difference (or gradient) of the phases between two buckets with He-II. But this means that you measure the velocity or the mass flux of helium. These, however, are macroscopic quantities, measured in a macroscopic way. So I do not understand what is the difference here with the usual interpretation of quantum-mechanical measurements using macroscopic devices.

*Anderson*: see answer to Casimir above.

*Peierls*: At the very beginning you stated that quantum mechanics is deterministic because the wave function satisfies a causal equation. Does this not imply that one regards the wave function as a physical object? This leads to all kinds of difficulties.

*Anderson*: I guess my answer to that is: If it's not a physical object—what else have we got? Quite seriously, I am saying that it contains all the physics but some parts of it are hard or impossible to observe because we are in it, and if you like, we don't observe alternative versions of ourselves.