QUANTUM THEORY AND THE INDEPENDENT EXISTENCE OF PHYSICAL OBJECTS

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CLYDE H. EVANS

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Richard Schlegel
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ABSTRACT

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EXISTENCE OF PHYSICAL OBJECTS

By

Clyde H. Evans

We note that quantum theory has reopened consideration of a philosophical problem of long standing: the question of the independent existence of physical objects. In particular, the problem we consider is whether the properties of physical objects can be considered independent and objective, as opposed to being subjective and dependent (upon other factors distinct from the object itself). We examine the results of different theories of physics as they might pertain to our problem. We find that classical physics gives an account of physical objects as being completely objective and independent. With relativity theory we find a collapse of this ideal of complete independence and objectivity; but not a total collapse. In quantum theory we find a further collapse of this ideal, but in a different way from that in either classical or relativistic physics. This leads to the central conclusion of the thesis: the existence of microscopic subjectivism.

This is the claim that on the microscopic level, physical objects do not possess their properties in an independent manner. In some states in which the micro objects can exist (viz, the states of superposition) the object has a given property but has no determinate or definite "value" for this property, unless it interacts with another object. Until such an interaction, the property is undefined and indeterminate: it has no value at all. We consider various possible responses to the claim of microscopic subjectivism. The one that seems most "desirable" is that which accepts the subjectivism on the micro level, but rejects it on the macro level. We examine these proposals in detail. We find that none are completely satisfactory. We conclude that, as of yet, no complete understanding is apparent of how to incorporate this new "fact" of microscopic subjectivism into our overall world view. We note finally that this inclusion in physical theory of a subjectivism at a fundamental level represents a radical departure from previous scientific thought.

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By

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Clyde H. Evans

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DEDICATION

This work is dedicated to Anita, and to to all those who will hopefully benefit from my training, education, and experiences.

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PART I

INTRODUCTION

Quantum theory (hereafter QT) has brought to the fore--in a scintillating way--an old philosophical problem of long standing: the problem of the nature of the existence of physical objects. This problem has many different aspects and can be formulated in quite different ways: Do physical objects really exist at all, or are they just "constructions" out of our experience? Do objects persist while not being observed? Which characteristics of an object are intrinsic to it, and which are "contributed" by an observer? Do all the properties of an object have the same status?

The question with which we shall be concerned is the question of the _independence_ of the various properties or attributes of physical objects. We will seek to discover to what extent the properties of physical objects can be considered _independent_ and _objective_, where this is meant in the following sense: if the exact nature of a property that an object in fact has, can be specified without appeal to anything distinct from the object itself, then this property will be considered to be characteristic of the object _independently_ of anything else; it is considered _objective_ since its complete specification is not _subject_ to any conditions other than those pertaining to the object itself. If it is not possible to provide such a specification for a given property, then this property will be considered as not being completely determined solely by the given object alone; for specification of this property will require reference to something else other than the object. To the extent that specification of the property is subject to these "outside" factors, it is considered a _subjective_ property.

It will be our wish here to study this question in light of whatever we can learn from physics. We hope to determine, in particular, in light of what we learn from physics, if any of the prominent historical solutions to the problem, must be reconsidered. We will attempt to form the best possible (present) understanding, consistent with present-day physics, of the independent/non-independent status of the properties of physical objects, and consider what epistemological or ontological consequences follow from such an understanding.

With such a complex problem it is not surprising that any such inquiry as this is beset with many difficulties, obstacles, and sub-problems. It will, of course, be impossible here to do more than indicate the appearance

of each of these aspects (of our overall problem). By the same token, we shall be obliged to assume a solution to some of these sub-problems, or presume a resolution of some difficulties--ignoring the (sometime formidable) objections associated with the adopted position. This procedure is acceptable for two reasons. The first is methodological: in the overall discussion, some problems appear earlier, others later. The problem of primary importance to us is one of the later ones. Thus, if we did not finally accept a given solution to a question (just so that debate on that issue could be closed) our inquiry would never get off the ground; and we could never proceed beyond consideration of the most preliminary questions. The second reason is pragmatic: the position which we shall adopt is one which is widespread, and for that reason will provide us with a base for profitable and useful discussion. (During our discussion I will not assume the responsibility of indicating, at each critical point, all the possible and actual divergent points of view--except when deemed helpful for the continuation of our discussion. I will presume that the reader will recognize these points and permit me (with the justification given above) to "gloss over" some very controversial points.)

Let us take a brief look at how this issue--of the independence of the properties of physical objects--has become such an important philosophical problem; and let us

try to formulate more clearly the exact nature of the problem.

I suppose that belief in an external world, populated by different objects, is one of the most "reasonable" beliefs imaginable. But justification of this belief proves to be disconcertingly difficult. Some have taken the tack of modern science: ignore the question of the "reality" of the physical world, simply take our experience of the physical world as given, assume that it is an object worthy of study, and then get on with the task of trying to find out as much as we can about it. Others have simply accepted the existence of objects "out of hand" as it were.

But the existence of physical objects has not always been accepted so unquestioningly. It has been argued that though we can never find justification, it is inevitable that we should believe in the existence of bodies independent of our minds. David Hume says in the chapter entitled 'Skepticism with Regard to the Senses' (Treatise, I, iv, 2): "We may well ask, What causes induce us to believe in the existence of body? but 'tis vain to ask, Whether there be body or not?" And further: "'Tis impossible upon any system to defend either our understanding or senses."

Still in this century, philosophers continue to be concerned about the existence of entities. Carnap [15] draws a sharp distinction between questions which arise

<u>within</u> a given system of concepts, or framework of ideas, and questions which are sometimes raised <u>about</u> that framework or system. Questions of the first sort belong to the field of some science or of everyday life, and are answered by the methods appropriate to those fields. Questions of the latter sort have traditionally appeared in metaphysics in the misleading form of questions about the reality or existence of some general class of entities corresponding to the fundamental ideas of the system of concepts in question. Thus philosophers have asked whether there really existed such things as numbers, whether space-time points were real, etc. But such questions can be significantly understood only as raising the <u>practical</u> issue of whether or not to employ and use the given conceptual scheme or framework of ideas. To answer affirmatively is, according to Carnap, simply to adopt such a framework for use.

This question--the justification of the existence of external objects--is by no means settled. But it is certainly true that not only is the existence of external objects (bodies) accepted by most--including philosophers, scientists, and men-in-the-street. It is also true that throughout most of the history of philosophy this belief has been almost exclusively dominant. This was certainly so for the Greeks.

As a matter of fact, the early Greek philosophers devoted their attention almost exclusively to cosmology, the inquiry into what makes the physical world tick and why it is the way it is. Leaving aside the question of the relation between the objects of our awareness and the "ultimate constituents," we know that the early Greeks surely believed in the existence of objects external to themselves. Not only did the Greeks accept the existence of external objects in their philosophy but gradually they also developed a distinction which was to prove of importance of the first rank: the distinction between an object and its qualities (properties).

It is clear from the writings of the Presocratic philosophers that they did _not_ have the concept of a quality. That is, they just did not think in terms of qualities at all. They certainly had no single _word_ for quality; but also their general discussion shows that they did not possess the concept either. Where _we_ should talk of qualities they tended to talk of things. The traditional opposites, for example--qualities like heat and cold--were always thought of as things.

In the development of Plato's philosophy some sensitivity to the necessity of distinguishing between qualities and things _begins_ to emerge. In the earlier formulation of the Theory of Forms, the Forms correspond to qualities and yet are thought of as, in some sense, things. In the

Theatetus (182a), however, Socrates is made to introduce, with apologies, the word for quality (ποιο'τηζ)--a word literally meaning "what-sort-of-ness," which was constructed out of the word meaning "what sort of" (πôιοζ) which was already in use. Nevertheless, the dialogues show that Plato never became completely acclimated to the necessity of making a rigorous distinction in his thought between things and qualities. He could not fully appreciate what it was to think of something as a quality, as distinct from a thing.

Aristotle laid the philosophical foundations of the distinction in his doctrine of categories. Philosophical categories are classes, genera, or types supposed to mark necessary divisions within our conceptual scheme, divisions that we must recognize if we are to make literal sense in our discourse about the world. To say that two entities belong to different categories is to say that they have literally nothing in common, that we cannot apply the same descriptive terms to both unless we speak metaphorically or equivocally. Aristotle first used the word "category" in a short treatise called Categories. He held that every uncombined expression signifies (denotes, refers to) one or more things falling in at least one of the following ten classes: substance, quantity, quality, place, time, posture, state, action, and passion. By "uncombined expression" Aristotle meant an expression considered apart from its combination with other expressions in a sentence.

(We might note that Aristotle intended this account to apply only to those expressions which we now call "descriptive" and "non-logical.") Two considerations are essential to Aristotle's theory of categories: that substances be properly distinguished from accidents and essential predication from accidental predication. The first is the one of most interest to us, and the accident of most interest to us is that of quality.

According, now, to Aristotle's account, the earlier Greek philosophers failed to make a distinction between the category substance (i.e., the object) and the category quality. Actually, it turns out that in some of his philosophical discussion, Aristotle shows that he was not completely at home with the distinction either. A distinction of this significance is not easily come by, and the struggle to understand it apparently is long and hard.

But sooner or later, the distinction between an object and its qualities (of which, in general, there are several) does become entrenched. At this point immediately (logically, if not historically) another problem raises its head: <u>do all of the qualities (properties) have equal status</u>? And here "status" can have any of several different meanings: do all the qualities have the same status logically, physically, psychologically, causally, epistemologically, ontologically, etc.? For instance, are any of the qualities of an object "more real"

than any others? There is a second question that appears at this point which is also quite important: is this question (viz, about the status of the various qualities) a question for philosophy or a question for physics? We will see that this question arises more than once during our inquiry. We will postpone consideration of it until later.

Let us consider the first question, i.e., whether all the qualities of an object have equal status (in any of the several senses of status). One answer that has been given (and a historically prominent one) is NO. The qualities are not all on the same footing. There are primary qualities and there are secondary qualities. So, in addition to the distinction between an object and its qualities, we have the further distinction between those qualities that are primary and those that are merely secondary. This distinction dates back in principle to Democritus, who said that sweet and bitter, warm and cold, and color, exist only by convention (νόμῳ), and in truth there exists only the atoms and the void (Fragmente der Vorsokratiker, H. Diels and W. Kranz). The distinction was revised by Galileo and accepted by Descartes, Newton, and others (among them Robert Boyle and significantly, John Locke).

With regard to the motivation for re-introducing this distinction (around the seventeenth century), I believe that the motivation for this was more methodological than metaphysical. That is, the distinction was propounded not so much because of what was realized or understood regarding the "intrinsic nature" of objects. Rather, it was propounded because of the requirements of the new-found methodological tool of the scientists of the day, viz, mathematics. It was the case that, given the science of the day, <u>some</u> qualities of objects were susceptible to mathematical handling and others were not. It was the case furthermore, that, given the science of that time, all observed phenomena could be satisfactorily explained by taking into account <u>just</u> the first set of qualities (viz, the ones susceptible to mathematical manipulation). Thus, it was believed that these qualities were of <u>primary</u> importance for our understanding of the phenomena and consequently for the intrinsic nature of the phenomena (and objects) themselves.

We will discuss the particular solution of the seventeenth century thinkers (of the problem of the status of the qualities of objects) a bit more in detail later. Here I wish only to point out that though the motivation for re-introducing the distinction can be called into question, and though the motivating reasons are clearly not all <u>philosophical</u>, this does not, <u>in itself</u>, preclude the possibility of just such a philosophical distinction. In philosophy, the connection between argument and truth (in an elementary and intuitive sense) is a tenuous one.

A position might be a true one, even though the argument in support of it is itself a bad argument. Thus, even though the methodological (i.e., mathematical) motivating reasons might be called into question, the position which these reasons are designed to support (viz, the primary/ secondary distinction) should be judged on its own merits. In other words, though this particular line of argument might not be convincing or adequate, there may indeed be another line of argument which is.

And *prima facie* there does seem to be ample evidence for believing in a distinction between the several properties of an object. We are all familiar with the case of a piece of clothing, say a necktie, that appears to be one color when looked at in the showcase, but appears to be of a noticeably different shade (of the same color) when looked at in the natural light outside the shop. It is also a common experience to find the same object appear to be of different colors when viewed under different kinds of light. Hence, the color of the object seems clearly to be a property that is *relative* to the observer or the conditions of observation and is not *inherent* in the object itself. On the other hand, regardless of the kind of light present, or any other of the usual kinds of changes in the conditions of observation, the mass (or weight) of the object remains the same. Thus, there does seem to be grounds for a distinction between some of the properties.

It is important to note that we can always recognize both a physical and a psychological correlate for each of our sensations. Thus, with respect to a coin there is the experienced (seen) shape and size, and there is the physical shape and size that can be measured. There is the physical shape of the coin (round), which can be measured with a compass, and the physical size, which can be measured with a ruler; and there is the sensible or experienced shape (round if seen from a perpendicular, elliptical if seen from other angles) and the sensible or experienced size (smaller if one recedes from the object).

In all this there is no distinction between primary and secondary qualities: both sets of qualities, we might say, have both their primary (physical) and secondary (experienced) aspects. This distinction between the physical and the experienced aspects was not always realized by philosophers. (This is especially true of some of the arguments advanced by Locke.) But when this distinction is kept in mind, many of the traditional arguments fall apart. Instead of a distinction between primary qualities (for example, mass) and secondary qualities (for example, color), we end up with a distinction between <u>physical</u> qualities and <u>sensible</u> qualities (which extends throughout most of the range of <u>both</u> the Lockean primary and secondary qualities).

To say this much is indeed helpful and is certainly progress. But the problem is not thereby removed. It is natural at this point to regard (for example) the physical shape as a quality of the object and the experienced shape as characteristic of our sense experiences. Now it is also true that there will always be some variation in the properties of an object--as perceived by an observer--because of the varying conditions under which the perception is made. The following question now arises (and this is the real question): can the Lockean distinction still be maintained in essence, even though the terminology has changed? That is, can we maintain a picture in which there is a sharp dichotomy between those properties of an object which are susceptible to variation due to varying conditions of observation (sensible qualities), and those properties which (on the other hand) are not susceptible (physical qualities)? Can we retain the distinction between those properties which are relative (i.e., relative to an observer) and those properties which are intrinsic to the object and completely independent from any other object, most especially, an observer?

Hence we see that the basic issue in our inquiry poses itself in several different ways. There are at least three different ways of formulating the problem that will be helpful for what follows:

a. Are there some properties of an object that never change?

b. Are there some properties of an object in terms of which all the remaining properties can be specified? That is, are there a certain set of predicates such that taking these predicates as primitive, all other predicates can be defined?

c. Is it possible to give a completely objective account of all the objects (and phenomena) of which we are aware; or must any such account inevitably include an irreducible element of subjectivism?

In formulation (c) I wish "subjectivism" to be understood in the following specific sense (which is perhaps slightly different from the meaning it frequently has): an explanatory account of the nature of objects would be considered objective if we could account for the existence and the characteristics (of these objects) solely by appeal to the objects under question, without in any way needing to consider any object distinct from the given ones; that is, if we could show that they were completely independent--in all their essential features--of all other objects. However, if this is not possible, if we find that some characteristics are subject to conditions determined by other objects, then our account, to that extent, is <u>subjective</u>. (Two comments are in order. First, when I speak of accounting for the existence of objects, I do not mean this in the sense of saying "where they came from." I mean this only in the sense of explaining why the objects exist in a certain state, or in a certain manner. Secondly, I am of course aware that one object can interact with another. In this case there is no question of providing an explanatory account which does not take into consideration "other objects." I am asking, rather, whether in those cases when there is no such interaction, i.e., when the object is <u>isolated</u>, is such an independent account possible. In short, I am asking whether we can maintain this concept of an <u>isolated</u> body.)

In the next part we will examine whatever physics can tell us regarding the basic question of our inquiry, as formulated in the three questions above.

PART II

THE EVIDENCE OF PHYSICS

According to the third formulation above of our central issue, our question has come down to this: can we give an account of objects and phenomena (the constituents of the physical universe) which is free of any element of dependence upon other objects or phenomena? Can we give an account of "external" objects in which they are not only "external" but also "independent"--independent of both: (1) other objects, and (2) us as observers? (Since we as observers are also objects, the desired distinction is clearly the element of consciousness which enters with observers. Historically, only the second of these points has been of great concern. But we shall see that the first is of <u>at least</u> equal importance. This point will receive detailed attention below.)

To seek answers to our questions we shall now appeal to the different accounts of nature provided us by physics. But first, it would be helpful to examine in a bit more detail what we have in mind when we speak of subjectivism.

(As noted above, there are two ways in which the independence of physical objects can be eliminated: (1) by the need to take into account other objects; and (2) by the need to take into account other objects with consciousness, namely, human observers. The second has received the most attention historically. So let us, for the time being, treat the problem just with regard to the second issue. We will, soon enough, find sufficient reason to consider also the first.)

Classical Physics

In what follows we will take subjectivism to mean the claim that the properties of external objects are not completely independent of any observer or any knower. This is the claim that in any discussion of external objects or their properties it is essential to include the observer; for the presence of and action of the observer is a crucial determinate of the properties of external objects. (We take the existence of these objects as given.) A moderate subjectivism would make the following claim: the action of the observer--or the interaction between observer and object--in some way or other has an effect upon the properties of the object. A more extreme subjectivism might claim that the object has no properties whatever independently of the interaction between observer and object.

Those thinkers that would be labelled subjectivist according to the above criteria claim subjectivism only with respect to the <u>properties</u> of objects, not to the existence of these objects. There have been those who wished to maintain this sort of subjectivism also with regard to the existence of the objects. There are difficulties facing both groups, but the subjectivist (according to our criteria) might have, at the beginning, to face some additional ones: (1) if <u>all</u> the properties of the object are included under the "subjective" classification, what sense does it make to speak of an independently existing object with no properties? (2) But if, on the other hand, only <u>some</u> of the properties are subjective, then how do we decide which are subjective and which are not?

The subjectivism we are considering here is not the trivial claim that properties are subjective because we always require an observer to observe them. This is always the case and is granted. The subjectivism meant here asks the question: "is observation simply the ascertaining of properties of objects that the latter possess whether they are observed or not, or is it an interaction that yields information only about the result of the interaction?" And subjectivism decides in favor of the latter. It denies that the properties exist independently in the object whether they are observed or not; and it asserts that it is only in the act of observation that the properties exist, or come into existence; are created.

The position adopted by the seventeenth century thinkers (and subsequently, the position underlying all of classical physics) was to specify certain properties of an object such as mass, motion, shape (the exact nature of the list varied from person to person) as objective and primary, and to specify all others as subjective and secondary (e.g., taste, smell, color, etc.). These latter properties were only secondarily properties of the objects. They were primarily <u>relational</u> properties between the object and the observer. If we take away the relation, we take away the property. Thus Galileo asks if there were no human beings around could the feather still cause a tickling sensation? But the most important element of this solution (of classical physics) was the claim that all the subjective, secondary properties could be "explained" in terms of the objective, primary properties. Hence, even though sweetness is a secondary property, the sweetness of the water can be explained in terms of the relative number, the configuration, and the relative motion of the water molecules and the sugar molecules. So even though the sweetness could only be <u>tasted</u> by an observer, the situation which accounted for (which caused) the sweetness--viz, the

molecular configuration and motion--could be explained and accounted for by appealing only to objective, primary properties (i.e., mass and motion). Hence even though there are properties that could only exist in an experiencer, i.e., an observer, the scientific account of the external objects, the properties of those objects, and the interaction between the objects and the observer were all completely given in terms of the objective, primary properties of those external objects and their elementary constituents. In short, classical physics said that: (1) there were two kinds of properties, objective properties and relational properties; and (2) the relational properties could be explained in terms of the objective properties. So the account of the world provided by classical physics was at root objective, with no subjective element entering at any essential point.

We note that with regard to all three formulations of our basic question classical physics must be regarded as in no way subjective, but completely objective. For: (a) there are certain properties of an object that do not change (viz, the primary properties, e.g., mass); (b) there are certain basic properties in terms of which all others can be specified (e.g., mass and motion); and (c) classical physics provides an account which is completely objective and independent.

This account of the world provided a picture in which certain "primary" properties were possessed "by the object" or existed "in the object"; in the fullest sense, these were properties "of the object." They were determined completely and solely by the nature of the object itself, and were in no way connected with anything distinct from the object itself. Thus we have a picture in which objects have a completely objective and independent existence. They are capable of existing independently (much like the old concept of substance) and evolving independently in time. When there is an interaction between a given object and some other object, we can, of course, no longer consider the object as evolving independently. But even here, we can account for the new time evolution of the object--and this, by appeal solely to those basic properties of the object which are themselves immutable and "objective." Hence in a very strong sense, this account of classical physics is an <u>objective</u> one.

Relativistic Physics

From this point on (i.e., from the culmination of classical physics), the total and absolute objectivity of classical science becomes successively eroded away. Each of the two major advances in physical theory since the appearance of classical Newtonian mechanics (viz, relativity theory and QT) has been accompanied by a loosening of this "objective" picture painted by science. With both of these developments we find an increase in the number of properties that are construed to be relational, and a corresponding decrease in the number of properties which retain their objective, independent status. Let us consider first relativity theory.

Since the time of Galileo the concept of relativity, in its broadest sense, has always been present in physics. Loosely speaking, this concept can be expressed simply as a question: do the laws of nature, and hence the physical situations described by them, always appear to be the same to different observers, even though the state of motion of the observers is not identical?

In classical physics, the state of any mechanical system at some time t_0 can be specified by constructing a set of coordinate axes and giving the coordinates and momenta of the various parts of the system at that time. If we know the forces acting between the various parts, Newton's laws make it possible to calculate the state of the system at any future time t in terms of its state at t_0. It is often desirable that during or after such a calculation we specify the state of the system in terms of a new set of coordinate axes, which is moving relative to the first set. A two-fold question then arises: how do we transform our description of the system from the old to the new coordinates, and what happens to the

equations which govern the behavior of the system when we make the transformation? The set of equations that describes the relations between the two sets of coordinates are known by the name of the Galilean transformation

$$x = x' + ut'$$

$$y = y'$$

$$z = z'$$

$$t = t'$$

In these equations it has been assumed that the coordinate axes in S and S' (the two coordinate systems) are parallel, that their origins coincide, and that the motion takes place in the x-direction; u is the velocity of the primed coordinate system S', relative to the unprimed one S.

Thus the principle of relativity takes the following form in classical mechanics: if S is an inertial system of reference, and S' a system of reference that moves with a constant velocity relative to S, then the laws of mechanics must have the same form in S and S', provided x, y, z, t, and x', y', z', t', are connected by a Galilean transformation. In short, we say that the laws of Newtonian mechanics are invariant under Galilean transformations. Of course, the next question is whether the rest of the physical laws are also invariant under this transformation (in addition to those of classical mechanics).

If so, this would mean that even experiments that do not rely exclusively on the laws of mechanics should also fail in telling the observer whether he is at rest or in uniform motion, using the connections given by the equations above between the primed and unprimed coordinates. It is precisely this problem--which puzzled scientists for nearly twenty years--that is solved by the next development of the concept of relativity: Einstein's special theory of relativity.

Einstein discerned the three assumptions underlying the Galilean principle of relativity and the different roles the constancy of light velocity plays in each. The three assumptions are the following: (1) there are inertial systems in which the laws of physics are identical; (2) if one system is inertial, any system of reference that moves with a constant velocity relative to the first one is also inertial; (3) the transcription of space and time data from one inertial system to another has to be done according to the Galilean transformation as given above. The first two assumptions assert that the relative configurations of bodies determine the physical occurrences (not the configurations and motions relative to some external frame of reference). This emphasis on the relative configurations of bodies will turn out to be of importance later. The third assumption indicates how observers must translate the results they have obtained in one inertial frame to get the results observed in any other inertial frame. Experiments indicate that the first two assumptions should be valid regardless of what assumptions are made about the velocity of light. But with respect to the third, it is known that the Galilean transformations tacitly assume that the time of occurrence of an event is the same in all inertial frames. The question arises whether or not this assumption is reasonable if the velocity of propagation of signals (light signals) is not infinite. Einstein showed that this is not a valid assumption. Rejection of this assumption left Einstein in this position: we can say that the constancy of the velocity of light in different frames (which has iron-clad experimental support), and the existence of equivalent frames, are logically compatible if we modify the transcription rules of space and time data from one inertial system to another. This means we must find another set of transformations to replace the Galilean set--a set such that the velocity of light should be the same for both frames of reference. These are called the Lorentz transformations

$$x = (x' + ut')\gamma \qquad \gamma^{-1} = \sqrt{1 - \beta^2}$$

$$y = y'$$

$$z = z'$$

$$t = \left(t' + \frac{x'u}{c^2}\right)\gamma \qquad \beta = \frac{u}{c}$$

Any physical description which satisfies these conditions is called Lorentz invariant.

Naturally, we will expect that some interesting consequences arise from the fact that we no longer have an absolute time, the same in all frames of reference. It turns out that the spatial distance between points will change as we change systems of reference. The duration of a process will change as we change reference frames (with a corresponding change in frequency and wavelength). Also, the mass will change as we change reference frames.

It will be recalled that earlier in our discussion we noted that even though the distinction, as originally formulated by Locke, between primary and secondary qualities could not be maintained, we saw that the essence of the distinction could still be preserved by distinguishing the <u>experienced quality</u> from the <u>physical quality</u> (i.e., the measured quality). It was by appeal to this <u>physical</u> property--i.e., a measured quantity--that one could still plead the case for objective, independent properties which were characteristic of the object and in no sense relational. Thus, even though a rod might appear smaller as we move away from it (experienced size), the length of the rod as measured by a ruler (physical size) would have the same value regardless of our position. Clearly, length was a property of the object. So also with mass. And the time of duration was a property of the process or phenomenon. These properties were objective and independent.

But this is precisely the verdict that is reversed by the theory of relativity. It has been seen that the mass of an object will change as we change our frame of reference (i.e., change our state of motion). Hence we can no longer think of the mass simply as characteristic of an object. Rather it is now a <u>relational</u> property between the object and the observer (as represented by a given frame of reference). Hence, the mass of an object has ceased being an objective, independent characteristic and now must share the ignominy of all the other properties which it had helped to relegate to the depths of non-independent, subjective existence. And also with spatial distance and time duration. Whereas, before, we clearly had three instances of objective properties, after relativity theory these properties are now also seen to be subjective.

It is undeniable, now, that relativity has put a crack in the edifice of objectivity erected by classical science. But it is valuable to ask the following question: did relativity completely demolish the structure or did it leave some elements of objectivity intact?

Just from what we have seen already it is understandable how the prevailing metaphysical interpretation of physics had become so intrenched as to be almost instinctive. This interpretation asserted that physical experience can be regarded as an effect on our senses of an objectively existing external world having certain definite and ascertainable properties. Modern science originated, however, in the determination to ignore the metaphysical question and to accept experience as an object of study, whether or not it was "real." But it comes as no surprise that, in fact, through force of habit, the metaphysical question <u>was</u> implicitly answered, and the phenomena studied by physics accepted as <u>real</u> with not as much as a batting of the eye.

It was the great lesson of relativity to teach us that this kind of metaphysical picture was no longer feasible. It had to be modified. And a better picture was devised. In the old picture the world consisted of pieces of matter, measured essentially by their mass, moving about, without thereby becoming changed, in an infinite extension called space and an independent extension called time. According to the revised picture, space and time are no longer independent but merge into a single continuum, space-time. This merger was made possible by the fact that, according to the definitions adopted for the time relations of separated events, a certain combination of space and time separations of events (the space-time "interval") is independent of the state of motion which we choose to assign to any one of the bodies concerned. Thus we can suppose that this combination of our measurements measures some <u>absolute</u> property of events, just as we previously thought that the measurement of length indicated some absolute property of a body.

It is still true, however, that the masses of bodies change with motion. But going farther in the same direction, the general theory of relativity succeeds in prescribing a combination of space, time, and mass measurements that is independent of motion altogether, and so--for the time being at any rate--can pose as an aspect of an external world of which physical measurements provide a quantitative description.

Thus with regard to our three formulations we find that the theory of relativity has wrought some substantial changes. With regard to (b) we can still say that there are certain basic properties in terms of which all others can be specified; and to this extent we still have objectivity. But with regard to (a) we find that many of the important properties that were considered objective in classical physics are now subjective in relativity theory, e.g., mass, length. There are other properties that are just as objective in relativity theory as they were in classical physics (e.g., electric charge which is Lorentz invariant). So according to formulation (a) we find that some subjectivism has sneaked in. And we find the same with regard to formulation (c), for now some of the properties must be specified relative to other things (inertial reference frames) distinct from the object, and are, according to this formulation, now subjective. But again as in

(a) there are still some properties that remain objective even according to formulation (c).

Hence, though we see that in both the special and the general theory we begin a process of relativization of the properties of objects, it is by no means the case that these theories leave our physical experience completely devoid of any objective foothold at all. What we have is a partial relaxation of the claims of objectivity in the external world. We will see now that though quantum theory goes even further with this process of relativization, it too results in only a partial and not a complete rejection of the idea of independently existing objects. But the subjectivism that is introduced by QT goes much deeper than anything we have seen till now, and cuts across all three of our earlier formulations.

Quantum Physics

Basic Ideas of Quantum Theory.--Now is there anything in QT that resembles the subjectivism we have been discussing? Let us consider the principle of superposition in QT. This principle is so central to QT that Dirac [19], in his definitive treatise, could derive most of QT from just this principle. The principle states that if we have a solution ψ_1 to the Schroedinger equation, and another solution ψ_2, then there exists still another solution to the equation given by

$$\psi = a\psi_1 + b\psi_2 \quad \text{with} \quad |a|^2 + |b|^2 = 1.$$

Or in other words, if, for a given physical system, we have a definite, physically observable state represented by ψ_1 and another state represented by ψ_2, then there is another definite state, physically observable and distinct from either ψ_1 or ψ_2, represented by

$$\psi = a\psi_1 + b\psi_2.$$

What about the properties of the physical system in these various states? With respect to a given property (observable) QT says that the system can assume only certain specified values for the values of that observable. Not all values are permissible. The values of the observable are quantized. (We assume discrete, non-degenerate spectra.) To each of the possible values of the observable there is a corresponding state of the system, represented by a state function ψ_i. To each of these states ψ_i there is associated a number a_i called the eigenvalue for that state. Thus whenever the system is in the state ψ_i, the value of the observable will be given by a_i. Also, upon repeated measurements of the observable, either on the same system or identically prepared systems, the value a_i is repeatedly obtained.

Suppose now that our system has a state ψ_1 with eigenvalue a_1, and also a state ψ_2 with eigenvalue a_2.

Then according to the superposition principle there is also a state $\psi = a\psi_1 + b\psi_2$. It is found that upon measurement we obtain (for the system in the state ψ) the value a_1 sometimes, and obtain the value a_2 sometimes. (The relative proportions are determined by the numbers a and b.) The order of occurrence of a_1 and a_2 is completely random; and in any given observation we are totally incapable of saying which of the two values we will obtain. Also, no other values besides a_1 and a_2 occur.

So the situation in QT is the following (with respect to a given property or observable): there are some states, pure states, to which the theory attributes definite and well-defined values of the observable in question. And upon measurement, in these states, those values invariably obtain. But according to the superposition principle, there are also superposition states such that: (1) the theory attributes no definite value of the observable; and (2) measurement of the observable yields different values on different trials. Thus, before the measurement, our description of the physical system (the object)--i.e., what the theory tells us about the system--contains (and is capable of containing) no reference whatsoever to the value of the observable. All the theory can tell us is that there are a certain group of values, one of which will emerge upon measurement. It cannot tell us which one will emerge. And most importantly, the theory cannot associate any particular one of the values with the state before a measurement is performed. Hence, as far as the theory is concerned, before the measurement the system is in a definite state with respect to the observable in question; but has no definite value for the given observable. But a definite value does always emerge upon measurement. So if we ask the question: "before measurement, before any interaction with an observer, what is the value of a given observable?," the theory can give no value. The theory can give the probability of a given value emerging upon measurement. But as to what the value is right now, before a measurement, the theory is powerless to say. After the measurement, we can point to one definite value as being the value of the system.

To conclude this introduction I wish to do two things: (1) show how this situation in QT qualifies to be called subjectivism; and (2) show why this subjectivism is different from and more far-reaching than anything allowed in classical physics.

Situation in QT Does Qualify as Being "Subjective".--From our preceding discussion of subjectivism an adequate statement of the subjectivist point of view might be the following: the properties of external objects have no status independent of an observer. Further, the properties of an object either only become definite (moderate subjectivism) or only come into existence at all (extreme

subjectivism) in the interaction between the observer and the object. Is this the way QT works? I wish to answer yes! In QT the interaction we are concerned with is the interaction designed to give us knowledge or information about a system, i.e., a measurement. We have seen that there are certain states (viz, the superposed states) such that certain properties indeed have no status independent of the observer (who forces the interaction). For with a superposition the very possibility of assigning a value to the observable <u>requires the interposition of an interaction</u>.

But what about the system before a measurement? Is this not a <u>definite</u>, perfectly well-defined state already --even by our own superposition principle? And does not a definite, perfectly well-defined state have definite, perfectly well-defined properties? Yes and no! Suppose the property we are considering is spin. Then our answer is yes: a superposition is a definite, perfectly well-defined state. It is crucial to realize this. And this state will exhibit physically properties that none of the "single" eigenstates (which constitute the superposition) will exhibit alone. But we must also answer <u>no</u>: it does <u>not</u> have definite, perfectly well-defined properties in this state. So even though this <u>is</u> a <u>definite</u> (and distinct) spin state, the state does not have a definite value of the spin associated with it. This is simply a

peculiarity of QT: <u>there are some states where a given property has no value at all</u>.

But at this point one might be inclined to ask: but if this is a definite spin state, as you say, what are we to say about the spin of this definite spin state before we make a measurement? Two obvious answers come to mind: (1) The state has no spin at all before the measurement. Clearly, this leaves you open to the charge of subjectivism of the most extreme kind. (2) The state is "somehow" simultaneously in all the spin states, but then becomes completely in one of the spin states upon measurement. And even this move still is subject to the charge of moderate subjectivism. I know of no interpretation with respect to the superposition principle that can completely avoid the charge of subjectivism. So in answer to the question "what are we to say about the spin before measurement?," one might well be inclined to reply "nothing." But if we are incapable of saying anything before a measurement, and yet are capable of saying something <u>after</u> a measurement; and if furthermore, this incapacity is theory-imposed, then I see no way to avoid the claim that the theory contains subjective elements--subjective because we are prevented, <u>in theory</u>, from ascribing any value to the property unless and until there is an interaction between the object described and the observer who is describing.

Difference Between Classical and Quantum Physics.--But one might ask "is the situation in QT really any different from the situation in classical physics?" Does not classical physics also require a measurement before we can ascribe a given property to a given system? The answer is emphatically no! In classical phase space the orbit or path, indicating the evolution or development of a system, is a continuous line; and through a given point only one line passes. The continuity of the path assures the continuity of the evolution of the system, preventing any discontinuous jumps or changes. A given point is fixed by a complete specification of a pair of generalized canonically conjugate variables at that point. And these in turn completely specify the state of a system described by those variables. Thus since only one line passes through any given point, this means that whenever a system is in a state described by the specified values of the variables at that point in phase space, the time development of that system is completely determined (i.e., there is only one path to follow). Thus if we know the state at any given time, we can always specify what its state will be at any given time in the future, or in the past. (If a system passes through a given point in phase space, then it has a uniquely determined future and a uniquely determined past.) Thus for any system in any state, if we know a previously completely defined state, we can always give another completely defined state at any desired time. This is also possible in QT. But in classical physics a completely defined state means including the values for all observables. Thus in classical physics, according to the theories, a system always has a definite value for each observable--<u>and this independently of whether a measurement is undertaken to reveal that value or not</u>. The value can be given by the theory. And since in classical physics the disturbing effect of a measurement can be omitted in theoretical discussions, not only does the property of the system exist independently of the observer, but is also stable and fixed in the face of an interaction with the observer. Clearly, this is radically different from the situation in QT.

Finally, I would like to make one last distinction to emphasize the difference between classical physics and quantum physics. In scholastic philosophy a distinction was made between what were called determinables and determinates. The modern revival of these terms was due to the Cambridge philosopher and logician, W. E. Johnson. In his <u>Logic</u> (1921) he states: "I propose to call such terms as colour and shape <u>determinables</u> in relation to such terms as red or circular which will be called <u>determinates</u>" [32]. It is easily seen that in classical physics the properties ascribable to physical systems are both determinable and determinate. Whereas in quantum physics

the properties are <u>at most</u> only determinable. For, using our example above, we cannot say that the system has a particular value for the spin; we can only say that it is in a spin state. But there is another difficulty. Suppose we consider the relation between determinable and determinates to be one of class to members. That is, suppose the determinable is a class whose members are the determinates (e.g., the determinable shape is a class which has as members triangular, rectangular, elliptical, etc.). Thus to say of a property that it is determinable is to say that though we cannot specify which particular determinate is applicable, we do know that one of them is applicable. But this is <u>not possible</u> in QT. Eugene Wigner [57] has shown that it is inconsistent with the laws of QT to assume that for a superposed state, we can, without contradiction, treat the state as though it were "really" in one of its constituent states. That is, for a superposed state, we cannot ascribe any one of the particular states to the system--even though the particular states we have in mind exhaust <u>all</u> the possibilities. But then what sense is there in the claim that the property is determinable? Admittedly, though it is not possible to say of a superposed state that it has a particular spin, it <u>is</u> possible to say that the state <u>has spin properties</u>; it <u>is</u> in a state of spin. This is undeniable. But I do think that we must admit the strangeness of the situation which allows us to say of a system that it has a certain property, and at the same time forbids us saying that it has a particular value for that property--even though unknown.

This is the situation of QT: I believe I have shown that here is a situation: (1) that is new--classical physics contained nothing of the sort; and (2) that is contrary to our everyday common sense notions regarding external objects. (And of course, this is the view cultivated by classical physics also.) There is a substantial and irreducible element of subjectivism in our description of external physical objects--as described by present QT.

Probability as Entrance of Subjectivism Into QT: Arguments for Non-Classical Probability.--It is generally thought that subjectivism enters QT at two points: probability and experiment. I wish to argue that subjectivism does enter QT through both experiment and probability. I will also examine the exact nature of the probabilities used in QT, in order to determine if these are ordinary classical probabilities or whether they are, in some sense, unusual non-classical kinds of probability.

It is, of course, well known that probability was not first used with QT. Probability was used and already fully developed in classical physics. But in pre-quantum physics we could always make a clear distinction between those areas which did and those which did not involve

probabilities. Classical mechanics, electromagnetism, and thermodynamics would belong to the second group; kinetic theory, statistical mechanics would belong to the first group. The last statement should be regarded as an _in principle_ statement. For it must be admitted that because we have experimental error, because we never have absolutely precise data, and because we sometimes do not have _complete_ information, probability makes an intrusion into even classical mechanics. But it is precisely the introduction of probabilities which enlarges the range of application of, say, classical mechanics in important ways, adapting the laws of that subject to the limitations of human powers of observation and of reasoning. But it is important to remember that the probabilities of classical physics are _always_ manipulable, or reducible (using the terminology of Henry Margenau) [40]. That is, they can always be reduced to certainties on the addition of more evidence or information. Thus, _if_ we knew _all_ the initial conditions for a given throw of a die, we could (assuming all the calculations could be done) reduce the probability of a four appearing from one-sixth (for a fair die) to either one or zero. In other words, the inability to specify the outcome of a given experiment was due _not_ to any characteristics or properties of the experimental system itself. Rather, this inability was seen to derive from the human limitations in completely specifying the

experimental situation to which our laws are to be applied. And the last element of the story is simply that this inability was in principle removable; it was only a practical blemish on the face of classical physics; it was a matter of "refinement." And finally, we could always approach the completely specified state as closely as we wished. Thus the lack of specificity which comes with probability could be dismissed as only a practical matter. It was characteristic of our knowledge, not of the "objects" known; and it could, in principle, be eliminated. Hence, we could still maintain a picture of objects or events as well-defined existents with well-defined properties. It was just our knowledge of them that was fuzzy, and we could make it as clear as we wished.

However, QT always involves probabilities--even in principle! (See Quantum Mechanics, Albert Messiah [44].) The probabilities arise in QT not merely when we attempt to get information about a system and we run into the human limitations mentioned above. Those probabilities are certainly present; but there are others also. And these probabilities are involved in the very definition of the properties to be observed. Furthermore, these probabilities are not reducible by the appearance of more information. Even with the maximum information allowed these probabilities remain.

But now (as is easily seen), the lack of specificity which comes with probabilities cannot be dismissed as a mere aggravation that can in principle be eliminated. Nor can we any longer attribute this lack of specificity solely to our knowledge of physical phenomena and restrict it there (so that our picture of the phenomena themselves in no way becomes tainted with this fuzziness). Since our very description of physical phenomena includes--irreducibly --this lack of specificity, it would certainly appear--at least on first glance--that this lack of specificity must extend over to the phenomena themselves. If this is the case, this conclusion has very important consequences of a metaphysical or ontological nature: metaphysical because there would be placed very strong limits on the nature of the phenomena (and objects) themselves. Hence it is important to understand both the presence of probabilities in QT and the nature of those probabilities.

It has been argued (by Feynman [22], Suppes [54], for instance) that even though the probabilities in QT are irreducible and are "here to stay," these probabilities are somehow different from the probabilities used in classical physics. I believe that even granting this, the force of the above epistemological observations is not lessened. But it is of value to examine this claim, if for no other reason than to help us delineate those aspects in which QT is both a physical and a logical development from classical mechanics (rather than being everywhere divided by a sharp, radical, and irreconcilable schism).

A. I would like to present first the argument of an emminent physicist, Richard P. Feynman [22]. At the Second Berkeley Symposium on Mathematical Statistics and Probability Feynman delivered a paper in which he stated: "The new theory asserts that there are experiments for which the exact outcome is fundamentally unpredictable, and that in these cases one has to be satisfied with computing probabilities of various outcomes. But far more fundamental was the discovery that in nature the laws of combining probabilities were not those of the classical probability theory of Laplace." In that paper Feynaman never explicitly states in exactly what way the "combining" of probabilities changed from the "classical" means. But his intention seems clear and I shall attempt to reconstruct what I take to be his argument.

Consider a double slit experiment performed with electrons. If the electrons are allowed to pass through slit A (with slit B closed), we will obtain a probability distribution P_1 for the arrival of the electrons on the screen. If the electrons are allowed to pass through slit B (with slit A closed), we will obtain a probability distribution P_2 for the arrival of the electrons at the screen. If the electrons are allowed to pass through with both slits A and B open, we will obtain a probability distribution P_{12} for the arrival of the electrons at the screen. Now it is an experimental fact that $P_{12} \neq P_1 + P_2$. This is as far as Feynman goes explicitly, so I must fill in what I take to be the rest of his argument.

We can write down the axioms of classical probability theory quite easily. Following Kolmogorov [34] we have:

> If X is a non-empty set,
> and f is a σ-algebra of subsets of X,
> then P is a probability measure on f such that
> (1) $P(X) = 1$
> (2) $0 \leq P(A) \leq 1$ for any A in f
> (3) $P(A \cup B) = P(A) + P(B)$, if A and B are mutually disjoint sets. $(A \cup B)$ is just the union of the two sets A and B.

Now when Feynman refers to the "combining" of probabilities, I presume that he has in mind combining probabilities according to axiom 3. Thus if we have two "events" A and B, which are mutually exclusive, then the probability of either event A or event B is given by $P(A \cup B) = P(A) + P(B)$. Now if $P(A \cup B) \neq P(A) + P(B)$ then we most certainly have a case of failure of the combinatorial laws of classical probability. But is that what Feynman had in mind? The probability statement written by Feynman was $P_{12} \neq P_1 + P_2$.

If we are to read Feynman as claiming that his equation implies a breakdown of classical probability we must interpret the equation in something like the following way: "The electron can arrive at the screen by passing through slit A, yielding probability P_1; <u>or</u> the electron can arrive at the screen by passing through slit B, yielding probability P_2. The electron certainly cannot pass through both slits A and B at one and the same time. (This also makes the two possibilities mutually exclusive.) Thus the probability of the electron passing through either slit A or through slit B should be the probability of $(A \cup B)$ which is just $P(A) + P(B)$."

Now it is well known that speaking of elementary particles like the electron as having a particulate nature is a "loose" way of speaking and can be very misleading in some experimental situations. Indeed, the kernel of Bohr's complementarity principle is that <u>if</u> we insist on speaking of the elementary constituents of atoms as "particles" and/or "waves," then we <u>cannot</u> use one picture to the exclusion of the other. We must use them both. And failure to do so quite simply lands us in conflict with empirical facts.

Now Feynman is as aware as anyone that the kind of talk used above about the electron is incautious talk. And consequently he does not use it. A phraseology closer to what Feynman would probably say (had he filled

in his argument) is the following: "The electron can arrive at the screen with only slit A open. The electron can arrive at the screen with only slit B open. The electron can arrive at the screen with both slits A and B open." But now there is a non-negligible difference between the two formulations. Whereas in the first formulation, the third possibility (viz, that the electron goes through both slits A and B at one and the same time) was declared clearly ridiculous, this is not the case now. For the third alternative under the second formulation is a viable one (viz, that the electron arrives at the screen with both slits A and B open). So now we have three alternatives, instead of just two. But the precise statement of axiom 3 is $P(\bigcup_i A_i) = \sum_i P(A_i)$. This means that the probability of any event which is the union of other mutually exclusive events is the sum of the probabilities of each of the constituent events taken separately. But the sum is taken over as many different alternatives as there exist. Thus in our more precise statement of the double slit experiment (and hence more accurate and more adequate) there are three distinct alternatives, and the formalism of classical probability theory should give us three terms in the summation, not two.

In short, the fallacy in the above argument for non-classical probability is the assumption that the situations represented by P_1 and P_2 are mutually exclusive

<u>and</u> exhaustive. They are <u>not</u> exhaustive. There <u>is</u> a third possibility and the presence of that third possibility is indicated by the "interference" term. The presence of the third term (the interference term) also shows that the formalism of classical probability <u>has</u> correctly accounted for <u>all</u> the alternatives and accordingly has a term in the summation for each alternative. [It is obviously true that the three alternatives are both: (1) exhaustive, and (2) mutually exclusive. (1) There are only four <u>logical</u> possibilities: A open, B open; A open, B closed; A closed, B open; A closed, B closed. We have utilized the first three cases. The last case is ruled out physically since then <u>no</u> electrons would arrive at the screen and we would have no experiment at all. (2) It is likewise clear that with A open and B closed, we cannot have <u>at the same time</u> either A open and B open or A closed and B open.]

We can see from the above just how dangerous "loose" and "incautious" ways of speaking can be. It deprived us of one of our alternatives. But we have seen that when we speak in more precise language, it is not the case that classical probability fails. For if adequate account is taken of <u>all</u> the possible alternatives the classical formalism will adequately describe the phenomena. So even if we attribute to Feynman the precise version of the experiment, I can still find no grounds for accepting his conclusion that QT requires a non-classical probability.

Let me mention briefly another argument for non-classical probability, which will be seen to be just a generalization of Feynman's argument, and hence guilty of the same error. It will be worthwhile to see the case stated in its full generality. Suppose A is an observable with eigenfunctions ϕ_1, ϕ_2, ϕ_3 ... . Let B be an observable that does not commute with A. (We will assume discrete, non-degenerate spectra.) Now the average value <B> of B in the state $\phi = \sum_i a_i \phi_i$ is given by $\langle B \rangle = \sum_i |a_i|^2 \langle B_i \rangle + I$ where $\langle B \rangle = (\phi_i, B\phi_i)$ is the average value of B in state ϕ_i and $I = \sum_{i \neq j} a_i a_j^* (\phi_i, B\phi_j)$. The interference terms are represented by I. Non-classical probability might be claimed here on the basis of the following erroneous argument. The events E_i, where 'E_i' = 'the system is in the state ϕ_i', are mutually exclusive and exhaustive, with probabilities $|a_i|^2$. If the classical formalism of probability applied we would have

$$\langle B \rangle = \Sigma \text{ (probability of } E_i\text{)(average value of B in } E_i\text{)}$$

$$\langle B \rangle = \Sigma |a_i|^2 \langle B_i \rangle$$

This differs, however, from the previous--<u>and quantum theoretically correct</u>--formula precisely by the interference terms I. Thus, the argument runs, the probability employed by QT must be non-classical. Here again the error involved is the assumption that the E_i are jointly exhaustive. The error is based on a failure to understand the implication of superposition. As was stated earlier, the superposed state is a physically distinct state. Thus not only may the system be in some state ϕ_i, but it may also be in any of the infinitely many superpositions of ϕ_i. And in the case at issue the system _is_ in just such a superposition, namely ϕ. (This example is due to Fine [23].)

B. The only other persuasive argument for non-classical probability in QT that I know of was given by Suppes [54]. This argument is also the one which strikes deepest into the heart of the theory of probability itself. Suppes has himself stated his case so succinctly that I will merely repeat it here:

> _Premise 1_: In physical or empirical contexts involving the application of probability theory as a mathematical discipline, the functional or working logic of importance is the logic of the events or propositions to which probability is assigned, not the logic of qualitative or intuitive statements to be made about the mathematically formulated theory. (In the classical applications of probability theory, this logic of events is a Boolean algebra . . . usually . . . a σ-algebra.)

<u>Premise 2</u>: The algebra of events should satisfy the requirement that a probability is assigned to every event or element of the algebra.

<u>Premise 3</u>: In the case of quantum mechanics probabilities may be assigned to events such as position in a certain region or momentum within certain limits, but the probability of the conjunction of two such events does not necessarily exist.

<u>Conclusion</u>: The functional or working logic of quantum mechanics is not classical.

Suppes supplies arguments in support of each of his premises. For these I refer the reader to his article. I will only quote one passage regarding premise 3. Suppes writes: "We conclude that in general the joint distribution of two random variables like position and momentum does not exist in quantum mechanics and, consequently, we cannot talk about the conjunction of two events defined in terms of these two random variables." In short, Suppes argues as follows: in classical probability theory we can assign probability to any two events and also the conjunction of those two events. In QT, in general, it is not possible to assign a probability to a conjunction of two events (even though we can assign a probability to each separately). And how do we know that in QT this is generally not possible? By the fact that in QT,

generally, there does not exist a joint distribution of two random variables.

Arthur Fine answers Suppes--and I think adequately--by going directly to the imputed reason why in QT we cannot assign a probability to a conjunction, viz, the non-existence of joint distributions for random variables. Fine shows that the quantities in QT are *not* random variable, but rather what he calls *statistical variables*. (This distinction was first explicitly made by Menger [43].) The basic distinction between the two is the following: (1) *given* a classical probability space $\langle X,f,P\rangle$ we can *always* define a real-valued function f, called a random variable, on X such that $f^{-1}(B)\varepsilon f$ for every Borel set B. (2) A statistical variable refers to a quantity whose set of values X is such that for some f and some P we can *construct* $\langle X,f,P\rangle$ such that $\langle X,f,P\rangle$ is a classical probability space. So, in a real sense, the difference amounts to this: which comes first? the classical probability space on which you define the variable (i.e., random variable), or the variable (i.e., statistical variable) about which you construct a classical probability space.

The benefit gained from making this distinction is seen in the following: a joint distribution for two random variables *always* exist, whereas, a joint distribution for two statistical variables may or may not exist. The non-existence of joint distributions for statistical variables

is neither surprising nor disturbing. [As to the claim that the quantities of QT are not random variables, all we need do is look at any textbook on QT. It is easily seen that no one has ever pretended that the quantities were functions on the events of some pre-established classical probability space. What QT does in fact, is to take a given quantity and assign to each set of its values the probability that the quantity takes a value from that set. (See the spectral theorem for hermitian operators.) Thus the actual procedure in QT is to treat each quantity just as it comes, but to incorporate the set of <u>values</u> of the quantity into a classical probability space.]

Hence the non-existence of joint distributions for the quantities of QT shows not that the probability used is non-classical, but rather that the quantities referred to <u>cannot</u> be random variables. And if we examine the way QT does <u>in fact</u> handle its quantities and associated probabilities, we see that the quantities of QT are not random variables but rather statistical variables.

Fine has shown that given a non-empty set of questions (or propositions) Q and a non-empty set of states S, each state $s \varepsilon S$ is a real-valued function from Q into the interval [0,1]. That is, for each state s, each question q induces a probability measure P_q^s which is a classical probability. A different question, say p--for the same state s--induces a <u>different</u> classical probability measure P^s_p. It is important to stress that both of these are classical probability measures. Now, given the state s if we try to ask both questions q and p at the same time, we would have a function given by $P^s_{p,q}$. This would be the joint distribution of p and q in the state s. But $P^s_{p,q}$ does not in general exist. (It turns out that the conditions for the existence of $P^s_{p,q}$ are just the same as the well-known conditions of compatibility of propositions or commutativity of operators.) When $P^s_{p,q}$ exists, it too is a classical probability measure. What this means is that the two component probability measures P^s_p and P^s_q can be combined into one "joint" probability measure $P^s_{p,q}$. But since we have seen that in general this is not possible in QT, it follows that in general it is not possible to form one "over-arching" probability measure which will apply to <u>all states</u> and <u>all questions</u>. But in spite of this, the probability used in QT for each state and for each question remains as classical as ever. (It is as though Suppes wants to take one element from a given probability space P^s_p--whose underlying structure <u>is indeed</u> a Boolean algebra--and another element from a different probability space P^s_q--whose underlying structure is also a Boolean algebra--and then demand that these elements so chosen form themselves a Boolean algebra. We have seen that this demand is not met in the general case. But we have also seen that this condition is not required in

order to have a classical probability measure for each probability that we do in fact use.)

In a loose sense, Fine's answer to the question whether QT uses a non-classical probability is yes and no. On a grand scale no, because it is not always possible to form a "comprehensive" probability measure. But on the small scale, in each individual case in which probabilities are actually used, yes. But the impossibility of forming a joint distribution (and hence the impossibility of forming a "comprehensive" probability measure) is an odd feature of QT that neither arises from nor leads to the incursion of any sort of non-classical probability in the theory. In his article, Fine provides a model for a classical probabilistic framework that is both adequate for the formulation of QT, and faithful to its applications.

Thus, I believe the sting has been dissipated from Suppes' objection. And again we have not found sufficient grounds for claiming that QT employs a non-classical probability.

Experiment As Entrance of Subjectivism Into QT.--Is experiment an avenue by which subjectivism can enter QT? The answer is certainly yes! For it is precisely because of the nature of experimental results that we have the irreducible, irremovable probabilities in QT. And we have seen that the presence of these probabilities does involve, to some extent or other, a renunciation of the "objective" character of physical systems and physical phenomena. But I must point out that in my initial presentation of the case for subjectivism, I made no mention at all of the many controversies which have raged over just these issues. It is abundantly clear that the problem of subjectivism is somehow connected with the so-called problem of measurement. And the many different points of view with respect to that problem are well-known. But I wish to claim that the case for microscopic subjectivism (i.e., subjectivism, restricted, for the present, to the level of microscopic particles and events) can be made without having first a complete and satisfactory solution to the problem of measurement. Hence, to this extent these two problems can be treated separately (i.e., the problem of measurement and the problem of microscopic subjectivism). Thus, it is possible to make the case for subjectivism without actively considering all the difficulties associated with the problem of measurement itself. To show this as clearly as possible, I have tried to show the bare skeleton of the argument in the form of four premises and a conclusion. I will first state the premises and the conclusion, then I will give reasons for accepting each premise. I hope, by this, to show that: (1) the conclusion of microscopic subjectivism is an unavoidable one; and (2) this conclusion can be

reached without getting ourselves embroiled in the hotly debated questions concerning the problem of measurement.

For the sake of simplicity let us consider a physical system which can assume one of two values for a given observable (property). Let the property be the spin of the system. Hence any fermion (spin one-half system) will do. We know that the system can exist in a state ψ_1 with its spin being ζ_1; or it can exist in a state ψ_2 with spin ζ_2. But <u>according to the superposition principle</u> the system can also exist in a state $\psi = a\psi_1 + b\psi_2$ (as we have seen earlier). Now the argument goes as follows:

1. $\psi = a\psi_1 + b\psi_2$ — This is a possible state of the system, and furthermore, it is a <u>spin state</u>.
2. But to the system in state ψ (before measurement) we cannot attribute either ζ_1 or ζ_2.
3. Thus, to the system in state ψ (before measurement) we can attribute no spin whatever.
4. But whenever we observe the system, the system <u>always</u> has a spin associated with it (ζ_1 or ζ_2).

Conclusion: Thus, ζ_1 (or ζ_2) comes to be associated with the system <u>only</u> in the act of observation.

Premise 1: (A) ψ is a state. For support of this claim we can, quite simply, make appeal to the principle of superposition. This principle demands that the superposition exist and that this be a possible state of the system. As to the question "Is this state not only possible but also actual?," we can appeal to the host of interference phenomena which require the superposition principle for their satisfactory explanation. There is, thus, both theoretical and experimental support for this claim.

(B) ψ is a spin state. For support of this claim we notice that: (i) ψ can be expanded in a complete, orthonormal set of states (here just ψ_1 and ψ_2), which are themselves spin states; (ii) ψ can be adequately described in terms of the Pauli spin matrices just as are ψ_1 and ψ_2; (iii) ψ exhibits all the characteristics properties of spin states, e.g., coupling with magnetic fields.

(C) ψ is a physically different state from either ψ_1 or ψ_2; and ψ has properties that neither of the states ψ_1 or ψ_2 has. (See Wigner [57].)

Premise 2: Wigner has shown (in the same article mentioned above) the inconsistency between decomposing ψ into ψ_1 or ψ_2 (and thus attributing ζ_1 or ζ_2 respectively) and the superposition principle. Thus, to avoid contradiction, we are forbidden to say that our system is, on some occasions, actually in state ψ_1 possessing spin ζ_1 and on some occasions actually in state ψ_2 with spin ζ_2. (Or using an ensemble of many systems: we are forbidden to say that some of the systems are actually in state ψ_1 with spin ζ_1, and some of the systems are actually in state ψ_2 with spin ζ_2.) If this ψ is to be the state function satisfying the Schroedinger equation, then it must <u>not</u> be decomposed, in any way, into the constituents ψ_1 or ψ_2. But whereas ψ_1 and ψ_2 both have a definite value of spin associated with the state, ψ has <u>no</u> definite value of spin associated with it. Hence, if we are forbidden to associate with our system either ψ_1 or ψ_2, then we are also forbidden to associate with the system in state ψ either ζ_1 or ζ_2.

<u>Premise 3</u>: If we cannot attribute to the state ψ either ζ_1 or ζ_2, then there is no spin at all that we can attribute to the state ψ. (And this is equivalent to saying that a system in state ψ has no spin whatever associated with it.) This is because ζ_1 and ζ_2 are the <u>only two</u> spin values which we could conceivably attribute to the state ψ. For these are the only two values of spin for which QT provides any warrant at all for associating with the state ψ <u>in any way</u>. Consider two cases: (1) if an observation is made; (2) if an observation is not made. (1) The only values that are ever obtained for the spin are ζ_1 and ζ_2. This is in agreement with: (a) the postulates of QT, and (b) experimental fact. (2) If an

observation is not made, we know that we cannot attribute a definite value of spin to the system. But QT does allow us to associate with each possible value of the spin a certain probability of occurrence upon observation. And even here the only spin values ever mentioned with respect to the state ψ are ζ_1 and ζ_2. It is as though we have the following logical statement

$$(\psi)(\zeta_1\psi \vee \zeta_2\psi \vee A_{\substack{\sim\zeta_1 \\ \sim\zeta_2}}\psi \vee N\psi)$$

that is, for every system in the state ψ, either we can attribute to it ζ_1, or we can attribute ζ_2, or we can attribute *any* other spin besides ζ_1 and ζ_2, or we can attribute no spin at all to the system. (This is certainly in agreement with orthodox QT, for by definition a complete set of eigenfunctions is exhaustive of the state function for which it is an expansion. To use other words, the eigen-vectors which form the complete set span every subspace of the Hilbert space associated with the system. To be sure, the complete set here consists of just ψ_1 and ψ_2, but the number of members in the complete set is of no importance. But this does mean that either you are in the subspace spanned by ψ_1 or the subspace spanned by ψ_2. There are no other subspaces.)

It is worthwhile to point out *that we have here a state with a given determinable property*, but to which

we can attribute <u>none</u> of the particular determinate properties--even though the determinate properties we have in mind do exhaust all the possibilities for the previously ascribed determinable property. This is like saying we have in a plane a line and a point off the line, and we know that the point has <u>some location</u> or other (determinable property). But we know the point is not above the line and we know that the point is not below the line. And this clearly exhausts all the possibilities.

<u>Premise 4</u>: As indicated above this is simply: (1) in agreement with the postulates of QT; but more importantly, (2) a matter of experimental fact. Hence, our situation is now this: if we do not observe the system in state ψ, we have a system with a determinable property (is a spin state) but without a determinate property (no definite value of spin). But if we do observe, we have a system with both a determinable and a determinate property (is a spin state <u>and</u> has a definite value of spin associated with it).

<u>Conclusion</u>: Hence, if before observation the system had a determinable property but no determinate property, and as soon as we like after observation the system had both a determinable property and a determinate property, then the determinate property (ζ_1 or ζ_2) came to be associated with the system <u>only in the act of observation</u>.

At this point an objection might be raised: alright, even if we admit that before observation the system had no definite spin, and after observation it did have definite spin, your point is not made (i.e., that the determinate property came to be associated with the system only in the act of measurement). For you have failed to realize that you are talking about two different states: before the observation, when there was no definite spin, the state of the system was ψ; but after the observation, when there was definite spin, the state is no longer ψ, the state has changed. And there is nothing unusual about having two different situations (with different implications regarding the spin) when the system is in two different states.

I would like to make two replies to this objection. (1) Clearly, here is a point where we find ourselves facing one of the questions traditionally associated with the problem of measurement, viz, the projection postulate. Roughly put, this is the claim that a change of a certain sort occurs in the state of a physical system when an observation is made on the system. In particular, if $\psi = \sum_i a_i\psi_i$ before observation, then after observation the system is no longer represented by ψ, but rather by one of the ψ_i. That is, the state has changed from ψ to ψ_i. Clearly, the force of the above objection rests on the validity of the projection postulate (in some formulation or other). Some have argued against the projection postulate [40]. But we can make our reply without taking sides on the projection postulate issue.

(2) Regardless of the outcome of the projection postulate debate, the objection can be avoided. Even if we grant the projection postulate and admit that we are dealing with two different states, it is still true that we are talking about the <u>same physical system</u>! And we have shown above that the system had no definite spin before the observation, but had a definite spin after the observation. I would end my replies to the objection with the following observation: (even admitting that there are two different states) though there may be nothing unusual about having two different <u>values</u> for the spin when the system is in two different states, there is certainly something unusual about having "two different <u>situations</u>" for the spin when the system is in two different states, if the "situation" in one of the states entails a definite <u>value</u> for the spin, and the "situation" for the other state has <u>no value at all</u> associated with it!

I would like to conclude this section with some remarks on an article by Joseph Sneed [52] in which he criticizes an empirical argument advanced by Von Neumann for the projection postulate. Though his objective in that paper was much different from mine here, some of his observations are pertinent. The point I wish to establish, by this consideration of Sneed's work, is that the import of the conclusions we draw here are of an ontological rather than an epistemological nature. I wish to make the point that, if we take our theory seriously, if we attribute any empirical import to our theory, then we must allow that when the theory makes an assertion this is a reflection of what is "going on" in nature. Hence I wish to claim that the limitations we discover in our inquiry are not merely limitations on our ability to know about phenomena; rather they are limitations upon the very nature of the phenomena themselves.

Sneed gives an account of an experiment (collision of an x-ray with an electron) which Von Neumann mentions. The idea is that by observing the paths of the recoil particles (by use of counter detectors), we can determine the central line of the collision (i.e., the line along which the vector sum of momenta of all particles lies). Von Neumann considers the detection of the two recoil particles as two different measurements, M_1 and M_2, of the same observable, R (the central line). Then, according to Sneed, Von Neumann's argument goes something like this:

1. Prior to making a measurement of R we are justified in making only statistical statements, i.e., probability statements about the value of R that will be obtained when any measurement of R is made.
2. After M_1 occurs, yielding the value r for R, but before M_2 occurs, we are justified in making the statement that the value of R which will be obtained when M_2 occurs will be r.

3. Therefore, the state of the physical system, on which M_1 and M_2 are made, changed when M_1 occurred, from a state in which we are justified in making only statistical statements about the value of R which will be obtained from any measurement of R, to a state in which we are justified in making a statement that some one particular value of R will be obtained from any measurement of R.

Sneed denies the soundness of the argument and states that

> the only thing that is described as changing when M_1 occurs is the membership of the set of statements one is justified in making about the result of M_2. Indeed, that this change does not follow from (1) and (2) unless one adds the addition premise:
>
> A. If (i) one is justified in making only statistical statements about the result of any measurement of R on physical system P then it is not the case that (ii) one is justified in making a statement that a certain result will be obtained from a particular measurement of R on P.
>
> It seems quite natural to regard A as being true by virtue of the meaning of "statistical statements," and consequently to regard it as an implicit assumption of Von Neumann's argument. However, in order to conclude that the state of a physical system changed, something in addition to A is needed. The following additional premise would be sufficient to allow this conclusion to be drawn:
>
> B. The state of a physical system P when (i) is true of P is different from the state of P when (ii) is true of P.

Sneed then considers how to justify B given A. He suggests that B may follow deductively from A given some statement like:

> C. The state of a physical system is identical with the set of statements one is justified in making about the results of measurements made on P.

Sneed then concludes:

> One might ordinarily say that the state of a physical system (at a given time) is, or is described by, the

> set of all true statements about the system (at this time). We would not normally identify the state of a physical system only as the statements we were justified in making about it. Clearly, the set of statements one is justified in making about a system might change even though the set of statements which are true about the system did not change.

Sneed has drawn a distinction between "the set of statements one is justified in making about a system" and "the set of statements which are true about a system." This is a distinction which, in general, would find sympathetic listeners. But if made without extreme care, it is a distinction which I think to be quite insidious. The difficulty I have in mind can be illustrated by simply asking the following question: how do we determine the membership in the set of statements which are true about a system? That is, how do we know which statements are true as opposed to those statements which we are merely justified in making? An example will help. We are told that a coin is flipped. Nothing more. We are surely <u>not</u> justified in saying (about the coin) that it landed heads up (or tails up). We <u>are</u> justified in saying only that the probability of heads up is a certain number p and the probability of tails up is a certain number $q = 1 - p$. So we have the set of statements which we are justified in making about the coin (our system), viz, the two probability statements. Now, what is the set of statements which are true about the system? (We are, of course, restricting our inquiry to just the heads-or-tails status of the coin, not its temperature or color or shape, etc.) That set of statements consists of exactly <u>one</u> statement: <u>either</u> heads up <u>or</u> tails up. We just happen not to know which one right now. (It is true, of course, that there are an infinite number of other statements, all "physically" equivalent to these two, that could be included in the set of true statements (e.g., the statement 'either heads or tails'), but this does not change the conclusion reached below.) But a quick look, an observation, will take care of that. Suppose we look and we find heads. Well if the coin is heads now it is certainly true that it was heads before we looked. For, since nothing is happening to the coin besides our looking at it, it must have been heads before we looked or how else would we have seen heads when we did look? Thus, here is a clear case of a difference between the set of statements we are justified in making and the set of statements which are true about a system. Or so the argument goes.

But in order to conclude that the coin was heads before we looked, it was necessary to infer the truth of a statement at an earlier time from the truth of a "corresponding" statement at a later time. That is, we concluded that the coin was heads <u>before</u> we looked, from the fact that the coin was heads <u>after</u> we looked. But as we have seen already in too much detail, this kind of "retroductive inference" is precisely what is forbidden in QT: e.g., from the observation of spin ζ_1 for a system, we certainly <u>can not</u> conclude that the spin of the system was ζ_1 <u>before</u> the observation. Hence it is seen that this proposed method of determining which statements about a system are true, as opposed to those statements which we are merely justified in making will simply not draw the distinction for which it was intended. But this proposed method was nothing more than observation, plain and simple. And appeal to any other method besides observation can easily lead us out of the realm of science and into metaphysics of the worst kind.

I would like to maintain that the distinction drawn by Sneed is an indefensible one. The idea that there are statements which are true about a system over and above (or independently of) those statements we are justified in making is an unexamined carry-over from the time of classical physics. In classical physics we had a picture of a uniquely determined world developing quite independently of our knowledge or ignorance of its progress. Classical physics was at once the result, the corroboration and the determiner of our intuitive feelings about the way the world "really" is. Hence, we could <u>always</u> maintain our idea of what was true about a system as opposed to what we were merely justified in saying. What was true was the actual state of the independently developing physical world; what we were justified in saying depended on the extent of the knowledge we had about the independent development (and remember, we could always make that knowledge approach as closely as we pleased to the actual state of the world).

But I wish to argue that this distinction requires re-examination in QT just as does the notion of a "state" require re-examination in QT. Certainly, observation would be the likely candidate for telling what is true (and then hopefully, what <u>was</u> true) about a system. But when that fails, we must make appeal to other critiera. Now this does not mean that any other criteria will automatically be unfit. But they should be carefully scrutinized. For in the absence of any empirical grounding of such criteria, even a "reasonable-sounding" criteria can turn out to be mis-directed and can lead us along a fruitless path. This danger is especially present in QT. For here we are in a strange land, where our intuitions about what sounds like a reasonable extra-empirical criterion were formed in another place and time, viz, in classical physics. And we should be naturally suspicious in a strange land that our previous biases <u>might</u> not retain their validity (if indeed, they ever really had any to begin with). The historical development of QT strikingly confirms us in this suspicion.

(The Einstein-Podolsky-Rosen paradox [20] is such a beautiful paradox because the initial, extra-empirical criterion imposed by Einstein, Podolsky, and Rosen is such a "reasonable" one. And one to which we are all surely sympathetic. But this was a criterion born out of classical physics and out of a mind "raised" on classical physics and a classical-physics view of the world (his theory of relativity notwithstanding). But what is reasonable in classical physics might not be reasonable at all in quantum physics. And this, in essence, was Bohr's answer [11] to Einstein, Podolsky, and Rosen.)

Am I trying to say that all there is of the physical world is what we know of it? What I am trying to say is that if what we claim to have are empirical theories, theories about the world, then we must regard them as such. And we must regard their pronouncements as having some significance and relevance for the world they are supposed to describe. I realize that the questions which are at issue here are difficult and complex ones, and do not admit of simplistic answers. But I also think that if we are to allow any descriptive content to our theories at all, then we must also allow that content to be descriptive of the world to which the theory applies. I do not wish to imply that we must accept a theory lock-stock-and-barrel, like it or not. I do believe that some "extra-theoretical" considerations are appropriate and even necessary. I only wish to maintain that if a conflict arises between the two, we should be strongly inclined to reject the "extra-theoretical" considerations--especially when those considerations can be clearly seen to arise out of a theoretical framework different (and in some aspects, alien) to the one under discussion. And I certainly believe that the notion of what is true about a system falls into this category.

Returning to Sneed, we see that an analysis of his premise C shows us that it is indeed true, i.e., that the state of a physical system is described by the set of statements one is justified in making about the results of measurements made on the system P. To use borrowed terminology of Henry Margenau [40] and Josef Jauch [29]: a state is the result of a series of physical manipulations on the system which constitute the preparation of the state. Two states are identical if the relevant conditions in the preparation of the state are identical. And what does a state tell us? We have seen that it is a fundamental fact of QT that identical states do not yield identical results for the truth or falsehood of a proposition. Thus, a measurement of proposition a may sometimes give the value 1 (true) or 0 (false). But if we know the state (the relevant conditions in preparation), we can form a probability measure and can determine the probability of any proposition a. And since a proposition is just a yes-no experiment, this means we can give the probability of any experimental outcome, i.e., any measurement made on the system P. So the state determines what statements we are justified in making about the results of measurements made on P.

Thus, since we no longer have this store of "true" statements, the best we can do is the set of statements we are justified in making. Now, the change (admitted by Sneed) in the set of statements we are justified in making is an important one. For the set of statements we are justified in making is just what the theory tells us, and what the theory tells us is--for all purposes--what the world is like. Thus, if what the theory tells us changes, what it is that is described by the theory, i.e., the world itself, also changes. Then all of Sneed's remarks are seen to be in good agreement with my position. Indeed, Sneed (later in his paper) proposes "to give up the view that a particular physical system must possess a unique value (in the usual sense of numerical value) for every observerable associated with it." This statement of Sneed's becomes even more pointed when we realize that in some cases the physical system not only possesses no unique value, but indeed possesses no value at all!

But let us notice that even if we are willing to admit the demonstrated subjectivism for electrons, protons, etc., we have no inclination whatever to ascribe this same subjectivism to the coin we mentioned above. And this is true whether we are acting as philosophers, scientists, or men-in-the-street. In spite of what QT obliges us to say about micro-systems, we still do believe that the coin was definitely heads <u>before</u> we looked at it. But if we believe the micro level to be that which "makes-up" the macro level, what happens to the subjectivism as we move from electrons to coins? Is the subjectivism <u>in fact</u> still present? Do we have any basis for believing that it is not? In particular, are there any physical reasons --i.e., reasons that can be provided by QT itself--for rejecting this subjectivism on the macro level? It is to these questions that we must now turn.

PART III

CONSEQUENCES OF MICROSCOPIC SUBJECTIVISM: POSSIBLE RESPONSES TO CLAIM OF MICROSCOPIC SUBJECTIVISM

Given the results of Part II it is fitting to ask what consequences flow from these conclusions? Just how serious is this subjectivism? How do we incorporate this new-found subjectivism into our world picture--as given by science? What adjustments or modifications are required in the way we think of the physical world in view of this subjectivism? Even if one is forced to admit the subjectivism we have discussed, there certainly seems to be nothing of the sort in our everyday experience. Can we account for both of these "facts" at the same time? In short, how do we _respond_ to this claim of irreducible subjectivism in physical nature?

In what follows I shall, on numerous occasions, refer to the distinction between _microscopic_ and _macroscopic_. By microscopic I mean that realm (of events, phenomena, interaction, systems, etc.) which is specifically governed by the laws of QT. By macroscopic I mean that realm in which the laws of QT are generally not required for the explanation of phenomena, and the laws of classical physics are for the most part adequate. Generally speaking, the microscopic realm is associated with small size, say, atomic and sub-atomic size; and the macroscopic realm is associated with large scale bodies (i.e., large relative to the microscopic bodies). This includes the familiar world of tables, chairs, billiard balls, etc. But the essential difference to keep in mind is that the microscopic is associated with specifically quantum effects and behavior; whereas the macroscopic is devoid of these effects.

For the most part, the macroscopic bodies we will discuss will be macroscopic measuring devices. That is, we will be specifically interested in those bodies, of macroscopic size, which obey the laws of classical mechanics and in addition whose states can serve as the final states of a device designed to give us information about a measured (observed) system. Hence, though it is possible to observe specifically quantum behavior with "large" bodies--in, say, the Josephson effect, properties of superconducting bodies, the emission of light--these will not be macroscopic systems suitable for use as a measuring device (in which we must have macroscopically distinguishable final states).

There are several responses one can make in face of the claim of irreducible subjectivism on the _micro_ level, ranging all the way from saying it is an accursed lie to saying this subjectivism extends throughout the entire universe. _A_. One could simply _deny_ the subjectivism even on the micro level. Here, there would be no question of incorporating the results into our overall physical picture. Rather, the energy expended here will be to the effect of showing how we can avoid such a result to begin with. _B_. One could _accept_ the subjectivism on the micro level, but wish to _reject_ it on the macro level, so that we could still maintain a more or less _realistic_ view with regard to the events of our daily experience. It would be incumbent upon holders of this view to show exactly how we can maintain this _dualistic_ position: subjectivism on the micro level and realism (objectivism) on the macro level. This problem becomes especially significant if one wishes to hold that in some way or other, the macroscopic is composed of, made-up of, constituted by, the microscopic. These proponents must tell us exactly how we can account _physically_ for this dualism. They must determine if QT can explain this situation adequately. They must determine if QT can tell us what happens--if anything --as we go from the micro level to the macro level to account for the "disappearance" of the subjectivism. There may be _philosophical_ problems associated with

holding such a dualistic view, but this is also a problem for physics, in the sense that this view must be compatible with present physical theory. If there are philosophical considerations which make this position unacceptable, then they must be weighed. But also, physics must tell us if this position is even scientifically acceptable. We will concentrate here on the physical issue.

C. A further response to the claim of microscopic subjectivism would be to accept not only the subjectivism on the micro level, but to accept it on the macro level as well. Here we will wish to distinguish between those who wish the subjectivism to apply to only inanimate (i.e., without consciousness) macro bodies, and those who wish the subjectivism to apply also to conscious observers.

D. A very interesting possibility would be to respond in somewhat the following way: this label of so-called "subjectivism" is worthless because, as a matter of fact, the distinction between the "subjective" and the "objective" is a meaningless one. We have no way to distinguish one from the other, and thus any attempted distinction is futile. We will see in detail what this proposal entails below.

It is clear that of all the different kinds of responses just outlined, that type under group B is the least reactionary (most middle of the road)--in the sense that this response would require the least amount of adjustment of our overall world picture (while allowing us to <u>accept</u> the conclusion of microscopic subjectivism). For this view would allow us to wholeheartedly accept the conclusion of the new physics (in its proper realm of application, viz, with respect to microscopic phenomena), while at the same time permitting us to preserve, intact (at the macroscopic level), that world picture which has served us so well for so long, viz, that associated with classical physics. Hence, we would be able to weave into our total picture, the new threads required by the new micro physics, without completely destroying the overall pattern in scientific thinking predominant from the seventeenth century. This would seem to be the most desirable avenue to take, not only for the sake of preserving tradition, but also since we have no way of knowing what the "correct" explanation will be, the (pragmatically) easiest and therefore (pragmatically) wisest thing to do is to make the change that disturbs the total explanatory system least. (I am aware of course of the important instances when the best explanation was also the most radical. However, the great majority of our explanatory attempts are clearly <u>not</u> of this kind. These radical revisions are the "revolutions" that are, almost by definition, few and far between. In other words, I intend this to be an "all-other-things-being-equal" statement.) At any rate, there is much (understandable) sentiment for this kind of solution. So I shall give only a brief discussion of the proposals in groups A, C, and D: and treat those under B in greater detail.

A. Deny Microscopic Subjectivism

I wish to consider in this section the views of those authors who respond to the claim of irreducible subjectivism by denying it. Those writers do not attempt to discover what consequences follow from the conclusion of microscopic subjectivism; they simply deny the conclusion and reject the subjectivism altogether.

Hidden variable theories. The first kind of proposal I wish to consider are the so-called hidden-variable theories. By this I have in mind any attempt to remove the novel features of QT (i.e., the subjectivism) by replacing the statistical QT on the micro level with a deterministic theory on, say, a sub-micro level. Or, to put it less crudely, this is an attempt to introduce hidden variables in order to characterize a phase space of microstates on which real-valued functions can be defined to represent the physical properties of micro objects in the manner of classical mechanics, so that the peculiar statistical relations of QT are simply explained by the incompleteness of the theory. Then when the variables associated with these "hidden states" are taken into account, we obtain a completely deterministic account of observed phenomena--the indeterministic nature of the account in orthodox QT being explained by the fact that we simply had an incomplete specification of all relevant data.

The first thing we must note is that a great deal of confusion exists regarding hidden variable theories. For example, contrary to popular opinion, the "hidden variable program" as stated above is manifestly <u>not</u> what was attempted by Bohm [6] and his collaborators. But at the same time, this kind of program has been the object of many discussions and proofs and has been the <u>raison d'être</u> for much ridiculing of Bohm. Hence, I shall wish to be especially clear about what we have in mind as we discuss hidden variable theories below.

Though it is true that Bohm does not advocate the simple minded kind of theory outlined above, many proofs have been directed to this kind of proposal. It is worthwhile to examine the import of those proofs. The first thing that comes to mind in any discussion of hidden variables is the now-famous proof of Von Neumann. But, as is certainly well known by now, the proof of Von Neumann --to the effect that hidden variables are impossible--does not prove all that it claimed to. We will consider, rather, the work of Kochen and Specker [33]. It can be shown that the Von Neumann proof is a weaker version of the same problem as re-formulated by Kochen and Specker: i.e., the Von Neumann proof does not rule out a large class of hidden variable extensions of QT that are ruled out by the theorem of Kochen and Specker. Hence a study of the work of Kochen and Specker will give us our strongest conclusions.

Kochen and Specker identify the physical parameters of QT with the hermitian Hilbert space operators. Now, a hidden variable extension of QT, as usually understood, involves the interpretation of the hermitian operators in Hilbert space as representing the physical attributes of objects. The problem--for the hidden variable theorists--is then to embed the QT into a more fundamental theory, so that these attributes are represented by real-valued functions on a phase space of hidden states, in such a way that it becomes possible to give a statistical mechanical derivation of the statistical theorems of QT in the classical manner. Kochen and Specker show that <u>this kind of embedding is impossible</u>.

However, if the physical parameters of QT, as represented by hermitian Hilbert space operators, are not interpreted as referring to attributes or properties of "quantum objects," then there is no motivation at all for developing a hidden variable theory (of the type discussed here). Kochen and Specker point out that the hidden variable problem without a certain condition which they derive is trivial: it is always mathematically possible to introduce a phase space, and to associate real-valued functions on the phase space with the physical parameters of a statistical theory, in such a way that the statistical theorems are recovered. The special condition of Kochen and Specker imposes a structure on the set of physical parameters suggested by their interpretation as physical attributes of objects. However, it is just this interpretation which is incompatible with the representation of physical parameters by hermitian Hilbert space operators. Hence, any proposal which maintains this interpretation is seen to be indefensible.

However, if one rejects this interpretation--of the physical parameters of QT as physical attributes of objects--then we have quite a different story. This is done by Bohm. We will examine shortly his rather unexpected results.

In addition to the results of Kochen and Specker, some recent work by J. S. Bell [3,4] places some very stringent restrictions on any hidden variable theory (including the type we have been discussing). QT predicts the probabilities of the various possible results of a measurement, not the individual results themselves. Bell asks if a more complete theory is possible: is it possible to have a theory that agrees with the predictions of QT but predicts the individual results, instead of merely their probabilities?

Bell considers the Einstein-Podolsky-Rosen Paradox. (Actually, he considers a variation of the original problem as formulated by Einstein, Podolsky, and Rosen. He considers the measurement of the spin of two particles which have interacted, by allowing each particle to be deflected through a Stern-Gerlach apparatus.) He shows that it is easy to make a hidden variable model that will work if the result of the measurement on particle one is allowed to depend <u>also on the direction of the other magnet</u> (of the other Stern-Gerlach apparatus). However, if Einstein's hypothesis of the independence of separated non-interacting systems is accepted then Bell demonstrates that no such hidden variable model can agree with all the predictions of QT (to within variations of, say, 5%). Thus, Bell concludes: "In a theory in which parameters are added to QT to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote. Moreover, the signal involved must propagate instantaneously, so that such a theory could not be Lorentz invariant." (For a more detailed discussion of Bell's theorem, see appendix.)

Besides being a very stringent restriction on hidden variable theories, this conclusion is significant because it essentially echoes the conclusion of Bohm--who started out explicitly adopting the hidden variable approach. In both Bell and Bohm we find a relaxation of the traditional concept of the independence of physical objects.

In a now classic paper, Moyal [45] investigated the possibility of reproducing the quantum statistics from joint probability distributions for position and momentum, i.e., of relating the statistical states of QT to a probability over a classical phase space. Moyal showed that the phase space probability distribution associated with a certain statistical state is not fixed but varies with the relevant physical parameters, i.e., the probability distributions may be said to be "relative to the measurement context."

Bohm, too, explicitly pointed out this aspect in his original 1951 "hidden variable" theory. In a section on Von Neumann's proof, he wrote [6]:

> His conclusions are subject, however, to the criticism that in his proof he has implicitly restricted himself to an excessively narrow class of hidden parameters and in this way has excluded from consideration precisely those types of hidden parameters which have been proposed in this paper. . . . For example, if we consider two non-commuting observables, p and q, then Von Neumann shows that it would be inconsistent with the usual rules of calculating quantum-mechanical probabilities to assume that there were <u>in the observed system</u> a set of hidden parameters which simultaneously determined the results of measurement of position and momentum 'observables.' <u>With this conclusion we are in agreement</u>. However, in our suggested new interpretation of the theory, the so-called 'observables' are . . . not properties belonging to the observed system alone, but instead potentialities whose precise development depends just as much on the observing apparatus as on the observed system. . . . Thus, the statistical distribution of 'hidden' parameters to be

> used in calculating averages in a momentum measurement is different from the distribution to be used in calculating averages in a position measurement. Von Neumann's proof . . . that no single distribution of hidden parameters could be consistent with the results of QT is therefore irrelevant here, since in our interpretation of the measurements of the type that can now be carried out, the distribution of hidden parameters varies in accordance with the different mutually exclusive experimental arrangements of matter that must be used in making different kinds of measurements. In this point, we are in agreement with Bohr, who repeatedly stresses the fundamental role of the measuring apparatus as an inseparable part of the observed system. [Italics added.]

Hence, even on Bohm's proposal we find that we do not recover a picture in which we have the deterministic development in time of independently existing entities. We find, rather, that we must entertain a picture in which objects no longer "possess" properties or attributes in the sense of these properties being characteristic solely of the object, and independent of any other object. There is now an "inter-dependence" among objects; a loss of that independence and complete "isolatability" that an individual object was once believed to have. And since this is precisely the conclusion to which we are driven by our acceptance of microscopic subjectivism, it is seen that Bohm's analysis is in satisfactory agreement with ours.

Purely statistical interpretation. We considered above the attempt to deny the basic claim of irreducible subjectivism by trying to replace the statistical laws of QT by deterministic laws on a sub-quantum level. And even though that was not the exact intent of some of the actually proposed hidden variable models (e.g., those of Bohm), we considered some of the restrictions that would have to be met by any theory of this type. We will now consider an attempt of a different sort to avoid the conclusion of an irreducible subjectivism in the micro world.

I now refer to the school of thought that advocates a purely <u>statistical interpretation</u> of QT [2]. The central issue is this: these writers maintain that QT is a theory about <u>ensembles</u> of systems, and not about <u>single</u> systems. In particular, they assert that a quantum state (pure or otherwise) represents an ensemble of similarly prepared systems. A quantum state, according to this view, does <u>not</u> provide a complete description of an individual system. But how do we then interpret the lack of specificity associated with a quantum state? They reply that this lack of specificity is <u>not</u> to be associated with the properties of a given individual system. Rather, each single system has well-defined properties at each instant; and this lack of specificity refers to the lack of complete uniformity among the observable properties of the different individual systems in the ensemble (to which the state function, by assumption, refers). That is, the statistical interpretation considers that physical systems which have been subject to the same state preparation will be similar in some properties, but not in all of them. And this difference among the different individual systems is the proper meaning of the "indeterminateness" of the state. Indeed, according to this view, the physical implication of the uncertainty principle is that no state preparation procedure is possible which would yield an ensemble of systems identical in all of their observable properties.

Many arguments, pro and con, have been given on this point, and the controversy has raged since the time of the Einstein, Podolsky, Rosen paradox. I do not wish to recount all these arguments giving their merits and demerits. Instead I will present a simple recent experimental result which I think tells decisively against the statistical interpretation.

Recently Mandel and Pfleegor [39] performed an experiment that is essentially just the double slit experiment. For the light sources they used two single-mode lasers, attenuated sufficiently so that generally only one photon was in the apparatus at any one time. To understand the experiment, it is crucial to remember that instead of a single light source, Mandel and Pfleegor used a double source; and furthermore, the two lasers were operated <u>independently</u> of each other. The two beams were superimposed on one another as a result of a small angle of difference in the direction of propagation.

Now essentially what happened was just this: even with the assurance of only one photon in the apparatus (for this see the experimental data), Mandel and Pfleegor found clear evidence of interference. Furthermore, when one of the lasers was turned off, all such evidence disappeared. They concluded that their experiment supports the well-known statement of Dirac: " . . . each photon interferes only with itself. Interference between different photons never occurs" [19].

How can we account for such a strange phenomenon? In the traditional double-slit analysis we can appeal to the interference of the wave fronts coming from the two slits. But here that does not seem possible since the experiment was intentionally designed to insure that the photon was from a single laser. But the resolving point here is that an uncertainty principle analysis shows that it is not possible to tell from <u>which</u> laser the photon came. Considering the photon as a particle and making an appropriate analysis in terms of position and momentum, it can be shown that if we identify the source as laser one or two, we will destroy the conditions required for the interference patterns (conditions from physical optics). But if the particle analysis is objectionable, Mandel and Pfleegor show that their results follows, in accordance with the usual principles of QT, from a superposition of the wave functions for electromagnetic fields from the two lasers. Thus, either because of the uncertainty principle or the superposition principle we are forbidden to know from which laser the photon was emitted. And

finally, it seems patently clear that in this experiment dealing with <u>single systems</u>, we have striking proof of the validity of QT not only statistically, but even for individual systems, thus laying to rest any contentions of restricting QT to merely ensembles.

B. Accept Microscopic Subjectivism, Reject Macroscopic Subjectivism

The kinds of solutions offered under group B seem to be the most "desirable," in that they would allow us to accept the new "fact" of microscopic subjectivism, but also allow us to reject this subjectivism on the macroscopic level--i.e., the level of familiar everyday experiences. For this reason we will postpone consideration of these proposals, and consider them in detail later.

C. Accept Microscopic Subjectivism, Accept Macroscopic Subjectivism

There are also those who, having accepted the subjectivism on the micro level, go on to entertain the possibility of subjectivism also on the macro level. I will distinguish, within this position, between those who wish to restrict the subjectivism to just inanimate (i.e., unconscious) macroscopic bodies, and those who wish to include also conscious observers. (Jauch clearly places himself in the first group, but does so rather subtlely. We will see more of his position later.) We will consider here first the work of Von Neumann [55] and then the work of Wigner [58].

Von Neumann argues that if we observe an object system I by use of an apparatus, system II, we will obtain the same results by use of an apparatus system III, to observe the combined system I + II. (This means in particular, that if the object system I was in a superposition, then the combined system I + II + III will also be in a superposition.) Likewise, a system IV, when used to observe combined system I + II + III, will again give the same results. (And again, the combined system of I + II + III + IV will be in a superposition.) This series can be continued on indefinitely, argues Von Neumann. In particular, the procedure can be carried on until the combined system is large enough to include the experimenter himself (in the sense that the combined system now includes the sensory apparatus of the scientist performing the observation). But, argues Von Neumann, the combined system <u>cannot</u> be extended to include the consciousness of the observer. We are perfectly free to draw the line between "object" and "apparatus" anywhere we wish all the way from the initial microscopic system to the physical body of the observer--we will obtain the same results. But with the consciousness of the observer we have the final demarcation line. We can, if we wish, continue to think of a superposition for the combined system of object plus apparatus all the way up to the consciousness of the observer. But here the superposition is invariably

reduced. Hence Von Neumann excludes the superposition (and thus the subjectivism) from the conscious states of the observer. But he does clearly allow the subjectivism to exist on the macroscopic (inanimate) level. That is, Von Neumann "takes QT seriously" and applies it to all (inanimate) systems whatever.

In a more recent article, Wigner has also "taken QT seriously," and entertained the question of the subjectivism extending to the consciousness of the observer. But Wigner is concerned with a serious difficulty facing any such account. Let the wave function of the observed system S be $\psi_s = \psi_s^1 + \psi_s^2$ where ψ_s^1 and ψ_s^2 represent mutually exclusive situations. Consider a device that measures accurately which alternative is realized upon a given measurement. The superposition principle then ensures that $\psi_s \otimes \psi_m$ must develop into a superposition of wave functions for the two alternatives:

$$(\psi_s^1 + \psi_s^2) \otimes \psi_m \longrightarrow \psi_s^1 \otimes \psi_m^1 + \psi_s^2 \otimes \psi_m^2$$

where ψ_m represents the wave function of the measuring device before the measurement; ψ_m^1 and ψ_m^2 represent the wave functions of the measuring device corresponding to the device having registered the occurrence of the respective alternatives. Now if we go further and include the observer in the quantum mechanical system, then we have

$$(\psi_s^1 + \psi_s^2) \otimes \psi_m \otimes \psi_o \longrightarrow \psi_s^1 \otimes \psi_m^1 \otimes \psi_o^1 + \psi_s^2 \otimes \psi_m^2 \otimes \psi_o^2$$

where ψ_o^1 and ψ_o^2 correspond to the observer's having observed alternative one or two respectively.

But we recall now that the alternatives one and two are mutually exclusive. Yet our theory gives a result that requires both mutually exclusive possibilities to exist in nature--until such time as the superposition is reduced by a measurement. Thus, if we have Wigner's friend observe the result of a yes-no experiment, QT tells us that for the compound system including the state of the observer, we must have a superposition of yes and no states. Hence, until Wigner asks his friend for the result of the experiment which was observed--thus performing the measurement--his friend must be in a state which includes both the yes and the no outcomes of the experiment. It thus appears that until he is asked, Wigner's friend does not know himself which alternative did in fact occur.

If we substitute for Wigner's friend some simple physical apparatus, such as an atom which may or may not be excited as a result of the yes-no experiment, then " . . . there is no doubt that $[\psi_s^1 \otimes \psi_m^1 \otimes \psi_o^1 + \psi_s^2 \otimes \psi_m^2 \otimes \psi_o^2]$ describes the properties of the joint system correctly, the assumption that the wave function is either $[\psi_s^1 \otimes \psi_m^1 \otimes \phi_o^1$ OR $\psi_s^2 \otimes \psi_m^2 \otimes \psi_o^2]$ does not" [58], because there is a difference in observable effects between these two possibilities. Wigner concludes that a being with consciousness must have a different role in QT than an inanimate measuring device. Also, he feels that this argument is entirely cogent as long as we accept the tenets of orthodox QT in all their consequences. He suggests that there is a violation of physical laws where consciousness plays a role. (In this particular case, perhaps there is a breakdown in the linearity of the equations of motion.)

D. Subjective/Objective Distinction Is Useless

The adherents [18,21,56] of this view do not wish to tamper substantially with the conclusions of Part I. They accept the existence of the superposition on the microscopic level (which is hardly surprising), and they admit that some ontological consequences will follow. But they interpret the meaning of the superposition in a different way. Crudely put, these authors replace the "indeterminate-ness" of the state with the "indeterminate-ness" of reality itself, or with the "indeterminate-ness" of the universe as a whole.

Wholesale application of the principle of superposition to all objects or systems seems to drive one to the conclusion that several mutually exclusive possibilities all somehow exist in nature at the same time. These authors accept this conclusion at face value and then go on (understandably) to show how our idea of "nature" must accordingly be modified. First, each component state of the superposition represents reality, i.e., a possible world. And since there never is any such thing as a reduction of this superposition (they say), each component is <u>realized</u>. That is, there is a <u>real</u> world existing for each of the components, and there are as many different real worlds as there are components. Thus the <u>full</u> physical world would contain a superposition of a myriad of interconnected physical worlds of the kind we know. An individual observer would personally be aware of only one response of a macroscopic measuring device, but a full account of reality would include all the other possible responses on an equal footing, though perhaps with unequal "weights." Thus human observers would be aware of only individual branches of the full reality of the world.

Now though these authors will readily admit that this proposal is counter-intuitive, they maintain that it would not involve any conflict with experience. For the mutually exclusive possibilities would be essentially non-interfering, e.g., the memories of individual observers could not contain cross references to the non-interacting branches of the wave function associated with the various "incompatible" possibilities. Thus no individual would be aware of more than one branch of the full objective reality. (Hence, the different "worlds" are in perfect peaceful co-existence.)

It is now clear why these authors can be described as advocating the abolition of the subjective/objective distinction. For if the "objective" is thought of as somehow existing independently of any particular individual, and the "subjective" is thought of in some way as a particular aspect of objective reality as-apprehended-by a given individual, then it is seen why, under this proposal, this dichotomy is not very enlightening. For according, to this view, the world of the individual observer (i.e., the subjective) and the world of the "super-reality" (i.e., the objective) are separated by a gap that is--<u>in principle</u>--never to close. The world of the individual observer is, for him, the entire whole; this world contains all there is for him to know and all he ever could know--even under the most ideal conditions. This world, though it may be called subjective, is as <u>objective</u> a world as he will ever know. And indeed he will never know another. Hence his world has all the characteristics usually associated with an "objective" world. And to call this world "subjective" and not "objective" surely seems misguided. [It is true that this world--of the individual observer--is not objective in the usual sense of "being the same" for everybody. Different observers will, in general, live in different worlds. But there is no way of knowing this, since our equations can contain no terms of cross reference between

the different "branches" of the wave function (i.e., between the different worlds). To a given observer, all other observers live in his world; and vice versa. Hence Leibniz's dictum "the best of all possible worlds" takes on a new significance. This is indeed the best of all possible worlds. But why? Because it is the only world (game) available to us (in town).]

Stapp [53] has pointed out that the usual interpretation of QT does not unequivocally rule out this possibility. Rather, it effectively circumvents the question by representing the responses of the measuring devices by specificaations that link the responses of the measuring devices to the experiences of the scientists that use the theory. This means that QT would then be a theory that describes what happens on a particular branch. Then the question of whether there are other branches of some "super-reality" becomes irrelevant.

It seems to me, further, that perhaps we have here an equivocation on the word "actual" or real." These authors promise to reveal to us a new insight regarding what "actually" is or exists. But what we find is that we are presented with an enlargement of our original picture, and that the property of being actual is ascribed not to the same level as it was in the original picture (and this level is still present), but rather to a "higher" level. In truth, all we have done is to shift the level to which "actual" is ascribed, but in such a way, I claim,

that we do great violence to the original meaning of the word. For according to this proposal, what is actual is the "super-reality"; and thus we are left in the position that what is "actual" cannot, <u>in principle</u>, ever be experienced by anyone.

In one sense, what is "actual" is what is actual to a given individual. That is, according to ordinary linguistic usage, we effectively identify actuality with the aspect of things that are relevant to our own personal experience. At the same time, there is a sense of "actual" which seems to refer to that which exists <u>independently</u> of any particular individual, and over and above any particular assessments of it. But if, <u>in principle</u>, it is impossible to know or to experience or to establish the existence of this "super-reality," then it seems grossly inappropriate to ascribe "actuality" to it. (This seems comparable to another situation: when trying to show that we sometimes suffer illusions, and we sometimes experience dreams, and, and that the dreams sometimes seem just as <u>real</u> as "reality," some thinkers have gone to the extreme of then asking if we cannot infallibly distinguish dreams from reality, how do we know that everything is not a dream. This is only superficially plausible, for it can be seen that the word "dream" is now being used in a sense different from what it originally was: "dream" was originally used to distinguish two kinds of states; but now there is only one kind of state, and "dream" has been

mis-appropriated to refer to this state--mis-appropriated because the meaning of "dream" has now changed drastically from what we expected it to mean from its previous usage. That is, if there is now only one kind of state, why use "dream" to describe it? we already have a perfectly good word which has just that meaning: waking-state.)

At any rate, one lesson is certainly clear: we must be very careful in our use of words in these matters; for ordinary linguistic usage is tied to our common sense ideas about the world. And here, it is precisely these ideas that are being called into question.

Besides all the above objections (if I may be facetious), Occam--at the sight of this proposal--must be turning over on his razor. But to be serious again (pity), though this proposal cannot be logically disproven, its import is so intuitively unappealing that, in the words of Stapp [53]: "It is doubtful that any theoretical construct could be secure enough to warrant acceptance at this price." The price is certainly too great for me.

Alternatives Under B

Let us now examine what possible solutions are available to us if we accept the subjectivism on the level of microscopic phenomena, but wish to reject or deny it on the macroscopic level. Before we look at actual alternatives, a few general comments will be helpful.

<u>Removal of subjectivism related to reduction of wave function</u>. According to current terminology the following equation is unfortunately used to represent two radically different states

$$\psi = a\psi_1 + b\psi_2$$

In the first case, this could be the equation for a <u>pure state</u>. Here, we have a given state ψ expressed as an expansion in a set of other states, ψ_1 and ψ_2. With respect to that observable for which ψ_1 and ψ_2 are the eigenstates, the state ψ is in an "indeterminate" state, as we have seen earlier. The system (represented by ψ) is not in the state ψ_1 and it is not in the state ψ_2. However, upon measurement one always obtains (for the value of this observable) either the eigenvalue associated with ψ_1 or the eigenvalue associated with ψ_2. In short, ψ is a superposition--with all the properties we discussed above.

But the same equation could also be used to represent a state called a <u>mixture</u>. The characteristics of this state can be expressed differently depending on the experimental context, but the meaning is the same. (a) To say of a single system that it is in a mixture given by our equation, means that the state of the system, ψ, is definitely <u>either</u> ψ_1 <u>or</u> ψ_2. The system is in one state, and one state only. Our inability to say which is only a matter of ignorance. And all this is true

whether or not a measurement is performed to ascertain the state. If a measurement is performed, the probability of finding the system in ψ_1 is 1 and the probability of finding the system in ψ_2 is 0; or *vice versa*. (b) If we have a single system re-prepared in the same state ψ many times, then we can state the meaning of the mixture by saying that the system is again definitely in *either* state ψ_1 or state ψ_2, and has the corresponding eigenvalue for the value of the observable (again, whether or not a measurement is performed); but now we can say further: after some preparations the system will be in ψ_1 and after some preparations the system will be in ψ_2. This is just an ensemble of many systems in time. Sometimes the state is ψ_1, sometimes ψ_2. (c) There is a third way of thinking of a mixture. If we have many identical systems, identically prepared, and present at the same time, then we have an ensemble of many systems in space. Now, each separate system is definitely either in ψ_1 or ψ_2; a given fraction are in ψ_1 and a given fraction are in ψ_2. The important thing to notice--and this is what is common to all three situations--is that when we have a mixture there is not the same "indeterminate" character of the state, as with a superposition. The state is definite and fixed; but more importantly the values of all observables are also definite and fixed. And most importantly, there are not the "interference effects" in a mixture that we find with a pure state.

Now, according to a widespread interpretation (which dates back to Von Neumann)[55] there are two ways in which the quantum state of a system can change (using the notation of statistical density operators to represent the state):

$$(1) \quad W \rightarrow W' = \sum_n (\phi_n, W\phi_n) P_n$$

$$(2) \quad W \rightarrow W_t = e^{-\frac{i}{\hbar}Ht} W e^{\frac{i}{\hbar}Ht}$$

The second case is just the continuous time evolution as prescribed by the Schroedinger equation. The first case is the discontinuous abrupt change brought about by the performance of a measurement on the system. Clearly process (1) results in a mixture. Thus, if we have a system initially in a superposition and perform a measurement on the system, we will end up with our system in a mixture, viz, W'; the system will be in one of the distinct states ϕ_n. In other words, the measurement "forces" the system from the state in which it is a superposition of the states ϕ_n to one of the particular states ϕ_i. This process has been called the <u>reduction of the wave function</u> (or reduction of the wave packet, referring to the special case of a particle), since the wave (state) function of the system is "reduced" from a superposition of states to a single state.

The reader will notice that this process is of great importance for our inquiry: quite simply, we have found the subjectivism to accompany the superposition, and we found the subjectivism to be absent in the mixture. And so if reduction of the state function always results in the transition of a superposition to a mixture, then perhaps this is just the process by which the subjectivism is removed or eliminated. In short, the answer to the question "exactly how is the subjectivism eliminated?" might be found by examining the reduction of the state function. Then one could claim that the subjectivism is always removed because superpositions (which are the source of the subjectivism to begin with) are invariably reduced to mixtures. And since any event or phenomenon of a macroscopic nature (subject to the indicated restrictions regarding macroscopically distinguishable final states) will have involved interactions (with the microscopic system) amounting to measurements, we could finally claim that though there is the subjectivism on the microscopic level, this subjectivism is systematically excluded from the realm of macroscopic phenomena--and the mechanism of exclusion is just the reduction of the state function.

Hence a lot of effort has been expended by the people in this group, trying to understand the reduction of the wave function, how it comes about; does it really accomplish what we wish, or if there are other ways to

accomplish the same end? Before we look at some of those attempts let us discuss in general terms, the basic idea of a reduction of the state function. For though there does indeed seem to be a connection between the reduction and the removal of subjectivism; and (on the given interpretation of measurement) though the necessity of such a reduction seems to be clear, we will see that the very idea of a reduction is not without its inherent difficulties--some of which appear to be insuperable.

<u>Difficulties Associated with Reduction of Wave Function</u>.--Arthur Komar [35] has shown that the following three assumptions are inconsistent:

(a) The initial state of the apparatus can be represented by means of some well-defined wave function $|II;j\rangle$

(b) When the apparatus interacts via the Schroedinger equation with the system I, assumed to be initially in the state $|I;i\rangle$, it "grinds" or "forces" the system into an eigenstate of the operator N, $|I;n(i,j)\rangle$, where the eigenvalue $n(i,j)$ is some unique function of the arbitrarily chosen initial states of the system and the apparatus, i and j, respectively.

(c) The apparatus should be a measuring device for the quantity N in the sense that if the system I

is initially in an eigenstate of N then, independent of the initial state of the apparatus, the apparatus will leave the state of the system unaltered.

In conformity with assumption (a) Komar assumes that the initial uncorrelated state of the combined system of the object being measured and measuring apparatus can be represented by the state vector

$$|I+II;i,j\rangle \equiv |I;i\rangle \, |II;j\rangle \tag{8}$$

where $|I;i\rangle$ and $|II;j\rangle$ represent the initial states of the system and apparatus respectively. The systems I and II are now brought into interaction and due to this interaction the combined system evolves to a final state

$$|I+II;f\rangle = S|I+II;i,j\rangle \tag{9}$$

where the only property of the Schroedinger equation we require is that it is linear.

If we are considering an apparatus which is to measure the property N of system I it will be convenient to expand $|I+II;f\rangle$ in terms of a complete set of eigenstates $|I;n\rangle$ of the operator N. Thus

$$|I+II;f\rangle = \sum_{n,k} |I+II;n,k\rangle \langle I+II;n,k|S|I+II;i,j\rangle \tag{10}$$

where

$$|I+II;n,k\rangle \equiv |I;n\rangle\ |II;n,k\rangle \tag{11}$$

The states of the apparatus $|II;n,k\rangle$ are denoted by two letters to indicate that there are many states of the apparatus which correspond to a given reading of the instrument.

From assumption (b), which is the hypothesis of the reduction of the wave function, we conclude that there exists a <u>unique</u> n(i,j) such that, for some k

$$\langle I+II;n(i,j),k|S|I+II;i,j\rangle \neq 0 \tag{12}$$

and for all k

$$\langle I+II;n',k|S|I+II;i,j\rangle = 0 \qquad n' \neq n(i,j) \tag{13}$$

Assumption (c), the statement that system II is a measuring device, requires that for each j there is some k such that

$$\langle I+II;n,k|S|I+II;n,j\rangle \neq 0 \tag{14}$$

and that for all j and k

$$\langle I+II;n',k|S|I+II;n,j\rangle = 0 \qquad n' \neq n \tag{15}$$

Let us now consider the particular initial state of system I:

$$|I;i\rangle = \alpha|I;n_1\rangle + \beta|I;n_2\rangle \quad \alpha,\beta \text{ expansion coefficients}$$

where $\alpha\beta(n_2 - n_1) \neq 0$ and $|I;n_1\rangle$ and $|I;n_2\rangle$ are understood to be eigenstates of the operator N which apparatus II is purportedly measuring. From eq. (8) we can conclude that for all n

$$\begin{aligned}\langle I+II;n,k|S|I+II;i,j\rangle &= \alpha\langle I+II;n,k|S|I+II;n_1,j\rangle \\ &\quad + \beta\langle I+II;n,k|S|I+II;n_2,j\rangle\end{aligned} \tag{17}$$

If we set n equal to n_1 in eq. (17) we have as a consequence of eqs. (14) and (15) that for some k

$$\langle I+II;n_1,k|S|I+II;i,j\rangle = \alpha\langle I+II;n_1,k|S|I+II;n_1,j\rangle \neq 0 \tag{18}$$

Thus, from eqs. (12) and (13) we must conclude that for this particular choice of i and j

$$n(i,j) = n_1$$

However, if in eq. (17) we set $n = n_2$, we get, for the same choice of i and j,

$$n(i,j) = n_2$$

We have thus contradicted assumption (b), viz, the uniqueness of n(i,j). Hence we have shown that assumptions (a), (b), and (c) are mutually contradictory.

Assumption (a) is just the position that physical systems--including macroscopic systems--are correctly represented by wave functions. Assumption (c) is just a statement of the essential property a system must have if it is to be adequate for performing measurements in QT. Yet, if we accept (a) and (c) we find we cannot also accept (b). Assumption (b) is just a statement of the intuitive idea that if we could follow in detail the quantum mechanical interaction between the system being measured and the measuring device, we would understand the precise result of the experiment. Alternatively, (b) is just the hypothesis of the reduction of the wave function. We have seen that we cannot accept (b) [assuming we accept (a) and (c)]. Thus it would appear that if reduction of the wave function is understood in terms of assumption (b), then the idea of a reduction of the wave function <u>cannot</u> be fitted into the formal structure of QT. And this conclusion is purely general, dependent in no way on the particular mechanism assumed for the reduction.

Whereas Komar has shown formally that the reduction of the wave function cannot be explained within the framework of conventional QT, Peres and Rosen [46] argue that as a matter of fact it is not even necessary to consider such a reduction. Peres and Rosen follow Von Neumann in allowing that interactions can be constructed such that, if the initial state of the object system S is $\phi = \sum_n c_n \phi_n$, and if the initial state of the instrument is Ψ_o, then the Schroedinger equation for the compound system leads to

$$\sum_n c_n \phi_n \Psi_o \longrightarrow \sum_n c_n \phi_n \Psi_n \qquad (1)$$

where the Ψ_n are orthogonal to each other. The process (1) is called by Von Neumann a "measurement"; but this definition agrees with the usual meaning of the word "measurement" only if the states Ψ_n are macroscopically distinguishable (e.g., different positions of a pointer on a scale). It is the second expression in eq. (1) that has caused all the trouble in this aspect of our interpretation of QT. It is valuable to ask ourselves why this is so. Put simply $\sum_n c_n \phi_n \Psi_n$ is very disturbing. Why is it disturbing? Well, it is not always disturbing, it is only so in case: (1) the expression $\sum_n c_n \phi_n \Psi_n$ represents a superposition; and (2) the Ψ_n represent macroscopic states, which furthermore, are macroscopically distinguishable. And this is disturbing because now we apparently have a state in which, for instance, our pointer occupies no one of the positions on the scale corresponding to one of the states Ψ_n. Rather the pointer is in a superposition of the Ψ_n. This is counter-intuitive, paradoxical, and surely disturbing.

Before we consider ways out of the cul-de-sac, let us, for just a moment, remove the restriction of the instrument as being a macroscopic device. Let the instrument be microscopic. Now let us ask what is it about QT that inclines us to think of $\sum_n c_n \phi_n \Psi_n$ as a superposition and not as a mixture? (We again assume that the initial state of the system is a superposition.) The answer is the Schroedinger equation and Eugene Wigner. In the proof referred to earlier, Wigner has shown that the time-evolved state from a pure state must also be a pure state and not a mixture. Hence if we begin with a superposition and the process is describable by the Schroedinger equation, then we must end up with a pure state also. Therefore our expectation that $\sum_n c_n \phi_n \Psi_n$ is a superposition has nothing to do with the macroscopic nature of the instrument. Let us then, again confine our attention to those cases in which the instrument <u>is</u> macroscopic. We will also assume that condition (2) above (i.e., the conditions for being disturbing to our sensibilities) is satisfied, viz, that the Ψ_n are macroscopically distinguishable. It would thus appear that the only path remaining open to us is to deny condition (1), i.e., to deny that $\sum_n c_n \phi_n \Psi_n$ is in fact a superposition. This is what Peres and Rosen do.

Peres and Rosen argue that the formalism of QT cannot speak for itself; it must be interpreted. An uninterpreted calculus is of no value to us, we must have rules of correspondence also. The appearance of $\sum_n c_n \phi_n \Psi_n$ does not, <u>by itself</u>, tell us whether it represents a superposition or a mixture. Something else, besides the pure formalism, must be brought to bear. Peres and Rosen argue as follows:

> . . . in orthodox QT we are compelled to interpret $\phi = \Sigma c_n \phi_n$ as a superposition of states, and not as a mixture, because interference effects between the various components ϕ_n can be demonstrated experimentally. We now intend to show that if we are dealing with macroscopically distinguishable states Ψ_n of a particle of sufficiently large mass, it is experimentally impossible to get them to interfere with one another. It follows that, in this case, we are not compelled to interpret the right-hand side of (1) $[\Sigma c_n \phi_n \Psi_n]$ as a superposition and can therefore interpret it as a mixture while maintaining agreement with experiment.

Peres and Rosen consider a mental experiment in which they have a macroscopic body of size a, density ρ and mass $m \sim \rho a^3$, passing through a screen in which there are two slits a distance b apart ($b > a$) and impinging on another screen, a distance L away. If this experiment is repeated many times interference fringes will be expected on the second screen, a distance $d \approx \frac{\lambda L}{b}$ apart, where $\lambda = \frac{h}{p}$ is the de Broglie wavelength. Thus $L \approx \frac{bd}{\lambda} > \frac{\rho a d}{h}$ and since $p = \frac{mv}{(1 - \frac{v^2}{c^2})^{1/2}} \geq mv$ it follows that the duration of each experiment is $T = \frac{L}{v} > \frac{mad}{h} \sim \frac{\rho a^4 d}{h}$. Now we assume that $T < 10^8$ years (estimated total lifetime of the universe). Also, in order to have observable fringes, d cannot be smaller than 10^{-8} cm, the interatomic distance in solid bodies. We then obtain, with $\rho \sim 1 \frac{gm}{cm^3}$ (this is a universal constant: a few nucleons per cubic Bohr orbit),

$$a < 1 \text{ cm} \quad , \quad m < 1 \text{ gm} \tag{2}$$

Macroscopic objects which do not satisfy eq. (2) cannot display interference effects in our experiment. Or alternatively, for any bodies larger or heavier than this, the time required for the experiment would be greater than the total lifetime of the universe!

However, to complete the proof if must be shown that the same holds for a macroscopic system coupled to a microscopic one. To do this Peres and Rosen show that the process represented by eq. (1), which was obtained from the Schroedinger equation, is <u>irreversible</u>. They argue that in order to reverse the arrow in eq. (1) in such a way that the left-hand side will again be interpretable as a superposition, we need a mechanism which brings back the macroscopic body from its possible final positions <u>with the correct phase</u>. This implies, in the WKB approximation, that we need an accurate control of the phase factor $\exp(i\int pdq/\hbar)$ for the whole process. This means that roughly we must have $p\delta q < \hbar$ where p is the mean momentum of the macroscopic body and δq is the uncertainty in its <u>total</u> path q (i.e., the limit of reproducibility of the experimental setup).

Again the total duration of the experiment will be $T \sim \frac{mq}{p} > \frac{mq\ \delta q}{\hbar}$. For a macroscopic setup we cannot achieve anything better than $\delta q \sim 10^{-8}$ cm (the interatomic distance) and we obtain, with, e.g., $m = 1$ gm and $q = 1$ cm, that the experiment must again last longer than the total lifetime of the universe.

I would like to note, in passing, that logically there was one more alternative open to Peres and Rosen: instead of denying either: (1) that the state is a superposition, or (2) that the Ψ_n are macroscopically distinguishable, Peres and Rosen might simply reply that there is nothing strange or disturbing at all about the initial analysis of the situation. That is, instead of denying either half of the paradox, they could simply say that the conclusion of the supposed paradox actually is not paradoxical at all. Logically speaking, this is just as much a viable alternative as either of the others.

It is not surprising that this alternative (of simply saying there is nothing paradoxical about having a superposition of macroscopically distinguishable states) has not been one of the most frequently chosen. For the very thing that makes this situation disturbing (to most) is also what makes it exceedingly difficult to understand in what conceivable way this situation could be construed such that it is no longer disturbing. I would think that the proposal of Everett, _et al_. considered above would qualify as alternatives of this kind. This kind of proposal seems akin to the "resolution" given by Carl Hempel to the paradoxes of confirmation in which he does not "disprove" either half of the paradox; rather he "shows" why the "paradox" should actually _not_ sound paradoxical at all. Of the two, the "resolution" of Hempel seems much the more reasonable to me.

Particular Alternatives.--Let us recall that all the solutions proposed in this section are such that: (1) we accept the subjectivism on the microscopic level, and (2) we reject the subjectivism on the macroscopic level. The adherents of this position claim that though the subjectivism is undeniably present in the microscopic realm, it "somehow" gets eliminated as we "move up to" the macroscopic level. To help us distinguish the possible alternatives (and the proposals actually made), let us separate the solutions according to what extent the subjectivism can be restricted to just the original object system, and to what extent the (physical) "surroundings" are also involved with this subjectivism. A crude way of stating this is to say we will separate the solutions according to where the reduction of the wave function takes place (i.e., how far from the original system does the reduction take place).

1. Indivisibility of the quantum. For all the following proposals, we will always assume the situation of interest is that of an atomic system (object) initially in a state of superposition, and then interacting with another system (apparatus) for the sake of making a measurement. And we recall that we associate the subjectivism with the presence of the superposition. Now, this first proposal claims that the subjectivism gets no farther than the first interaction, i.e., that the superposition is reduced right at the first interaction. The

argument here is that in any interaction whatever--whether or not it is a measurement--there is always the exchange of a single quantum between the two systems, e.g., a quantum of energy, a quantum of angular momentum, etc. And this proposal accepts as a basic postulate the indivisibility of the quantum, where that is here interpreted to mean that a given quantum is always associated with a given value for the quantity of the property exchanged. A quantum cannot be associated with several different values for the observable. Hence, a quantum is "whole" and cannot be "divided" among different values.

Now consider the case when one of the interacting systems is in a superposition. This is a state which has associated with it several different values of the observable in question. (It is the desire to measure this given observable that determines "what kind of" quantum is exchanged.) But the exchanged quantum cannot likewise be associated with several different values. Hence, upon interaction, the exchanged quantum is associated with <u>only one</u> of the states constituting the superposition. On different repetitions of the interaction, the quantum exchanged is associated with the value of the observable corresponding to the different constituent states of the superposition, with the respective probabilities of the different values being given according to the usual rules. A concrete example: we have an electron in a superposition of spin up and spin down states. Now the quantum exchanged in any subsequent interaction with the electron cannot be partially a quantum of angular momentum "up," and partially a quantum of angular momentum "down." Hence, the actual exchanged quantum is a quantum associated either with spin up or spin down.

It is seen that the "indivisibility of the quantum" makes it impossible to retain the "indeterminate-ness" of the initial state. It forces a decision to be made between the constituent states of the superposition. Thus, as regards any "physics" of the situation after this first interaction, we are dealing with a mixture and no longer with the initial superposition.

This account claims to accomplish two things: (a) it allows us to accept the subjectivism on the micro level, while at the same time allowing us to prevent its occurrence at the macro level, and (b) it shows us exactly what mechanism accounts for this prevention, viz, the indivisibility of the quantum.

As far as I know, the only person who has ever maintained a position of this sort is Niels Bohr [9,10]. Bohr's writings (on this and other aspects of the problem) are very incisive and deserve, I feel, more attention than they currently receive. But I also think there are certain difficulties and weaknesses with Bohr's solution. Bohr's proposal has an initial attraction because we seem to have learned something very basic and profound about the way nature, on its elementary levels, can and cannot interact with itself. But by the same token, to say this much and no more really leaves the question unanswered. For if we can find nothing more to say about the "indivisibility of the quantum" besides the fact that it is there, then we have merely replaced one question with another--without any substantial gain in the process. It might be replied that if we can solve the subjectivism/objectivism problem by appeal to the problem of the exchange of the quantum (which we would have regardless), then we have made progress: we have reduced two problems to one. But this does not seem to be the case. For the problem of the exchange of the quantum is just the problem of how systems interact. And it is precisely here, in the nature of the interaction between systems, that we seek the solution to the subjectivism/objectivism problem. Hence, we have not solved one problem by reducing it to another; we have merely replaced one formulation of the problem with another formulation of the same problem.

However, the most serious objection to this proposal seems to be the following: if it is correct then it is never possible in an interaction to preserve a superposition; the coherence among the component states of the superposition is invariably destroyed. This would mean--for either the second system in the interaction or for the combined system--that the subsequent physical behavior would be the same whether the first system was a superposition or a mixture. And since we can only obtain information about a system from its effects on other systems --through interactions--this proposal would apparently make it impossible for us to ever detect the difference between a superposition and a mixture. And this is patently untrue.

In particular, this proposal seems to rule out any interactions of the type

$$\Psi_o \sum_n c_n \phi_n = \sum_n c_n \phi_n \Psi_o \longrightarrow \sum_n c_n \phi_n \Psi_n$$

This interaction is just a simple consequence of the Schroedinger equation. Bohr (or a supporter) might reply that it is alright that this proposal does conflict with the Schroedinger equation in this way. For we have known ever since Von Neumann that there are interactions which do not obey the Schroedinger equation, viz, measurements. So the conflict between our proposal and the Schroedinger equation is neither new nor surprising, they might say. But the difficulty with this reply is that the "indivisibility" proposal is not restricted to just measurements; it applies to <u>all</u> interactions. And unless we are given some criteria for distinguishing those interactions which are measurements, our criticism of the proposal must remain.

2. Jauch [30] claims that the measurement problem is a pseudo-problem. He accepts the subjectivism on the micro level. As to whether the subjectivism is also present at the macro level, his answer is a unique one. But he claims to have shown the problem to be a pseudo-problem because he can, with just a straightforward application of his formalism, reconcile, on the one hand, the invariable appearance of a mixture upon measurement, with the theoretical prediction (according to the equations of motion) of a pure state, on the other hand.

Jauch considers the traditional view that the state of a system can change in two radically different ways:

$$W \rightarrow W' = \sum_n (\phi_n, W\phi_n) P_n \tag{1}$$

$$W \rightarrow W_t = U(t)\ W\ U^{\dagger}(t) = e^{-\frac{i}{\hbar}Ht}\ We^{\frac{i}{\hbar}Ht} \tag{2}$$

where W represents a density operator for the state in question; P_n is just a projector onto the state ϕ_n. The first process represents the change that comes about as a result of a measurement on the system, always resulting in a mixture. The second process is the change wrought simply by the unmolested time evolution of the system; this change obviously satisfying the Schroeding equation.

Jauch wishes to consider the observation of a quantum mechanical system S by a measuring device M = m + A. The measuring device can be decomposed, for the purpose of

analysis, into a microscopic part m in which all the specifically quantum mechanical effects take place, and into a second part A, which merely serves to provide amplification of the quantal effects in m to the level of macroscopic events so that they might be observed by an experimenter. Then the problem of measurement reduces to the following: if we apply the equations of motion (i.e., eq. 2) to the combined system of object (S) and measuring device, we expect a pure state; but if a measurement has truly been performed we expect (from eq. 1) a mixture. How can these both be?

Jauch says there are two cases to consider: (1) we apply the equations of motion to the combined system of S + m; (2) the combined system is S + M. To understand the treatment of the union of two quantum mechanical systems Jauch appeals to work done essentially by Von Neumann [55]. Let the density operator for the object system S be W^{I}; for the measuring device W^{II}. Each of these operators is given in the respective Hilbert space of the two systems. Finally, let W be the density operator of the combined system; this operator is given in the space which is the tensor product space of the two component spaces.

Jauch shows that if both W^{I} and W^{II} represent pure states, then W will also represent a pure state. But the reverse question is, of course, the one of interest, viz,

if W represents a pure state, what can we say about W^I and W^{II}? Jauch shows that the necessary and sufficient condition for W^I and W^{II} to be pure states, given that W is a pure state, is that $\Phi = \phi \otimes \psi$, where Φ is the state vector in the combined space, and ϕ and ψ are the state vectors respectively in the component spaces I and II. In general, if W is pure, W^I and W^{II} will be mixtures.

Let us consider first the combined system S + m and thus apply the theory to the object system S and the <u>microscopic</u> part m of the apparatus. Let $\Phi_{\pm o} \equiv \phi_{\pm} \otimes \psi_o$ where we have assumed system I to have a two-dimensional state vector space; also ϕ_+ and ϕ_- are two orthogonal vectors in this space and are eigenstates of the quantity to be measured. If the states above represent pure initial state vectors before I and II interact, then the joint system is also in a pure state after the interaction and given by

$$\Phi_{\pm} = U\Phi_{\pm o} = \phi_{\pm} \otimes \psi_{\pm}$$

The state vectors of system II define a three-dimensional space. It contains a state ψ_o which describes a "state of readiness" of the measuring device and two more states, denoted by ψ_+ and ψ_-. Here ψ_+ is the final state of system II after it has measured system I in the initial state ϕ_+; likewise ψ_- is associated with state ϕ_-.

We now consider the general initial state of the form

$$\Phi_o = \alpha_+\Phi_{+o} + \alpha_-\Phi_{-o} \tag{14}$$

where $\alpha_\pm$ are two arbitrary constants subject to the normalization condition $|\alpha_+|^2 + |\alpha_-|^2 = 1$. Since U is a linear operator, we must have for the final state after the measurement in this case

$$\Phi = U\Phi_o = \alpha_+\phi_+ \otimes \psi_+ + \alpha_-\phi_- \otimes \psi_-$$

This is the state of system I+II after the measurement. It is pure, as it must be since the initial state was pure.

But Jauch now asks why does this state furnish us with a measurement of the alternatives ϕ_+,ϕ_-? The reason is that, when reduced to the measuring system II, the state is a mixture, therefore each individual system realizes one of the events of the mixture and these events can, by virtue of the amplifying device A, be made data for an observer. That is, even though the combined system is in a pure state (W is pure), we do not "consult" the combined system (for our information); rather we consult one of the component systems, m (through the intervention of A, of course), and this component system is in a mixture. Thus, it is the mixture that is presented to us. Hence, the relevant part of the state Φ for us is its

reduction to the system II. Jauch has rules for obtaining the reduced density operator, and he obtains

$$W^{I} = |\alpha_{+}|^{2} P_{+} + |\alpha_{-}|^{2} P_{-} \tag{15}$$

$$W^{II} = |\alpha_{-}|^{2} Q_{+} + |\alpha_{-}|^{2} Q_{-} \tag{16}$$

where he introduces the projection operators $P_{\pm}, Q_{\pm}$ with one-dimensional ranges containing $\phi_{\pm}, \psi_{\pm}$ respectively.

Thus Jauch concludes: "This result shows clearly the reason for the occurrence of process (1) under the influence of a measurement. It is here merely a mathematical consequence of the reduction of a pure state (14) to one of its component subspaces." Hence, Jauch seems to have reconciled the prediction of a pure state with the appearance of a mixture--and all this without appeal to any specific "measurement" process; everything here is obedient to the Schroedinger equation.

But suppose we apply the theory to the combined system S + M. Now, we have combined the measuring device M with the system S to form a larger (classical), system S + M which is now simultaneously a system and a measuring device. In this case there is no question of reducing the state (14) to one of the component systems. There is therefore no possibility of changing the state into a statistical mixture in this manner. Yet, we believe that considered as a measuring device, S + M will record a mixture.

Jauch introduces the projection operators $\Pi_+ = P_+ \otimes Q_+$ and $\Pi_- = P_- \otimes Q_-$. QT, in accordance with process (2), tells us that the final state of S + M considered as the system is the pure state (14). However, process (1) applied to S + M considered as the measuring device tells us that this final state should be $W = |\alpha_+|^2\Pi_+ + |\alpha_-|^2\Pi_-$. Now what do we make of these two separate states? Jauch claims that these two states are in the <u>same equivalence class</u>. By that Jauch means that two states W_1 and W_2 are equivalent with respect to a given set of observables Γ if

$$\mathrm{Tr}AW_1 = \mathrm{Tr}AW_2 \qquad \text{for all } A\varepsilon\Gamma$$

Thus physically, this means that the two states W_1 and W_2 cannot be distinguished by any measurement whatsoever with observables from the set Γ.

Jauch's claim is thus to the effect that the pure state (14) and the mixture (17) are equivalent states, and thus cannot be distinguished by any measurement from the same set of observables that contain the projectors Π_+ and Π_-. Hence in case we have S + M as the combined system there never is a reduction of any kind--not of the combined state vector (which is pure), nor of the density operator to a component subspace. But one might now be inclined to ask: granted that the two states "look alike" as far as measurements are concerned, which state do we <u>really</u> have present? If Jauch wishes to preserve his solution of the first case (i.e., S + m) and again avoid appeal to process (1) then he must surely opt for state (14). And this he does: Jauch writes:

> A final question: does this result mean that the two states (14) and (17) can under all circumstances, never be distinguished by a measurement? It does not mean this. It means, this can never be accomplished with measurement from the Abelian set Γ which contains the two projections Π_+ and Π_-. In order to distinguish them it is necessary to have at one's disposal an observable which is not in this set. An observation of such a quantity will no doubt reveal that it *is indeed (14) which is the final state after the interaction*, in agreement with the Schroedinger equation. [Italics added.]

It is crucial, in Jauch's argument, that we take into account the *classical* nature of the measuring device S + M. For this classical nature assures us that the set of observables of the system forms an Abelian set. And it is this which enables us to place the states of the system into equivalence classes. Thus, a classical (i.e., macroscopic) device is definitely necessary for this analysis. It would thus appear that Jauch has shown that even granting the subjectivism on the micro level, the special characteristics of systems on the macro level make it impossible for the subjectivism to be present there--and this was shown by a straightforward application of Jauch's formalism. But this conclusion would be mistaken. First, it will be noticed that we can always perform the quantum analysis on the system S + m. In that case, the "measurement," the "reduction," the elimination of the subjectivism takes place purely with reference to m, *which is by* definition the microscopic part of the apparatus. The amplification by A, bringing to bear the macroscopic part of the apparatus, might not take place for some time after the original interaction between S and m (as has been pointed out by Jauch himself). Hence, the elimination of the subjectivism has occurred much earlier than the macroscopic part of the apparatus ever enters the picture.

Furthermore, Jauch himself says that the true state of the system (when we use S + M) is the pure state (14). This certainly means that Jauch would expect--even on the macroscopic level--the manifestation of a pure state, and hence, the presence of the subjectivism if we could find a suitable observable. It is Jauch's position that there is indeed subjectivism on the macro level, but as a matter of fact, we cannot detect it as long as we measure observables in the same set with the projections Π_+ and Π_-. Unfortunately, Jauch gives us no hint at all of what kind of observable is required to break the equivalence between the two states (14) and (17). In other words, we actually do have subjectivism on the macro level, but for all practical purposes, it never shows its face.

Jauch has given us an account which completely avoids process (1) and obeys the Schroedinger equation. It would thus seem that the measurement problem has been solved. But I do not think this is the case. For instance, the reader will notice that (with system S + m) after the interaction not only is m in a mixture, but so is S (and we recall that S was initially in a pure state). So looking at Jauch's analysis physically we have a system S in a pure state, then after interaction with m, it is in a mixture. It is true that this can be accounted for with Jauch's formalism, but actually all that Jauch has given us is a new mathematical formalism to account for the same physical process as was accounted for under the usual interpretation of QT, viz, upon interaction a system in a pure state is "forced" into a mixture. There is indeed value in the new formalism, but it has added no more to our physical understanding than we already had. In particular, this formalism has contributed one thing of great importance. It shows that the reduction from the pure state to the mixture (thought of in the usual sense) is not dependent upon interaction with a macroscopic system. This reduction occurs even when the second system is microscopic. The reader will notice the similarity here to the previous proposal of the "indivisibility of the quantum." And to the extent that Jauch's proposal is similar, it is subject to the same comments as above.

Another attempt at accounting for the removal of subjectivism by appeal to the specifically macroscopic nature of the apparatus is that due to D. I. Blokhintsev [5]. Blokhintsev wishes to measure the linear momentum of a micro particle by letting it collide with a sphere of macroscopic proportions. The sphere is resting at the top of an inverted potential well as shown below.

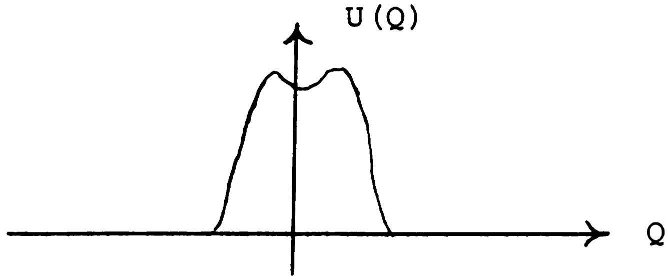

Figure 1

The well is narrow enough so that a slight "push" in either direction will tumble the sphere from the well. The well is high enough so that we get an irreversible reaction, i.e., once the sphere has fallen from the top of the well, it can never get back to the top.

Blokhintsev's procedure is then the following: he writes a wave function for the particle as a superposition of plane waves of plus and minus momenta. He writes a wave function for the sphere as a Gaussian wave distribution centered about the potential. He then writes a combined wave function for the compound system. Blokhintsev then assumes a simple coupling between the two systems representing the energy of interaction. He then writes a Hamiltonian for the total system, puts this into the time-dependent Schroedinger equation and seeks a solution for the wave function of the compound system after the interaction. To find the wave functions Blokhintsev uses first-order perturbation theory. Once he obtains these wave functions he uses them to form the density matrix for the compound system. This matrix has numerous terms; in particular, most of them are cross-terms representing interference states.

But at this point Blokhintsev takes the crucial step for his solution: he takes the asymptotic solution of the density matrix at $t \to \infty$ and when the sphere is far away from the center of the potential well. With this, all terms drop out of the expression for the density matrix but two: that term which gives a probability of 1 that the sphere will be found to the left of the well (when the coordinate of the sphere $Q \to -\infty$); and that term which gives a probability of 1 that the sphere will be found to the right of the well (when the coordinate of the sphere $Q \to +\infty$). Hence by a simple application of the laws of QT, Blokhintsev has shown how we can go from an initial state of superposition (for the combined system) to a mixture. And this was done by invoking the macroscopic nature of the measuring instrument. For it is the macroscopic nature of this particular apparatus that results in the irreversible reaction, and thus justifies the use of the asymptotic solutions.

What Blokhintsev has done I believe to be valuable, but its chief weakness I feel, is its lack of generality. It is certainly a "contrived" situation we have with the sphere perched precariously on top of the potential well. But it is true, of course, that we do wish our measuring devices to be in metastable states before a measurement. However, this example seems quite limited in its applicability and it seems to have none of the "general" characteristics of more general kinds of measurement situations. Furthermore, this approach is subject to the same objections as are all those which place the "reduction" in the macroscopic behavior of the measuring device. That is, the macroscopic part of the apparatus might not be utilized for some time after the original interaction between the object system and the apparatus; and we do not expect that the pure state persists throughout this indefinite time. For instance, we certainly feel that a photographic plate will perform a measurement--and hence a reduction--of the result of a single photon through a double slit experiment. Yet, the arrival of the photon could very likely be recorded by a single silver halide complex. And it might be days before the film is developed. But surely the measurement has been performed as soon as the photon strikes the plate; and we believe that the superposition does not persist for the weeks or months until the (macroscopic) process of developing occurs.

3. In the solution of Daneri, Loinger, and Prosperi [16,17], the measuring apparatus is schematized

as a macroscopic system which possesses, besides the energy, at least one other macroscopic constant of the motion. The value of this constant characterizes an invariant manifold ("channel"). In each manifold certain ergodicity conditions hold and there exists an equilibrium macro-state towards which the system evolves spontaneously. The apparatus is assumed to be initially in the equilibrium state belonging to a given channel and the interaction with the observed system determines a transition of the apparatus towards a state belonging to another channel, which depends on the initial state of the observed system. Then the apparatus evolves towards a new equilibrium state.

Following the notation of Daneri, Loinger, and Prosperi and using statistical operators, we have:

case 1: $W_o = P^{I}_{[\phi_k]} \; P^{II}_{[\Phi_o]}$ before measurement interaction

$W' = P_{[\phi_k \Phi_k]}$ right after interaction

$W_t = U(t) \; P_{[\phi_k \Phi_k]} \; U^{+}(t)$ after long time

case 2: $W_o = P^{I}_{[\phi_k]} \; P^{II}_{[\Phi_o]}$ before measurement interaction

$W' = P^{I}_{[\phi_k \Phi_k]}$ right after interaction

$W_t = P^{I}_{[U(t)\phi_k]} \; P^{II}_{C_{ke_k}} \; N_k$ after long time case 3: $W_o = P^{I}_{[\sum_r C_r \phi_r]} \; P^{II}_{[\Phi_o]}$ before measurement interaction

$W' = P_{[\sum_r C_r \phi_r \Phi_r]}$ right after interaction

$W_t = \sum_r |C_r|^2 \, P^{I}_{[U(t)\phi_r]} \; P^{II}_{C_{re_r}} \; N_r$ after long time

W represents a statistical operator. $P_{[\ ]}$ denotes the projection operator onto the manifold [ ], i.e., $P_{[\psi]}$ denotes the statistical operator P representing a pure ensemble of systems each in the same quantum mechanical state represented by the Hilbert space vector ψ. N_k is just a normalization factor. U(t) is the time evolution operator $\exp(\frac{-i}{\hbar}Ht)$.

The three cases are distinguished as follows: in case 1 (initially) the object (system I) is in the state ϕ_k; the apparatus (system II) is in the state Φ_o. Each system is represented by a Hilbert space vector in its respective space. Right after the interaction the composite system is represented by a vector in the product space. After a long time the interaction has ceased, the two systems evolve independently, but yet remain correlated such that we can still represent the state of the combined system by a projection in the composite (tensor product) space of the two systems. In this case system II is considered to be a non-macroscopic system.

In case 2 everything is the same except now system II is a macroscopic device. Now, according to Daneri, Loinger, and Prosperi, the time evolution of the macroscopic apparatus is known: this is just the ergodic behavior of the macroscopic apparatus. Thus a sufficiently long time after the interaction, the two systems will have evolved independently, and the apparatus will be in the equilibrium cell of channel k (where this channel is determined by the initial state of the object).

In case 3, the apparatus is again macroscopic, but now the object is in a superposition of different states ϕ_r. Right after the interaction the projection is onto a manifold determined by a superposition of vectors. After a long time, the apparatus is in a fixed equilibrium state; the object is in the corresponding time-evolved state; and all this with statistical weight $|c_r|^2$. System I is, in all three cases, microscopic.

Daneri, Loinger, and Prosperi explicitly write down only the equations for case 3. The equations for case 2 are implicit in their writings. They do not consider case 1 at all. It is valuable to consider case 1 because it highlights the fact that for Daneri, Loinger, and Prosperi, "measurement" is a macroscopic phenomenon; and if we restrict system II to being a microscopic system, "measurement" will never result. The reason for this is clear: it is only in virtue of the fact that we have

macroscopic devices that we can appeal to the ergodic evolution for the removal of the interference terms present in W' of case 3. The ergodic development of system II is dependent upon the fact that it is macroscopic. Hence, a microscopic device would not be subject to the ergodic development, and thus would not result in the destruction of the interference as required.

Unfortunately, the program of Daneri, Loinger, and Prosperi does not accomplish all that it claims. Note, in case 3, that the transition from W' to W_t is not given by the continuous time evolution equation for density (statistical) operators, viz,

$$W_t = U(t)\ W_o\ U^+(t)$$

Rather, W_t is simply written down to correspond to what "we know" we will obtain upon a measurement. This in itself is not surprising, for the recognition of two distinct modes of change of state (as represented by the density operator) goes back to Von Neumann in 1935. But what is objectionable--or rather, regrettable--about Daneri, Loinger, and Prosperi, is that they give us no more understanding of the connection between the two than did Von Neumann. It is precisely the transition from W' to W_t that we wish to understand. And this is precisely what Daneri, Loinger, and Prosperi cannot tell us. As pointed out by Bub [12], the ergodic theory of Daneri,

Loinger, and Prosperi gives us the development of the macroscopic apparatus only in that case when the apparatus is initially in a given channel. Their theory has no mechanisms by which we can understand the development of the apparatus if it initially is in a superposition of different channels. It is only after the apparatus can be assigned to a single particular channel that the ergodic theory is applicable. But at this point the troublesome aspect of the whole measurement problem has been "passed." How to account for the fact that we go from a superposition of states to one particular state is precisely the measurement problem. Daneri, Loinger, and Prosperi have shed no new light on this question.

Why is the Daneri, Loinger, and Prosperi theory unsuccessful in this? Daneri, Loinger, and Prosperi have shown that

$$M\left\{\sum_{i=1}^{s_{k\nu}} |(\Omega_{k\nu i}, U(t)\Omega_{k\mu j})|^2\right\} \approx \frac{s_{k\nu}}{s_k} \approx \delta_{\nu e_k}$$

$$\left|M\left\{\sum_{i=1}^{s_{k\nu}} (\Omega_{k\nu i}, U(t)\Omega_{k\mu j})^*(\Omega_{k\nu i}, U(t)\Omega_{k\mu' j})\right\}\right| \ll$$

$$\frac{s_{k\nu}}{s_k} \ll \delta_{\nu e_k} \approx 0 \qquad (\mu \neq \mu')$$

Here, M is just the operation of time averaging, $\Omega_{k\nu i}$ are the eigenvectors characteristic of a given channel and given cells, s_k is the number of dimensions of the kth channel and $s_{k\nu}$ the number of dimensions of the νth cell. Thus they can argue that if the initial state of the apparatus is given by

$$\Psi_k(o) = \sum_{\mu j} W_{k\mu j}\Omega_{k\mu j} \quad \text{where} \sum_{\mu j} |W_{k\mu j}|^2 = 1$$

then the norm of the projection of $\Psi_k(t) = U(t)\Psi_k(o)$ onto the cell $C_{k\nu}$ (time-averaged) is just $\approx \delta_{\nu e_k}$. That is, after sufficiently long time state vector of apparatus is (approximately) <u>certainly</u> in C_{ke_k}.

On the other hand, if the initial state of the apparatus is a superposition from different channels, we have

$$\Psi(o) = \sum_{k\mu j} W_{k\mu j}\Omega_{k\mu j} \qquad \sum_{k\mu j} |W_{k\mu j}|^2 = 1$$

Now, the norm of the projection of $\Psi(t) = U(t)\Psi(o)$ onto cell $C_{k'\nu}$ (time-averaged) is $\approx p_{k'}\delta_{\nu e_{k'}}$, where $p_{k'} = \sum_{\mu j} |W_{k'\mu j}|^2$. Note that $p_{k'} \neq 1$, but $\Sigma_{k'}\, p_{k'} = 1$. Following the notation of Daneri, Loinger, and Prosperi, we let $u_{k\nu}(t) = \sum_{i=1}^{s_{k\nu}} |(\Omega_{k\nu i}\ , u(t)\Psi_o)|^2$, then we have in the first instance and the second instance respectively

$$M\, u_{k\nu}(t) \approx \delta_{\nu e_k} \tag{9.8}$$

$$M\, u_{k\nu}(t) \approx p_k \delta_{\nu e_k} \tag{9.9}$$

The difficulty facing Daneri, Loinger, and Prosperi is a satisfactory interpretation of eq. (9.9). Daneri, Loinger, and Prosperi interpret $u_{k\nu}(t)$ as "the probability of finding the system in the macrostate $C_{k\nu}$ at the time t." This appears uncontroversial. Indeed it seems to be just the Born interpretation of probabilities. But this is not the case! The Born interpretation identifies the norm of a projection of vector

$$\text{norm}\left(P_{C_{k\nu}}\Psi_k(t)\right) = \sum_{i=1}^{s_{k\nu}} \left|\left(\Omega_{k\nu i}, \Psi_k(t)\right)\right|^2$$ where $P_{C_{k\nu}}$ is a projection operator onto the manifold represented by $C_{k\nu}$

with the "probability of being found in that state" <u>only on the condition that a measurement is performed, i.e., an interaction</u>. And this condition <u>cannot</u> be satisfied in the Daneri, Loinger, and Prosperi model. For the probability in question is supposed to be the probability of the apparatus being found in a given macrostate. But during this time no interaction of any kind takes place. The interaction with the object has long since ceased, and the apparatus is evolving as a free, closed system. This interpretation of Daneri, Loinger, and Prosperi

results from a licentious use of the Born interpretation. In addition, in their interpretation of eq. (9.9) Daneri, Loinger, and Prosperi say the equation shows that the apparatus is in the macrostate C_k with probability p_k. This also is mistaken. What Daneri, Loinger, and Prosperi showed was that the vector $\Psi(t)$ has non-zero projections (viz, p_k) on a given set of manifolds. But it does not follow that the vector $\Psi(t)$ then has a probability p_k to be in the kth manifold.

These objections have the effect of vitiating Daneri, Loinger, and Prosperi's interpretation of eq. (9.9). Thus Daneri, Loinger, and Prosperi are left without a satisfactory account of the transition from the superposition to one of the states of the superposition. Hence their account is of no value in accounting for the removal of the subjectivism which accompanies the superposition. When the apparatus is in a given channel, their account is both valuable and correct. But when the apparatus is initially in a superposition of states from different channels, then the theory of Daneri, Loinger, and Prosperi does not help us to understand the evolution of the system to one of the constituent states (of the superposition). And hence the theory of Daneri, Loinger, and Prosperi is of no help in solving the problem of measurement, nor is it of any help in understanding the "removal" of subjectivism by appeal to a macroscopic interaction.

The following diagram should help the reader to visualize the objection to the theory of Daneri, Loinger, and Prosperi.

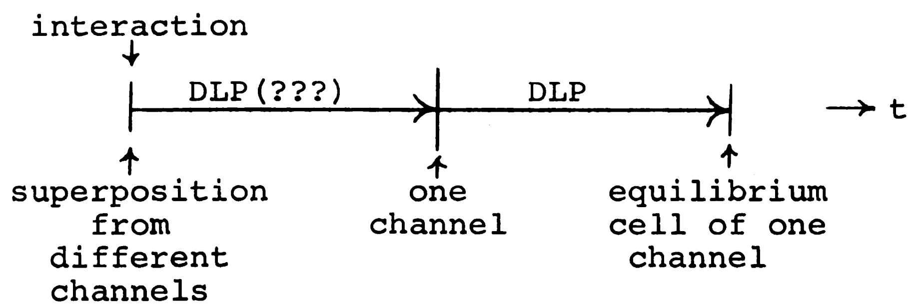

Figure 2

The theory of Daneri, Loinger, and Prosperi is clearly applicable after the apparatus is in one channel. But the theory is not applicable before that point. Hence Daneri, Loinger, and Prosperi can give no account of "how" the apparatus gets to be in one channel.

(I might note in passing that it is clear that Daneri, Loinger, and Prosperi renounce the validity of the Schroedinger equation in their proposal. For the objection can be raised that if we start with a pure state, eq. (9.9) and if its evolution is governed by the Schroedinger equation, then Wigner's proof shows that we must end up with a pure state, and not with a mixture as do Daneri, Loinger, and Prosperi. Daneri, Loinger, and Prosperi have attempted to answer this objection, but, it seems to me, in a very unsatisfactory manner.)

PART IV

CONCLUSION

I think it would be very valuable, as I begin this concluding section, to state exactly what I wish to affirm and what I wish to deny. This investigation has certainly been a study of "physical reality." But I do not wish here to deny or even question the *reality* of physical nature. Rather, what I wish to question in this study are certain *aspects* of physical nature. In particular, I wish to question traditional beliefs about the *independence* of the existence of individual physical systems; I wish to ask if we can think of individual physical systems as having an *objective* existence, in the sense that they exist and have their properties independently of other physical systems. But, it is just these *aspects* or *features* of physical nature that I wish to question, not the *reality* of physical nature. Let me indicate two considerations that show not only that we *can*, but also that we *must*, accept the micro cosmos as *real*.

First, a consideration directly from the principle of superposition. We have seen that it is the existence of superpositions that leads us to question the independence of individual physical systems. But the limitations placed upon physical nature by QT do not go so far as to deny reality to the phenomena under question. I mentioned, during the discussion of relativistic physics, that though relativity theory eliminated some of the marks of independence of a physical system, it by no means removed them all: the relativization was a partial one, not a complete one. I also stated that this is likewise the case in QT: in QT we have a further relativization (of the independence of physical systems), but again it is a partial one.

In QT we find that for a system which is in a superposition of states we are no longer able to think of the system as possessing _all_ its properties completely independently from other systems. We find that for a complete specification of a given property, reference must be made to other physical systems. (By _complete specification_, I mean being able to say not only that the system has the given property, but also that it has a definite _value_ for that property. And of course, the _given_ property is that for which the component states of the superposition are eigenstates.) Hence, the _independent_ characterization of a particular physical system is restricted to this extent.

But it is a simple tenet of QT that, for a given system, even if it _is_ in a superposition with respect to a specified observable, it can, at the same time, be in an eigenstate with respect to another observable. For example, if a given micro-particle is in a superposition of x-spin, then it can be in an eigenstate of y-spin. Hence, though the first observable requires reference to another distinct physical system for its complete specification, the second observable suffers no such limitations. This second observable can be identified and assigned a definite value by appeal solely to our object system. Thus, with respect to this observable we can still speak of a system as possessing a property, and of the property being characteristic of the system as opposed to being merely a relational property between the system and something else.

Thus while some of the properties of a physical system must be considered subjective and relational, others --at the same time--are just as objective and independent as ever. So what we have is neither a complete or absolute subjectivism nor a complete or absolute objectivism (i.e., absence of subjectivism). We do not have the claim that a micro physical system--in every respect--has a subjective, relational, and "dependent" existence. Rather at any given time, some of the properties of the system do not have an independent existence. But this "lack of independence" does not permeate the system. There are other properties, at the same time, that maintain their independent status. And since it was this lack of independence which prompted us to question the objective existence of some of the properties, then the independence of the other properties compels us to attribute to them an objective existence--and thereby, an objective existence (i.e., reality) to the system as a whole which possesses these independent properties.

We notice finally, that under different circumstances different observables will be the "independent" ones. Thus, no one set of observables are forever relegated to being "dependent." The roles of "independent" and "dependent" can be interchanged at will. For instance, if a given system is in an eigenstate of y-spin ("independent" y-spin), it will be in a superposition of x-spin ("dependent" x-spin). But this situation can be completely reversed simply by allowing the system to pass through a Stern-Gerlach device which has its magnetic field oriented in the x-direction. Then the system will be in an eigenstate of x-spin ("independent" x-spin) and in a superposition of y-spin ("dependent" y-spin).

We also notice that there are some observables that seem not to participate in this drama at all, and are always found to be "independent." I am speaking of those observables for which no superposition has ever been found, e.g., electric charge. This is a clear violation of the superposition principle, and understandably, this proves to be a suspension of the primary conclusions of this

thesis. The reader will note that the existence of these states which violate the superposition principle (i.e., those states associated with different values of electric charge, states which fall under the so-called _super-selection rules_) gives even stronger evidence of the objective, independent existence of the physical system itself, since we have here a property of the system that is _always_ independent.

The second consideration which leads us to attribute a real existence to micro physical systems comes from relativistic QT. We have considered the results of classical physics, relativistic physics, and quantum physics. We have not, until now, mentioned relativistic quantum physics. But it is essential to do so, primarily because non-relativistic QT is basically a limited and incomplete theory. This is due to the fact that non-relativistic QT is incapable of accounting for the following fundamental aspects of the physical universe: (1) The merger of space and time into a single four-dimensional continuum effected by the special theory of relativity as we have seen. (2) The spatio-temporal pervasiveness of the basic physical quantities requiring a field-theoretic approach to physical nature. (3) The discovery of the sub-universe of elementary particles including the unexpected variety of their masses, life-spans, and their numerous quantal characteristics such as spin and strangeness, in addition to their susceptibility to creation and annihilation. In

contrast to non-relativistic QT, several contemporary relativistic field theories are able to account, more or less satisfactorily, for these three aspects of the physical world. In particular, these can be accounted for by quantum electrodynamics, which is the only theory which we shall consider.

First of all, the essential elements of QT are also present in quantum electrodynamics. Thus, several field quantities, e.g., the electric and magnetic field strengths, are not measurable simultaneously because they are represented, in the new theory, by non-commuting hermitian operators. But what can quantum electrodynamics tell us about the reality of micro physical systems? What quantum electrodynamics tells us is basically this: from quantum electrodynamics we can stress the <u>existential import</u> of the fact of the <u>countability</u> of various types of elementary particles possessing various momenta, charges, spins, etc. This is a way of establishing the existence of physical entities that was used by Poincaré in another context. Poincaré argued that molecules are real since the number of molecules in a particular region can often be reliably determined: he felt that it would be impossible to count non-existent or unreal things. If this type of argument is accepted, then we can apply the same kind of reasoning in quantum electrodynamics to Feynman's rules concerning the matrix elements related to various processes at the quantum level.

We find that the physical reality of electrons, positrons, and photons (these are the three types of elementary particles and their interactions with electromagnetic fields that form the subject matter of quantum electrodynamics), including the reality of the momenta, energies, and polarizations ascribable to them, can be supported by an argument exactly duplicating Poincaré's argument in favor of molecular reality. We can argue that electrons, positrons, and photons possessing specifiable momenta, energies, and polarizations must exist since they can be counted. The possibility of determining the numbers of any of these particles assumed to possess any specifiable momentum, energy, and polarization properties is made apparent by the well-established Feynman rules concerning the matrix elements (i.e., the amplitudes of transition probabilities) for any process involving the three types of micro systems and their associated fields. According to Feynman, any matrix element of this type corresponding to a particular system S of elementary particles and their respective fields involves several physically significant numbers: (1) The number of electron-lines external with regard to S which imply a specified momentum and polarization. (2) The number of external photon-lines with a specified energy, polarization, and possibly with a specified external electromagnetic field. Further, (3) the number of internal lines of the three types of

particles involved in every matrix element, in addition to (4) the number of closed electron loops with an even number of electron-lines.

It is also true that from within quantum electrodynamics we could stress the existential import of the empirically verifiable occupation numbers. And the other facts and laws concerning elementary particles contribute to the establishment of an objective, independent, physical world in many other ways. But I think the point has been made. It is simply a logical triviality that elementary particles could not be created or destroyed if there were no elementary particles at all. It is equally obvious that no classification of elementary particles based on their rest mass, or charge, or spin, or strangeness or any other attribute could have been validly established if there were no set of elementary particles to classify. The growing list of discoveries concerning elementary particles and presupposing their physical reality is as impressive as any physical discovery ever was.

Therefore (at the risk of being accused of beating a dead horse), it is not the physical reality of physical systems that I wish to question here, but only the question of the objective, independent existence of the properties of those systems. Having just given a defense of the reality of physical systems, and before we attend to our final conclusions, let me give--from a new direction--a defense of the claim of the "lack of independence" of the properties of micro physical systems. This argument comes from a consideration of the so-called uncertainty principle (or alternatively, the indeterminacy principle). Discussion of this principle has perhaps been conspicuously absent from our investigation, for this principle is rightly considered as one of the central tenets of QT. We will show that the indeterminacy principle involves us in the same kind of severe limitations as does the superposition principle when it comes to ascribing properties to microphysical systems.

We will consider what can be conveniently called the "strong indeterminacy principle." This theorem was proved a few years after the discovery of Heisenberg's classical and less comprehensive indeterminacy principle [38]. The theorem can be stated as follows: let a, b, c, d be four real numbers satisfying the inequalities $b > a$ and $d > c$. Let P(a,b) be the property possessed by any system S such that S is within a sphere of radius b but outside a concentric sphere of radius a. Similarly, let M(c,d) denote the property possessed by every system S such that the modulus of the linear momentum of S exceeds c and is smaller than d. Under these assumptions, the strong indeterminacy principle asserts that the hermitian operators on the Hilbert space associated with S and

corresponding to the properties P(a,b) and M(c,d), respectively, do not commute with each other for any quadruple of numbers satisfying the above inequalities. According to the standard interpretation of QT, no system S can have at the same time both properties P(a,b) and M(c,d) unless the numbers b and d are suitably restricted. This means, however, that no matter how unsharply a position and a corresponding momentum be defined, no system can have either unless the outcome of a measurement of either P(a,b) or M(c,d) performed on S entails, in conjunction with other data and QT laws, that S has the property measured. <u>If none of these two properties were measured on S then S would not have any P or M-properties whatever</u>. Thus we see, in a different context, and using different words, the same severe limitations whenever we attempt to speak of the <u>unobserved</u> properties of physical systems in the micro world.

<u>Concluding remarks</u>. We began our investigation with the conviction that the kind of solutions proposed under group B would be the most "desirable." That is, we felt this kind of solution would require the least amount of adjustment overall since it would allow us to accept the subjectivism on the micro level (thus accepting the claims of QT), and at the same time allow us to reject the subjectivism on the macro level and thus retain an intuitive macro realism (thus maintaining at least the approximate

validity of the metaphysical structure of classical physics and common sense). Therefore we looked to these accounts to tell us how we could have the subjectivism on the micro level, but find that it had somehow been "dissipated" when we come to the macro level. And, finally, we sought to associate this "dissipation" of the subjectivism with the reduction of the wave function. Let us recall what we have discovered during our inquiry.

We found that the idea of a reduction of the wave function, as usually understood, cannot be formally fitted into the standard framework of QT (Komar). We were advised that the question of a reduction actually need never arise: a reduction is always a reduction of a superposition to a mixture; it is not possible for the combined system (including the macroscopic measuring apparatus) to _ever_ be in a superposition to begin with--because if there were such a superposition, detection of its interference effects would require some wholly unreasonable conditions. Thus for the combined system we always have a mixture (Peres and Rosen). It is true that Peres and Rosen never tell us _how_ or _why_ we get the mixture in the _combined system_ from the initial superposition of the _object system_; they only tell us that we know it must be a mixture. We have found that it is _consistent_ with QT to assume that a reduction _never_ occurs, but that there is some kind of "localization of consciousness" (Everett, _et al_.). We

were told that formally, we could simultaneously account for the presence of a superposition and also the fact that it always _appears_ as a mixture (Jauch). Unfortunately, this proposal gave us no greater _physical_ understanding of the problem than we had with the indivisibility argument which makes appeal to an "_irrational_ element" in nature (Bohr). Further, we have found that specific attempts to "locate" the reduction in the macroscopic behavior of the measuring device were less than satisfactory, both for individual reasons peculiar to each account, and because all such accounts suffer from the objection that some times the macroscopic "amplification" of a measurement result does not occur until much later after the interaction between the object system and the measuring instrument (Blokhintsev, and Daneri, _et al_.).

Therefore we must conclude that, _as of yet_, no satisfactory account of the reduction of the wave function, as usually understood, has been given. To that extent we have no complete understanding--in the direction originally sought--of how we can adopt an overall view of physical nature that is able to accommodate both the microscopic subjectivism and the macroscopic objectivism (lack or absence of subjectivism). If one is still inclined to hold onto the two "basic facts" of the situation--viz, the existence of subjectivism on the micro level and its absence on the macro level--this is not necessarily cause

for despair. But of course, this is likewise no cause for rejoicing.

What we sought was an account which begins with microscopic subjectivism, and then explains, using QT, why this kind of subjectivism is not present on the macroscopic level; and this was to be achieved by showing how the wave function is reduced. Though this has not satisfactorily been accomplished--in complete generality--this does not mean that it cannot or will not be. It does mean, however, that to the extent that this "explanation" of reduction is required to complete the quantum theoretical account of measurement, we still have with us the exacerbating difficulties of interpretation that have been with us from the beginning. (Let us hope that the situation in QT is not of the same kind as prompted someone to say: "The poor we will always have with us.") In particular for our investigation here, it means that to this extent we have not been able simultaneously to account for the subjectivism on the micro level and its absence on the macro level. Hence our position would be that of being obliged to accept the new-found subjectivism on the microscopic level, but with the knowledge that regrettably we do not yet have a full understanding of how to incorporate this new "fact" into our overall world-view.

But this should not distract the reader's attention from the truly significant result that we have obtained. In microscopic subjectivism we have discovered an aspect of physical nature that is surely remarkable. This is an idea that is totally alien to the very spirit of scientific thought which has prevailed essentially unchallenged since the modern inception of science around the seventeenth century: this is the view that the physical world can be described ultimately in terms of independently existing individual parts with well-defined properties. But viewing particles on the micro level as in some sense the "ultimate constituents of matter," we have found that at the very heart of the physical world--the independently existing individual parts--we cannot maintain this view. There is an interconnection and interdependence between these "ultimate constituents" that is nearly all-pervasive.

What is the nature of this interdependence? In the absence of any definitive solution to the problem of the reduction of the wave function, we cannot say categorically that human consciousness will play no role in understanding this process. But it certainly seems to be the case that consciousness plays no essential part on the macroscopic level. For we have a wealth of evidence supporting the approximate validity of classical physics and thus supporting the assumption of an "observer-independent" physical world embodied in this theory. However, on the microscopic level we see clearly that the source of the subjectivism found there is in no way connected with the consciousness of any observer. We found that this subjectivism (with respect to the properties of micro objects) arises because of the need to take into account _other physical objects, not other physical objects with consciousness_. Consciousness has entered into our discussion in no essential way whatever. Thus we see that with regard to the two ways of removing the independence of an object (i.e., because of the need to take into account: (1) other objects, (2) other conscious objects), it is only the first that is in operation here. We have found that microscopic objects have a dependent existence alright, but it is an "interaction-dependence" not an "observer-dependence." We note that this runs counter to a prominent philosophical tendency of considering only a kind of "observer-dependence." To this extent our conclusion of microscopic subjectivism is substantially different from many philosophical claims of subjectivism, for we locate the origin of the subjectivism _not_ in the presence of conscious observers, but in the existence of other physical objects.

It is well known that theories entailing some feature of ultimate interconnection of this sort is not new in philosophy. (Compare the coherence theory of truth and the ontological position underlying it. See, for example, the philosophy of F. H. Bradley.) But in

physics this idea is completely novel. Since the beginning of modern science, physics has steadfastly refused to relinquish the straightforward ontological (and epistemological) realism that has served it so well and so long (that is, until the advent of relativity theory and quantum theory). It is indeed remarkable--in light of a long history of philosophical subjectivism, relativism, and idealism--that physics now seems compelled to adopt the view that at the very heart of the matter there is this irreducible subjectivism. This clearly is a radical shift in the very fabric of scientific thought. Investigation of such a noteworthy result has unquestionably made this inquiry worthwhile.

POSTSCRIPT:

PHYSICS AND METAPHYSICS

"A metaphysician," wrote James Clerk Maxwell, "is nothing but a physicist disarmed of all his weapons,--a disembodied spirit trying to measure distances in terms of his own cubit, to form a chronology in which intervals of time are measured by the number of thoughts which they include, and to evolve a standard pound out of his own self-consciousness." [In a paper of 1873 on determinism and contingency published posthumously in L. Campbell and W. Garnett, The Life of James Clerk Maxwell (London, 1882), p. 436.] Denunciation of metaphysics is, in fact, one of the threads that has run unbroken through the history of physics since its fantastic upsurge in the seventeenth century. As a matter of fact, physics has not been the only domain from which dispersions have been cast upon metaphysics. Even *within* philosophy--in recent times, especially--there have been those that have treated metaphysics with distrust and even disdain.

It would not be surprising, one might argue, if we discovered that physicists in fact had a jaundiced and

distorted view of what metaphysics really is. Hence we would be prepared to treat their criticisms with perhaps less seriousness than they would wish. But on the other hand, similar criticisms by philosophers *might* deserve more careful attention. In particular, everyone knows what a devastating critique Kant made of metaphysics. In light of Kant's critique how could there be enough of metaphysics "left standing" to warrant any further consideration of it at this time? Let us try to discover just what metaphysics *is*, or is *supposed* to be.

The name of the subject is the name given to a treatise by Aristotle. He described the subject of his treatise as the science of Being as such, a supremely general study of existence or reality, distinct from any of the special sciences and more fundamental than any of them. Aristotle argued that there must be such a science; since each of the special sciences, besides having its own peculiar subject matter, made use in common with all the others of certain quite general notions, such as exist, same, like and unlike, unity and difference, possible and impossible. Such common notions such as these would provide the topics of the general science of being. A more recent characterization of metaphysics is provided by F. H. Bradley. He writes: "We may agree, perhaps, to understand by metaphysics an attempt to know reality as against mere appearance, or study of first

principles or ultimate truths, or again the effort to comprehend the universe, not simply piecemeal or by fragments, but somehow as a whole."

One might be in complete sympathy with the program as outlined by Aristotle and Bradley and still hold that, as a matter of fact, the history of metaphysics shows us that this discipline has gone from one state of disgraceful shambles to another. Not only have metaphysicians proposed some of the most peculiar systems of thought imaginable, but different philosophers have proposed radically different systems--with these systems supposedly "explaining" the same things. There seem to be as many different metaphysical systems (many of which are quite strange in their own right) as there are practicing metaphysicians. There hardly ever seems to be any agreement or any progress (one might continue). There just does not seem to be enough in metaphysics which is generally agreed upon, and which would make it stable enough, clear enough, and well defined enough to warrant treating it as anything like a mature and respectable discipline.

While these kinds of feelings might be considered a bit extreme, they do point out something which is very important, and which, to a great extent, accounts for the kind of "multiplicity" of views mentioned above: many important metaphysical arguments are not the sort of arguments that can just be accepted as valid or rejected as invalid by certain and generally agreed upon rules. Their value or their faults are likely to lie much deeper, in some central concept or idea which the metaphysician is trying to articulate. And since, understandably, there can be great variation in the central idea or concept adopted by a philosopher, there will likewise be great variation in the resultant conceptual systems--which often are simply mutually incompatible with each other.

Let us try to see in more detail exactly what it is that metaphysics attempts to do. There are two ways in which we can think of metaphysics. It is only the first that falls victim to Kant's criticisms. Kant argues that we are inclined to think of the universe, its nature, and its history as being in principle completely knowable. We realize, of course, that our actual experience and knowledge of it are inevitably partial and limited; no doubt we do not think that we ourselves can sensibly aspire to know everything about everything. But we still do, says Kant, have the idea that what we do know is a fragment of some ideal whole, that what we experience is a part of some <u>totality</u> of things and events. In the same way, says Kant, we have the ideal of some final and absolute explanation. Here too, we are aware that our own explanations of events are given merely in terms of further events, which themselves stand in need of yet further explanation. But as we extend our always limited power to explain, we

think of ourselves as making some approach to a goal, getting nearer to the standpoint, which doubtless in fact we shall never reach, from which everything could be wholly explained without any remainder. The central idea here is that of completeness. Our powers and our experience are always limited, but as we seek to extend our knowledge and understanding, we are impelled to think of ourselves as getting fractionally closer to the ideal goal of complete knowledge, and complete understanding, of the sum of all things.

Now Kant sees the metaphysician as a thinker obsessed by this ideal of completeness. He seeks the explanation not of this or that, but of everything; he seeks to know the nature, not of some things, but of all things. Because we know what it is to know something about something, and to know more and more about more and more, the metaphysician thinks that we must be able to say what it would be like to know everything about everything.

It is this unrestricted use of "all" and "everything" that is characteristic of the metaphysician. But this unbridled usage is precisely the real ground for the impossibility of this traditional kind of deductive metaphysical system: there can be no sense in the notion of complete knowledge. By "extrapolating" his arguments all the way to the top (as it were), the metaphysican has extended the use of certain concepts beyond the domain

of their meaningful application, says Kant. Concepts that we apply within the world cannot necessarily or even normally be applied intelligibly to the world as a whole.

This kind of metaphysics, which we can call onto-logizing, can never be justified. And the reason is quite simple. Any assertion of the existence of something, like any assertion of the occurrence of something, can be denied without logical absurdity. So the reasons for assertions of existence or occurrence can never be purely conceptual considerations. Hence we can never return to the kind of metaphysics that begins with nothing but "first principles," and then proceeds to tell us what the world is like. If we wish to know what there actually is, or what actually goes on in the universe, then we must dig in the appropriate parts of the universe itself.

However, there is another way in which we can think of metaphysics which is not subject to the kind of criticism levelled by Kant. To see this consider the following: it is true that what most traditional metaphysicians had in mind to do was to provide an ontology, a general picture of the world. But they would maintain that if an ontology, an account of the actual structure of the universe, could be demonstratively established, important consequences would ensue on the conceptual level. For instance, if a materialist ontology, to the effect that the universe is really a system of independently existing

material bodies influencing each other by impact, were true, then it would follow that a certain conceptual system, viz, that embodied in the language of classical mechanics, was correct and that all others, however handy for immediate practical purposes, were ultimately inadequate and misleading.

Hence, we form a picture in which the real point of constructing a metaphysical system is the recommendation of a particular conceptual framework, with the prime aspiration of a metaphysician being that of <u>conceptual revision</u>. Thus the enterprise of metaphysics emerges as, above all, an attempt to re-order or to reorganize the set of ideas with which we think about the world; assimilating to one another some things which we customarily distinguish, distinguishing others which we normally assimilate; promoting some ideas to key positions, downgrading or dismissing others. It is supremely a kind of conceptual revision which the metaphysician undertakes, a re-drawing of the map of thought--or parts of it--on a new plan. And the concepts which the metaphysician manipulates in this revision, are always concepts like knowledge, existence, identity, reality--which as Aristotle said, are common to all the special and departmental studies.

Viewed in this manner, the metaphysician's purpose is not simply to deduce a conclusion from the facts. It is rather to show that the account of those facts, when

we reflect on them, has a hole in it, a hole which is exactly filled by the metaphysician's special concept. This concept may be one, like that of "an experience," which exists already in a rough form in our ordinary language, and which the metaphysician takes up, dignifies and refines into a principle of explanation. Alternatively, if he is a very thorough going metaphysician, the concepts he uses in this way may be much more technical and remote from ordinary thinking, like Leibniz's "monads" or Kant's "noumenal objects."

The metaphysician feels an inconsistency or difficulty or incompleteness in what we naturally tend to think about some features of our experience, or rather in what seems to be presupposed by what we so think. In resolving this, he will try to show that some concept on which we rely is secondary to, or presupposed by, some other concept which he has introduced or extended from elsewhere. This concept of his may have a special place in the answer to the problem in question (like the empiricist's use of "experience"), or he may use it widely elsewhere (like Plato's Forms) to solve other problems. Hence, the metaphysician's activity can be seen (and has been seen by some) as primarily one of reallocation: the extension of some favored concept to a primary place in the account of things at the expense of more familiar concepts.

Thus, we must certainly conclude that deductive metaphysics, viewed in the sense of providing an ontology for the world, starting from "first principles," is no longer a "feasible" activity for a philosopher. For under this view what was commonly expected of a metaphysician was that he should assert the existence or occurrence of things unseen and give for these assertions purely philosophical or conceptual reasons. If he was not an ontologist, he was not a metaphysician. But this view is no longer a tenable one. And philosophy is the better for it. If metaphysics is to be retained as a worthwhile enterprise at all, then it must be in the sense of the conceptual revision described above.

But by now the reader might feel that this is all very nice and interesting about metaphysics, but what has any of this to do with physics? Indeed, it might even be felt that these are on exact opposite ends of the intellectual spectrum. For (it might be felt) even the most general and basic laws of physics would ultimately depend for their acceptability upon the results of observation and experiment in a way which is quite uncharacteristic of the principles of a metaphysical system. The methods of physics (and science in general), the tests for acceptability of physical laws, remain quite different from the methods of metaphysics and the test for acceptability of metaphysical principles. Though this much seems undeniable, the following is also true: even though the most general laws or axioms of a unified science would not count as metaphysics, it is true that many metaphysicians have thought it at least a part of their task to lay, or to lay bare, the foundations of science (e.g., Descartes or Kant). They conceived of the relation as being between metaphysical foundations and the scientific superstructure.

How are we to view the relation, if any, between physics and metaphysics? Putting the question in a crude way, we can quite bluntly ask: does physics need metaphysics? and does metaphysics need physics? The answer to the second question is clear. We have seen that the only kind of respectable metaphysics that is left is metaphysics considered as a kind of conceptual reorganization; wholesale ontologizing is gone forever. But clearly, the activity of conceptual revision is also ontologizing of a "milder" sort. For the revision of conceptual frameworks is recommended only because the new framework is regarded as a more accurate reflection, or more adequate representation of the way things really are. And the only thing that can keep even this milder form of ontologizing from becoming the vicious type is the assurance that this ontologizing is not done solely by appeal to philosophical and conceptual considerations. Rather there must be proper regard for the contingent, empirical elements of nature.

We noted earlier that if we wish to know about what actually goes on in the universe, then we must quarry in the appropriate parts of the universe itself. Thus to keep our mild ontologizing from becoming vicious, we must make our initial appeal not to "first principles" but to physics--since physics is precisely what tells us the goings-on of the universe. Metaphysics of this kind--and this is the only kind which remains defensible--must be done <u>in light of</u> physics. Philosophizing in this sense is therefore a <u>second-order</u> enterprise which is done <u>given</u> the results of the first-order studies, viz, physics. We cannot, any longer, attempt to build a metaphysical system from scratch, we can only build it "on top of" physics. That is, we can erect a metaphysical structure only if it has physics as its foundation.

But just as physics is so intimately connected with metaphysics, I wish to argue, in conclusion, that metaphysics is just as intimately connected with physics. For physics seeks to give more than just a correlation between facts. It seeks a deeper understanding of why the facts are the way they are. It seeks, in other words, to give a correlation between facts by appealing to "the way the world is." And as physics progresses it attempts to provide a more and more adequate understanding of "the way the world is." Clearly now we can see that metaphysical assertions arise out of the very depths of the scientific enterprise. For not only does physics attempt to tell us "the way the world is" (which is patently metaphysical), but it seeks to do so in an ever more adequate way. And a change to a more adequate view of nature will very frequently make necessary a change in the ideas and notions required to articulate this view of nature--i.e., a conceptual revision. Physics, at its most fundamental levels, is metaphysics.

Thus in a very meaningful sense, there was a kernel of truth in the first part of Maxwell's statement. For in many crucial ways both metaphysicians and physicists are seeking the same treasure: understanding the "way the world is" (as completely as possible). But instead of saying "A metaphysician is nothing but a physicist disarmed of all his weapons," we might more fairly, more adequately, and certainly more ecumenically, express the situation by saying "A physicist is a metaphysician armed with all the weapons of modern science--mathematics and technology--in all their awesome power."

And we have just seen in the body of this thesis, an instance of precisely this kind of inter-dependence between physics and metaphysics. For we have arrived at a conclusion regarding the ontological status of the ultimate constituents of the physical universe (viz, the conclusion of microscopic subjectivism). This is most certainly the kind of assertion sought after in traditional metaphysical

descriptions of the world. We note: (1) that this assertion was arrived at by an investigation conducted within physics. Hence we have a metaphysical claim arising out of a study of the fundamental aspects of physical theory; and (2) since any acceptable philosophical theory must not be in conflict with existing physical theory (assuming, of course, that the philosophical theory is about some aspect of the physical universe), any future philosophizing of a metaphysical or ontological nature, must be done in light of what we have discovered here (from physics) about the ontological nature of the ultimate constituents of the physical universe. Hence, we see that any future metaphysics must be done in such a way that it picks up where physics leaves off, in the sense that it must begin with whatever metaphysical conclusions are provided by physics and proceed from there. In particular, any future metaphysical account of nature must take into account the demonstrated interdependence among the elementary constituents of the world.

Hence there is indeed a kind of study that is both respectable and necessary for our understanding of the physical universe. It may be thought of as a part of physics, and we have seen that physics at its most fundamental levels, is indeed metaphysical in character. Or it may be thought of as a part of philosophy. In that case, we have seen that it is a second-order discipline

which requires physics as its foundations. In either case, we see that the connection is certainly intimate enough to warrant consideration of both aspects by anyone who seeks a "complete" understanding of the nature of the physical world.

BIBLIOGRAPHY

BIBLIOGRAPHY

1. J. Albertson, American Journal of Physics, 29, 478 (1961).

2. L. E. Ballentine, Reviews of Modern Physics, 42(4), 358 (1970).

3. J. S. Bell, Reviews of Modern Physics, 38, 447 (1966).

4. J. S. Bell, Physics, 1(3), 195 (1964).

5. D. I. Blokhintsev, Soviet Uspekhi, 11(3), 320 (1968).

6. D. Bohm, Physical Review, 85, 180 (1952).

7. D. Bohm, Physical Review, 85, 166 (1952); 87, 389 (1952); 89, 319, 458 (1953).

8. D. Bohm and J. Bub, Reviews of Modern Physics, 38, 453, 470 (1966).

9. N. Bohr, Atomic Theory and the Description of Nature (Cambridge Univ. Press, New York, 1934).

10. N. Bohr, Atomic Theory and Human Knowledge (Interscience, New York, 1963).

11. N. Bohr, Physical Review, 48, 696 (1935).

12. J. Bub, "The Daneri-Loinger-Prosperi Quantum Theory of Measurement" unpublished.

13. M. Bunge, ed., Quantum Theory and Reality (Springer, Berlin, 1967).

14. E. A. Burtt, The Metaphysical Foundations of Modern Physical Science (Routledge and Kegan-Paul, 2nd edition, 1932).

15. R. Carnap, "Empiricism, Semantics, Ontology" (1950).

16. A. Daneri, A. Loinger, G. M. Prosperi, Nuclear Physics, 33, 297 (1962).

17. A. Daneri, A. Loinger, G. M. Prosperi, Nuovo Cimento, 44B, 119 (1966).

18. B. S. DeWitt, Physics Today, Sept., 1970.

19. P. A. M. Dirac, The Principles of Quantum Mechanics (The Clarendon Press, Oxford, 4th edition, 1958), p. 33.

20. A. Einstein, B. Podolsky, N. Rosen, Physical Review, 47, 777 (1935).

21. H. Everett, III, Reviews of Modern Physics, 29, 454 (1957).

22. R. P. Feynman, Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability, 533 (1951).

23. A. Fine, Philosophy of Science, 35, 101 (1968).

24. N. R. Hanson, The Concept of the Positron (Cambridge Univ. Press, 1963).

25. W. Heisenberg, Physics and Philosophy (Harper and Brothers, New York, 1958).

26. W. Heisenberg, The Physical Principles of Quantum Theory (Univ. of Chicago Press, 1930).

27. R. J. Hirst, The Problems of Perception (Humanities Press, New York, 1959).

28. M. Jammer, The Conceptual Development of Quantum Mechanics (McGraw-Hill, New York, 1967).

29. J. Jauch, Foundations of Quantum Mechanics (Addison-Wesley, Reading, Mass., 1968).

30. J. Jauch, Helvectica Physica Acta, 37, 293 (1964).

31. J. Jauch, E. P. Wigner, M. M. Yanase, Nuovo Cimento, XLVIII, 144 (1967).

32. W. E. Johnson, Encyclopedia of Philosophy, Vol. 2, p. 357, "Determinables and Determinates."

33. S. Kochen and E. P. Specker, Journal of Mathematics and Mechanics, 17, 59 (1967).

34. A. W. Kolmogorov, Foundations of the Theory of Probability (Chelsea Publishing Co., New York, 2nd English edition, 1956).

35. A. Komar, Physical Review, 126, 365 (1962).

36. B. Koopman, Proceedings of the Symposium in Applied Mathematics, VII, 97 (1957).

37. F. London and E. Bauer, La theorie de l'observation en mechanique quantique (Actualities Industriells et Scientifiques, Hermann, Paris, 1939).

38. G. Ludwig, Die Grundlagen Der Quantenmechanik (Springer-Verlag, Berlin, 1954).

39. L. Mandel and R. L. Pfleegor, Physical Review, 159, 1084 (1967).

40. H. Margenau, Philosophy of Science, 30(1), 138 (1963).

41. H. Margenau and L. Cohen, in Quantum Theory and Reality, ed. M. Bunge.

42. H. Mehlberg, in Quantum Theory and Reality, ed. M. Bunge.

43. K. Menger, Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability, 2, 215 (1954).

44. A. Messiah, Quantum Mechanics (North Holland Publishing Co., Amsterdam, 1964).

45. J. E. Moyal, Proceedings of the Cambridge Philosophical Society, 45, 99 (1949).

46. A. Peres and N. Rosen, Physical Review, 135, B1486 (1964).

47. A. Petersen, Quantum Physics and the Philosophical Tradition (MIT Press, Cambridge, 1968).

48. N. Rosen, American Journal of Physics, 32, 597 (1964).

49. R. Schlegel, Completeness in Science (Appleton-Century-Crofts, New York, 1967).

50. R. Schlegel, Synthese, 21(1), 65 (1970).

51. A. Shimony, American Journal of Physics, 31, 755 (1963).

52. J. Sneed, Philosophy of Science, 33, 22 (1966).

53. H. P. Stapp, Physical Review D, 3(6), 1303 (1971).

54. P. Suppes, Philosophy of Science, 33, 14 (1966).

55. J. Von Neumann, Mathematical Foundations of Quantum Mechanics (Princeton Univ. Press, Princeton, 1955).

56. J. A. Wheeler, Reviews of Modern Physics, 29, 454 (1957).

57. E. P. Wigner, American Journal of Physics, 31, 6 (1963).

58. E. P. Wigner, in The Scientist Speculates, ed. I. J. Good (William Heineman, Ltd., London, 1961).

59. E. P. Wigner, American Journal of Physics, 38(8), 1005 (1970).

APPENDIX

APPENDIX

Bell's theorem states that no theory can: (a) give contingent general predictions of the individual results of measurements; (b) be compatible with the statistical predictions of QT (to within, say, 5%); (c) satisfy "local causes." I will explain what all this means. A prediction of an _individual_ result is, for example, the prediction of whether an individual particle in a Stern-Gerlach device will be deflected up or down. QT predicts the probabilities of these two alternatives, but not, in general, the individual result itself. The word _general_ in condition (a) specifies that the individual results of Stern-Gerlach-type measurements are to be predicted by the theory.

The axis of a Stern-Gerlach device can be rotated: it can have different alternative possible settings. The word _contingent_ in condition (a) means that the theory gives predictions for the various possible alternative settings. It does not merely give predictions only for the one unique setting that is actually chosen by the experimenter.

Consider now an experiment in which two particles are made to collide, with each of the scattered particles going through one of two Stern-Gerlach devices, denoted respectively by D_1 and D_2. Our three conditions are now just: (a) if the experimenters had actually adjusted the mechanical devices to give the alternative experimental setup, then these alternative experiments would have had certain definite results--and these results are predictable by the theory; (b) the equations of the theory, regarding any correlations between the two devices, must hold (to within 5%); (c) the requirement of local causes is that the deflection of the particle going through the first device should not depend appreciably on what is the setting of the second device, and *vice versa*. The setting of D_1 can be made just before the arrival of particle 1 and the setting of D_2 can be made just before the arrival of particle 2. In this case, any large dependence of the results of a measurement made by D_1 on the orientation of D_2 or any large dependence of the results of a measurement made by D_2 on the orientation of D_1, would require a large and almost instantaneous effect of a far-away cause.

These three assumptions all seem plausible. Yet taken together they are incompatible and lead to a contradiction (see Wigner [59]; Stapp, Bell, Ballentine). The immediate conclusion to be drawn from this theorem is that the demands of causality, locality, and individuality cannot be simultaneously maintained in the description of nature (assuming we wish to have at least statistical agreement with QT). Causality demands contingent predictions; locality demands local causes of localized results; individuality demands the specification of individual results, not merely their probabilities.

The contradiction posed by Bell's theorem is rather disconcerting, for these three assumptions are such that we would not want to give up any one of them. Assumption (b) is obviously untouchable, there is simply too much empirical support of the statistical predictions of QT. Assumption (a) is essentially just the requirement that nature be fundamentally lawful, in the sense that the individual results are specified by contingent rules. And assumption (c) is just the condition that the world be fundamentally separable into independent parts, on the macroscopic level. For if a cause can have a large instantaneous effect far away, then far apart macroscopic objects can no longer be considered separate and distinct in the usual sense.