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PHILOSOPHICAL CONSEQUENCES OF QUANTUM THEORY

Reflections on Bell's Theorem

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Cover: figure from John S. Bell's Collège de France lecture, "Bertlmann's Socks and the Nature of Reality," delivered to an audience of philosophers and physicists in 1980. The reader not already aware of the profound implications of Bertlmann's socks for the nature of reality should consult Bell's elegant lecture, originally published in the *Journal de Physique*, 1981, and reprinted in his anthology, *Speakable and Unspeakable in Quantum Mechanics,* 1987.

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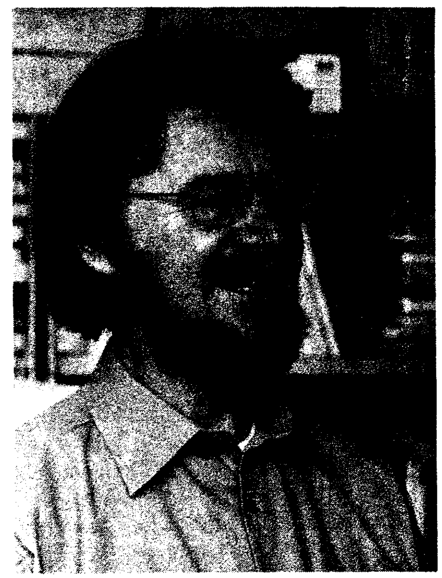
For John Bell
In honor of his sixtieth birthday



A BACKGROUND ESSAY

JAMES T. CUSHING

The purpose of the papers in this volume is to discuss the hard questions and hard choices that recent quantum physics has presented for philosophy in general, not just for the philosophy of science. The authors examine what has been established, what options are still available, and what revisions, radical or otherwise, may be necessary in our philosophical views. Since the volume is intended for philosophers in general, and not just for experts in the foundational problems in quantum theory, the papers are not thick with technical details. A central development to which all of these papers are in some way related is Bell's theorem. There is an enormous literature on the technical aspects of Bell's theorem and on the foundational problems of quantum mechanics (see, for example, Ballentine 1987). My task here is to provide some introductory material that will help the reader new to this subject to understand the subsequent papers. Let me be explicit in stating that the tale which follows is not always chronologically faithful to the historical record nor is it in all details a literal transcription from the original papers cited. By way of orientation, I begin with a general and somewhat loose overview of the subject and then proceed to define terms and concepts more precisely.

A little history

While it is true that interest in the interpretative problems of quantum mechanics received major impetus from the seminal paper of John Bell (1964) on the Einstein-Podolsky-Rosen (EPR) paradox, there was life in the field before Bell—and before EPR too (see, for example, Wheeler and Zurek

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1983). As early as 1913, *before* Bohr's paper of that year on the semiclassical model of the hydrogen atom had appeared in print, Rutherford pointed out a problem for causality in Bohr's model. Bohr had postulated that the frequency ν of light emitted by an electron in its transition from an initial energy level E_m to a final level E_n (figure 1) is given by

$$E_m - E_n = h\nu.$$

To Rutherford, it appeared as though the electron would have to know to what energy level it was going before it could decide what frequency it should emit (Hoyer 1981, 112). By 1917, Einstein wanted to know how, in Bohr's model, the photon decided in what direction it should move off (figure 2). Schrödinger attempted a largely classical interpretation of his own equation, but Max Born (1926) proposed a consistent statistical interpretation of quantum mechanics. Determinism, in the sense of our being able to predict *the* unique outcome of a measurement on an event-by-event basis, was gone from the formalism, although Einstein and Schrödinger struggled (Przibram 1967) against what became codified as the "Copenhagen" interpretation of quantum mechanics. True, the majority of physicists (if they chose to think about the issue at all) *believed* that atomic events could not, even in principle, be predicted on an event-by-event basis. Still, one could (and some notables did) question the completeness of quantum mechanics, asking whether there might not exist a successor theory which *could,* in principle, make such event-by-event predictions. In fact, von Neumann ([1932] 1955, 313–328) offered a "proof" that such "hidden-variables" theories could not exist. Much later, Bell (1966) did address the question of the relevance of that "proof."

In 1935, Einstein, Podolsky and Rosen (EPR) published a paper in which they questioned the completeness of quantum mechanics. That is, they asked whether one could be certain, on physical grounds, that more could be specified (or known) about a system than could be predicted with certainty by the formalism of quantum mechanics. By means of a specific thought experiment, they argued that the incompleteness of quantum mechanics was entailed

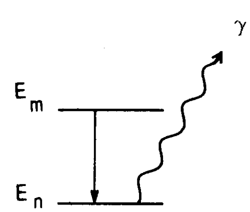

Figure 1.

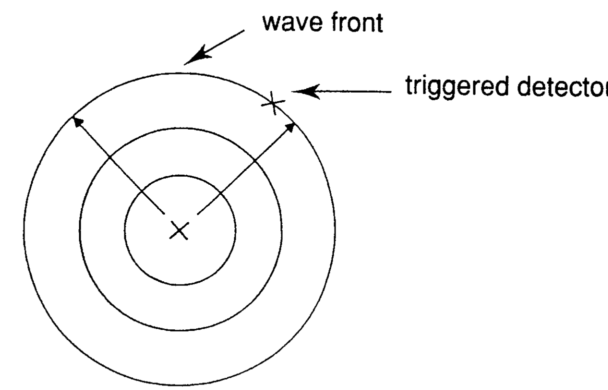

Figure 2.

by the formalism of quantum mechanics itself, along with entirely plausible assumptions excluding action at a distance ("locality") and about the reality (or definiteness) of a physical quantity independent of our choice to observe it. Their argument has been lucidly discussed by Shimony (1978). We do not consider the original EPR thought experiment here. For pedagogical purposes, there is a simpler one due to Bohm (1951). The EPR paper did not offer any alternative theory to quantum mechanics, nor did it mention hidden variables. Nevertheless, the additional parameters that would be necessary to give a complete specification of the state of a system have subsequently come to be referred to as "hidden variables" and any theory encompassing such parameters as a "hidden-variables theory."

Figure 3 is a schematic representation of Bohm's thought experiment. At the center is a source (such as an atom) which decays and emits two electrons (or photons)[1] in opposite directions as indicated in the figure. For simplicity of discussion, we assume that the spin of the atom ("source") is zero, both before it emits the two "electrons" and after as well.[2] Conservation of angular momentum then requires that the spin of electron 1 must be oppositely directed to the spin of electron 2. That is, if the first electron is observed to have spin "up" in some direction, then the second electron's spin, if observed along that same direction, would be found to have spin

[1]Most of those experiments actually performed to date have used photons. Other quantum systems (such as electrons, protons and kaons) have also been employed (or at least proposed). I use the term 'electrons' here only because the reader may have an easier time picturing what is going on.

[2]For the present introductory discussion it does no real harm for the reader to picture the spin of an atomic system as one would the spin of a ball or of a planet about an axis through its center.

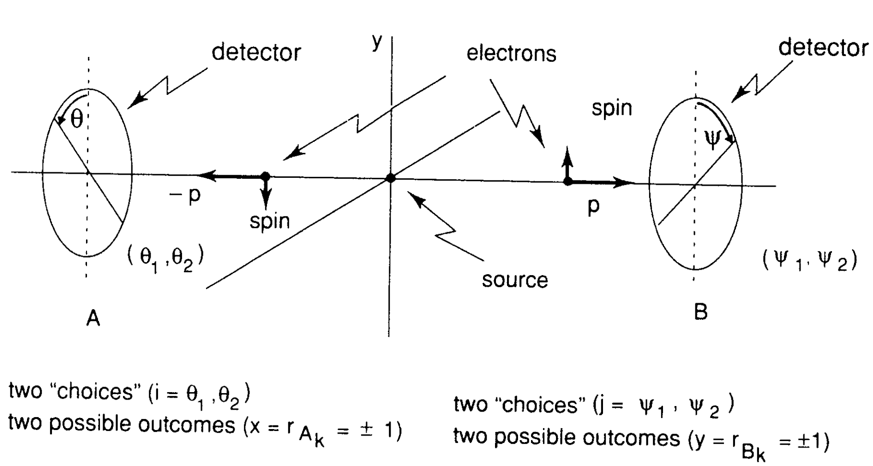

Figure 3.

"down." We choose units such that the observed spin of the electron in any given direction is either $+1$ or -1.[3] At stations A and B there are instruments (labeled "detector" in the diagram) which can be set to measure the spin of an electron along an axis transverse to the line of flight of the electrons. In each detector this transverse axis can be chosen or set in either of two orientations: θ_1 or θ_2 at station A, and ψ_1 or ψ_2 at station B.[4] It is an empirical fact that, whenever the spin of an electron is measured along a given axis, one always finds that spin to point *either* "up" $(+1)$ *or* "down" (-1) along that axis, but *never* to have some fractional, intermediate value for its projection along an axis. That is, each individual measurement or observation yields either $+1$ or -1, never any other value.[5]

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[3]The magnitude of the spin of an electron is $\frac{1}{2}\hbar$. Here $\hbar = h/2\pi$, where h is Planck's constant. We *could* choose a system of units in which $\frac{1}{2}\hbar = 1$. Or, equivalently, we can express all spin projections in terms of multiples of $(\frac{1}{2}\hbar)$.

[4]The use of a Greek ψ, here and later in this essay, to denote an angle of orientation should cause no confusion with the use of psi to denote a wavefunction. Not only would the distinction be clear from the context, but in the body of this essay we neither write down any explicit wavefunctions nor use any symbol for them.

[5]This behavior of the measured value of the spin projection of an electron is not peculiar to *this* particular experimental arrangement. It is a feature of nature whenever an electron's spin projection is measured by *any* means.

In summary, then, the experimenter (or observer) at station A has two choices (θ_1 or θ_2) for instrument settings, and for each instrument setting a result (or datum) $+1$ or -1 is possible for the outcome of a measurement. Similarly, at station B the choices are ψ_1 or ψ_2 for each of which an outcome $+1$ or -1 is possible. This experiment, or sequence of observations, can be repeated as many times as we wish. Each repetition is a run, which we label with an index k, where $k = 1, 2, \ldots N$, with N being the total number of runs. Let us denote by r_{A_k} the result at station A for the k^{th} run. The quantity r_{B_k} has a similar meaning for station B. It is important to appreciate that, for any given run, r_{A_k} can have *only* the value $+1$ or -1 (no other), and similarly for r_{B_k}. For the probability (or distribution) of the outcome r_{A_k} (at A when the setting is θ) and r_{B_k} (at B when the setting is ψ), we use a notation suggested by David Mermin:

$$p^{AB}(r_{A_k}, r_{B_k}|\theta,\psi). \tag{1}$$

More generally, $p^{AB}(x,y|i,j)$ represents the joint distribution of obtaining a result (or outcome) x at station A when the setting (or parameter) i has been chosen by the experimenter at A, while the result y obtains at station B when setting j has been chosen there. The spirit of this thought experiment is that the experimenter at station A can make a free choice of the setting (or parameter), i, *independently* of the free choice, j, his colleague makes at B.

2. *Bell's theorem*

Prior to Bell's 1964 paper, the question of whether or not there could exist a deterministic hidden-variables theory with no instantaneous action at a distance seemed incapable of resolution. Of course, no one had succeeded in writing down an empirically adequate example of one. But, that did not prove that one could not exist. After all, if a student fails to solve a difficult homework problem, the reason could be that he or she lacks the wit to do it or, indeed, it could be a problem with no solution. In the absence of a successful deterministic, local, hidden-variables theory, discussion of the *possibility* of such a theory could appear to be little more than idle argument appropriate only for a free Saturday afternoon or for cocktail parties. Bell's paper changed that in a dramatic fashion. The strength of a theorem is inversely proportional to the strength of the assumptions it makes. That is, if you assume a lot and prove a little, no one is particularly impressed. But if you (apparently) assume practically nothing and obtain a remarkable result, that is impressive.

In effect, Bell (1964) argued that determinate (i.e., predetermined prior to the measurement) projections for the spins of the electrons and locality are incompatible with the (spin) correlations predicted by quantum mechanics.

Once his argument had ruled out determinate values, then it also ruled out the possibility of a local, deterministic theory. Subsequently, Bell (1971) gave an argument that did not involve any type of determinism (or even determin-iteness). Nevertheless, for simplicity of presentation here, I take the license of speaking of his proof in terms of the restrictive framework of a deterministic, local theory which could account for the known outcome of a simple (thought) experiment. Here, as previously, 'deterministic' means just that it is in principle possible to specify enough about the system so that the outcome of the experiment could be predicted on an event-by-event basis. Again, 'local' here serves to characterize the absence of instantaneous action at a distance. Such locality would seem to be required by special relativity (figure 4). At this stage of the discussion, locality requires a first signal principle according to which two events separated by a distance l cannot affect each other before a time $t = l/c$ has elapsed, where c is the speed of light. Certainly, such determinism and locality would be granted as unproblematic by anyone inclined toward a classical ontology. The idealized experiment considered by Bell was simple and its "known" outcome not in serious doubt.

If we let λ denote those parameters (the "hidden" variables) necessary to give a complete specification of the state of a system, then, again in Mermin's notation, $p_\lambda^{AB}(x,y|i,j)$ is the corresponding joint probability. Here λ

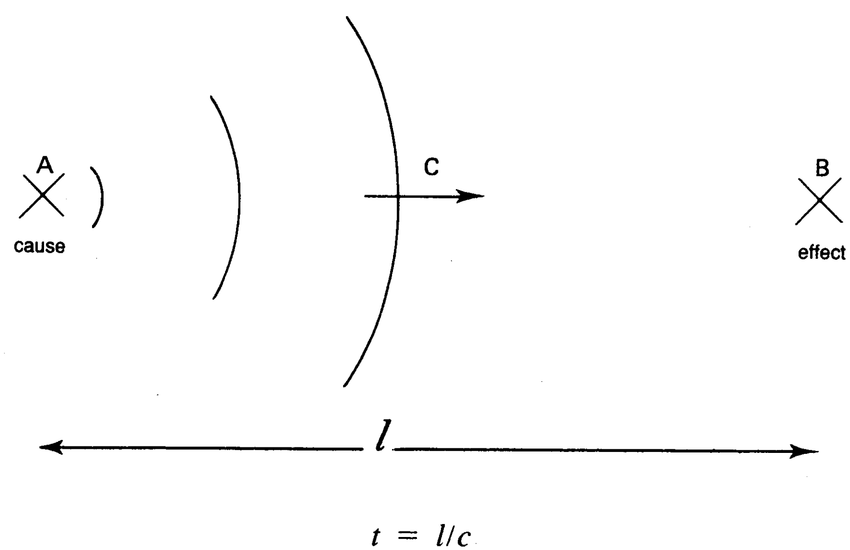

Figure 4.

stands collectively for *all* λ_1, λ_2, λ_3, . . . , necessary for a complete state specification (of the source and of the electrons as they emerge from the source). (Not all of these need be *hidden* variables.[6]) In terms of a density function $\varrho(\lambda)$ which assigns appropriate weighting to the λ, the joint distribution (Eq. [1]) of the experimental results is given as

$$p^{AB}(x,y|i,j) = \int p_\lambda^{AB}(x,y|i,j)\varrho(\lambda)d\lambda. \tag{2}$$

On the basis of locality, Bell argued that these $p_\lambda{}^{AB}$ could be factored as

$$p_\lambda^{AB}(x,y|i,j) = p_\lambda^A(x|i)p_\lambda^B(y|j). \tag{3}$$

From this factorization (or "factorizability," a term due to Fine [1981, 536 ff]) assumption it follows, essentially by algebraic manipulations alone (Clauser and Horne 1974), that the observed distributions for the experiment of figure 3 must be bounded as:

$$\begin{aligned} -1 \le p^{AB}(++|\theta_1,\psi_1) + p^{AB}(++|\theta_1,\psi_2) + p^{AB}(++|\theta_2,\psi_2) \\ - p^{AB}(++|\theta_2,\psi_1) - p^A(+|\theta_1) - p^B(+|\psi_2) \le 0. \end{aligned} \tag{4}$$

Here $p^A(+|\theta_1)$ is the probability of observing an electron with spin up along the θ_1 direction at station A irrespective of what the spin measurement outcome is at station B.[7] The term $p^B(+|\psi_2)$ has a similar meaning for station B.

In fact, the actual experiment (in a real laboratory with real equipment) is much more difficult to do than my rather glib characterization in figure 3 might lead one to expect. A detailed discussion of the experimental situation can be found in the comprehensive review article by Clauser and Shimony (1978) and in Redhead (1987b). There also exists a general, less technical review by Shimony (1988). Such experiments have been carried out, some of the latest and most convincing being those by Aspect, Grangier, Dalibard, and Roger (1981, 1982) in Paris. The empirical results are representable, well within the limits of experimental error, by the simple distributions:[8]

[6]In all of the papers in the volume (*except* Redhead's), this λ can include the quantum-mechanical state-vector specification (often denoted by Ψ).

[7]The *marginal* $p^A(+|\theta_1)$ is recovered from the $p^{AB}(++|\theta_1,\psi)$ and $p^{AB}(+-|\theta_1,\psi)$ as

$$p^A(+|\theta_1) = p^{AB}(++|\theta_1,\psi) + p^{AB}(+-|\theta_1,\psi).$$

That is, we sum over *both* possible outcomes at station B. The careful reader may wonder why the left side of this equation is not written as $p^A(+|\theta_1,\psi)$. We shall discuss this point below (see Eq. [8]). For now let us simply state that this is an *empirical* fact about the marginals of this experiment. This can also be seen to follow from Eqs. (5) below.

[8]It is also a fact that the formalism of quantum mechanics yields the results of Eqs. (5). However, that has nothing to do with a discussion of the significance of the Bell inequality. For our purposes now, we can take Eqs. (5) as phenomenological fits to the data—discovered perhaps by a modern-day Ptolemy! For the present discussion, they need not be considered as having been derived from some theory.

$$p^{AB}(++ \mid \theta, \psi) = p^{AB}(-- \mid \theta, \psi) = \frac{1}{2}\sin^2\left(\frac{\theta-\psi}{2}\right), \qquad (5a)$$

$$p^{AB}(+- \mid \theta, \psi) = p^{AB}(-+ \mid \theta, \psi) = \frac{1}{2}\cos^2\left(\frac{\theta-\psi}{2}\right), \qquad (5b)$$

$$p^{A}(+ \mid \theta) = p^{B}(+ \mid \psi) = \frac{1}{2}. \qquad (5c)$$

Although we shall return to Eqs. (4) and (5) in more detail below, one can already see the crux of the conflict by choosing, for example,

$$\theta_1 = 60°, \theta_2 = 180°, \psi_1 = 0, \psi_2 = 120° \qquad (6)$$

in Eq. (4) and then using the *empirical* distributions of Eqs. (5) to obtain[9]

$$-\frac{1}{8} \geq 0. \qquad (7)$$

The logical skeleton of the argument is that the assumptions of locality and determinism, plus the actual experimentally observed distributions of the real world, have produced the contradiction of Eq. (7). Although one can, in principle, attempt to undermine the empirical leg of the triad upon which this argument rests (cf. Clauser and Shimony 1978), each successive experiment forecloses more such possible loopholes and makes such a line of attack ever less plausible. So, the arrow of *modus tollens* appears more reasonably directed at the assumptions of locality and/or determinism. We have purposely not gone into the details of the argument by which Bell passed from Eq. (3) to the (Bell) inequality of Eq. (4) because we want to focus on the logical structure of the argument. In the appendix to this essay, the reader can find a simple proof of a contradiction like Eq. (7). So, Bell's remarkable result, or theorem, is that no deterministic, local hidden-variables theory can account for the empirical result of the experiment. It is worth emphasizing that these types of correlations are a pervasive feature of the quantum world. They are not peculiar to the Bohm-EPR class of experiments alone. However, the Bohm-EPR configuration is in a sense the "simplest" one yet known which exhibits these "mysterious" quantum correlations.

[9]That is, if, for the sum of the six distributions in the middle term of the inequality of Eq. (4), the expressions of Eqs. (5) are used with the values specified in Eq. (6), then these six terms sum to −9/8. Thus, Eq. (4) becomes

$$-1 \leq -\tfrac{9}{8} \leq 0,$$

which implies Eq. (7).

Let me stress two points here. First, Bell never wrote down a single local, deterministic theory. Rather, he proved, without ever having to consider any dynamical details, that *no* such theory can in principle exist. The entire class was killed at a stroke—a classic "no-go" theorem. Second, Bell's theorem really depends in no way upon quantum mechanics. It refutes a whole category of (essentially) classical theories without ever mentioning quantum mechanics. And it turns out that the experimental results not only refute the class of local, deterministic theories but also agree with the predictions of quantum mechanics. (That is, a straightforward application of the rules of quantum mechanics does lead to the results of Eqs. [5].) Abner Shimony (1984b, 35) has appropriately given the name "experimental metaphysics" to this type of definitive empirical resolution of what appears to be a metaphysical question.

3. *Some distinctions*

In my presentation thus far, I have been rather cavalier in oversimplifying the issues and in conflating terms that must be carefully distinguished. So I now turn to the purpose of the subsequent papers in this volume and to some of the work that the authors have done in recent years. Today, when one looks back at Bell's original paper and at some of the early responses to it, one is struck by at least two facts. First, the paper contains a modicum of mathematical formalism. Depending upon one's level of mathematical sophistication, the proof may not be immediately transparent and one can wonder whether something has gone awry in those pages and symbols. After all, the result is *so* remarkable: it forces us to face indeterminism and/or nonlocality *in principle*. Could the proof be flawed? As often happens with great discoveries, proofs are subsequently fashioned which make the important result seem almost self-evident. Bell's theorem was no exception. Eventually, there were picture proofs and nonmathematical discussions (d'Espagnat 1979; Mermin 1981a, 1985) of Bell's result and of the quantum-mechanical riddles it makes us face. While such discussions are nontechnical, they can remain rather long and involved. The reader's eyes may glaze over before the end. However, if one is willing to pay the price of a little algebra—really, only about six lines of arithmetic—one can immediately go from Bell-type premises and a requirement of empirical adequacy to a contradiction like $1 > 2$ (Stapp 1971, 1979; Redhead 1987a). The mathematics is *so* simple and brief you are certain no error has been made. You think you understand it all! (The details of such a proof are given in an appendix to this paper.)

So then, first, the formalities or manipulations in the proofs were greatly simplified. But then, the second, and in many ways more difficult, phase began—unpacking the assumptions and the meanings of the terms used in these proofs and coming to some understanding of just what the implications are. This is a job that philosophers are particularly well equipped to do. The terms 'reality', 'determinism', and 'causality' cannot be used interchangeably and one must be especially careful to distinguish between locality and separability. Perhaps a few sketchy definitions will help for a start:[10]

reality – existence of an objective, observer-independent world (often closely related to determinate values)
determinism – sufficient information at t_o allows prediction of a specific result at a later time t
causality – a specific preceding event (or "cause") for every effect — a concept familiar from prequantum, classical theories
locality – no influence transmitted faster than light
separability – spatially separated systems always have independently definable properties and existence (and these properties exhaust the description of any system made up of these subsystems).

Arthur Fine (1984a, 1984b) and Don Howard (1985, 1987) have provided a useful perspective for several of these issues by their careful and enlightening historical reconstructions of Einstein's views on locality and separability, bringing out essential differences here between Einstein and Bohr. Henry Folse (1985), Don Howard (1986), and Dugald Murdoch (1987) have done similar work in reconstructing Bohr's philosophy of science. Furthermore, as we indicated previously in Eq. (3), a crucial mathematical step in the usual proof of Bell's theorem is the factorization of a certain expression for joint probabilities. A long debate has arisen as to the physical warrant for this step. This factorizability (or "Bell" locality or statistical independence) is not implied by the first signal principle of relativity ("Einstein" locality). Michael Redhead (1983) and Linda Wessels (1985) have analyzed in detail the assumptions made in various proofs of Bell's theorem. (Actually, there are by now many similar results that are generically referred to as "Bell theorems.") In an important analysis, Jon Jarrett (1984) pointed out that the Bell locality (or factorizability) used in the proofs of these theorems is the logical conjunction of two other conditions, violation of either of which leads to some form of nonlocality. Abner Shimony (1984a) has termed these violations controllable and uncontrollable nonlocality. The first would violate the first signal principle of special relativity, but the second is innocuous in this regard. Since quantum mechanics exhibits only uncontrollable nonlocality, special relativity and quantum mechanics seem capable of a peaceful coexistence (Shimony 1978; Redhead 1983, 1987b).

[10]In the present context, the term 'determinism' is usually predicated of a *theory*, as in a *deterministic* theory. In quantum field theory, 'causality' is used in a sense rather different from (but related to) the classical cause-effect one. (See Cushing, 1986, for a fuller discussion of the meaning of the term 'causality' in modern theoretical physics.) The reader should be warned that the terms 'locality' and 'separability' are the most problematic as far as universally-agreed-upon definitions are concerned. The ones I give here alert the reader to a distinction between these terms. However, each author below must be checked carefully for his or her own precise use of these terms. It is also true historically that the evolution of an explicit distinction between those two terms was a long time in coming. (See Howard [1985] and Folse [this volume] for careful discussions of this issue.) Furthermore, we distinguish among different types of locality and nonlocality. Finally, d'Espagnat (1984) treats the issues of reality and of separability carefully and at great length.

Let me summarize these important distinctions that Jarrett (1984) has made. By "*Jarrett locality*" (or what Shimony [1984a] terms "*parameter independence*"), we mean that the single distribution for outcome x at station A (given the state specification λ) is independent of the choice j made at station B:

$$\text{(I)}\quad p_\lambda^A(x|i,j) = p_\lambda^A(x|i). \tag{8}$$

A similar condition follows for $p_\lambda^B(y|i,j)$. If this condition is respected (as it is, for instance, by quantum mechanics [Shimony 1984a]), then the experimental determination of such a distribution cannot be used to send a signal from station B to station A (e.g., to transmit information faster than the speed of light). However, if condition (8) were violated, then such a signal could be sent[11] and Shimony has referred to such a circumstance as "*controllable nonlocality.*" Controllable nonlocality (which obtains if Eq. [8] is violated) would produce a conflict with the first signal principle of special relativity. By "*Jarrett completeness*" (or what Shimony terms "*outcome independence*"), we mean that the single distribution for outcome x at station A (given the state specification λ) is independent of the measurement outcome y at station B:

$$\text{(II)}\quad p_\lambda^A(x|i,j,y) = p_\lambda^A(x|i,j). \tag{9}$$

If this condition fails (as it does for quantum mechanics) while (I) holds, then an explanation in terms of (past) common causes is not possible (cf. Shimony 1984a and Jarrett, this volume). However, the violation of Eq. (9) does *not* allow the measurement of such a distribution to be used for superluminal

[11]It is important to note that the *observable* (or experimentally relevant) distributions are the *integrated* $p^A(x|i,j)$ or $p^A(x|i)$ (cf. Eq. [2]), *not* the $p_\lambda^A(x|i,j)$ or the $p_\lambda^A(x|i)$ of Eq. (8). The point is that not all of the λ need be under the control of the experimenter (even in principle). Even if the $p_\lambda^A(x|i,j)$ did depend upon j, it does not follow that the integrated quantities $\int p_\lambda^A(x|i,j)\varrho(\lambda)d\lambda$ will still *necessarily* depend upon j. It is only these integrated quantities which are directly related to experiment. Jarrett's argument that a violation of Eq. (8) would allow signaling assumes an idealized situation (cf. his paper in this volume).

signaling. For this reason, Shimony has termed a violation of condition (9) "*uncontrollable nonlocality*." Such uncontrollable nonlocality produces no conflict with the first signal principle of special relativity. (Shimony [1984a] gives a general proof that the uncontrollable nonlocality of quantum mechanics does not produce a violation of the first signal principle of special relativity. This is what he calls "peaceful coexistence" between quantum mechanics and special relativity. See also Shimony [1986] for further discussion of these implications).

The importance of Jarrett's (1984) clarifying analysis in terms of the conditions (I) and (II) of Eqs. (8) and (9) respectively is that (I) and (II) *together* imply the factorizability of Eq. (3) and hence allow derivation of a Bell inequality. In Mermin's notation, Jarrett's argument can be summarized as follows. From the very meanings of joint and conditional probabilities, it follows that

$$p_\lambda^{AB}(x,y|i,j) = p_\lambda^A(x|i,j,y)p_\lambda^B(y|i,j). \tag{10}$$

If we now assume that both (I) and (II) hold, then Eq. (10) reduces to the factorizability condition of Eq. (3). Thus, if we can block either move (8) or move (9), we can block the standard derivation of a Bell-type inequality.

Another insightful observation about the meaning of the Bell inequality was made by Fine (1982b). He argued that Bell inequalities of the type in Eq. (4) above are the necessary and sufficient conditions for the existence of a deterministic hidden-variables model which will produce the joint distributions for the Bohm (EPR) experiment of figure 3. But the existence of such a complete set of state variables λ is equivalent to a common-cause explanation (in the common past of the parts of the system to be observed) for these distributions or experimental outcomes. Knowing that there is such an empirically applicable test for the possibility of a common-cause explanation will prove important for the discussions which follow in subsequent papers in this volume.

4. *Philosophical implications*

We can now ask just what the implications of all of this are for our view of the physical world. Thus far we have pointed out certain restrictions on allowable world views (or representations of reality) that are demanded by quantitative relations (the Bell inequalities) containing only empirically measurable distributions of experimental results. In a sense, the tone has been negative since we have stressed what type of theories or explanations are *not* possible. Must we, for example, abandon belief in an observer-independent reality? Or, as David Mermin has put it, "Is the moon there when nobody looks?" We have shown what cannot work rather than exploring some theory or explanatory framework that is successful in reproducing the results of experiment. Of course, we *do* have an empirically adequate theoretical framework within which to organize the observational data—namely, quantum mechanics. However, this enormously empirically successful theory has difficult interpretative problems associated with it. Henry Stapp (1979, 14) makes a point similar to Mermin's when he characterizes our immediate reaction to a literal acceptance of some of the more extreme interpretations of quantum mechanics:

> One objection to this view is that it seems excessively anthropocentric, at least if consciousness is reserved for human beings and higher creatures. Before the appearance of such creatures the world would be synthesizing endless superposed possibilities, with nothing actual or real, waiting for the first conscious creature to occur among the possibilities. Then a gigantic collapse would occur. Similarly, the Martian landscape would be nothing but superimposed possibilities until Mariner landed and some observer in Houston viewed his TV screen. Then suddenly the rocks and boulders would all snap into their observed places. This view seems to assign a role to such observers that is out of proportion to their place in the world they create.

That is, our most successful theory of processes at the microlevel, namely quantum mechanics, poses serious problems for scientific realism (which requires roughly and at a minimum that our scientific theories are to be taken as giving us literally true descriptions of the world).[12]

Bas van Fraassen (1982a; this volume) has argued that the experimental violation of the Bell inequality tells against scientific realism. That is, if scientific realism does not work at the microlevel, then it cannot be generally valid. In a provocative article, Asher Peres (1985) has posed yet another quantum paradox "as a challenge to those physicists who claim that they are realists" (p. 201). His conclusion at the end of that article (p. 205) is that "Any attempt to inject realism in physical theory is bound to lead to inconsistencies."[13]

At the other end of the spectrum from van Fraassen (1980) or Peres on views of scientific realism, we find Ernan McMullin (1984) who points to the great success *structural* theories have enjoyed in several sciences (such as chemistry, astrophysics, geology, and genetics) in taking a starkly realistic

[12]John Bell (1987) has recently published his own collected essays on the philosophy of quantum mechanics, and on interpretations of it other than the standard Copenhagen one.

[13]At the 1986 Quantum Measurement Theory Conference (Greenberger 1986) in New York City, I mentioned to Peres that his position appeared to be an instrumentalist one. He replied with no apparent discomfort that others had told him that before. For a physicist's statement on an instrumentalist interpretation of quantum mechanics, see Peres (1988).

view of the entities contained in those theories. It is in regard to the interpretation of the ontologies underlying *mechanical* theories (whether classical or quantum) that problems most often arise (McMullin 1989). McMullin recommends treating these theories as a special class and considers as inappropriate the demand for a realistic interpretation of *force* or *field* that would be unproblematic for *molecule* or *gene* or *galaxy*. He is willing to put aside for the present certain difficult questions of a realistic interpretation for mechanics (that is, classical mechanics, quantum mechanics, quantum field theory—all of what would seem to many to be the foundations of physics): "Because of its many special features, mechanics is quite unsuitable as a paradigm of science generally, though philosophers are wont to overlook this" (1984, 10). Rather than being *the* paradigm of natural science, much of physics becomes, at least in the context of this issue, an anomaly. It appears that McMullin restricts consideration to cases that satisfy, in some broad sense, Newton's Rule III of Reasoning in Philosophy in Book III of the *Principia* (Newton [1726] 1934):[14] "The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever." This rule is often taken as saying that we may extrapolate general features of the macroworld to the microworld. To some, McMullin's circumscription of mechanics may be too costly a move to make on behalf of scientific realism. Somewhere between van Fraassen and McMullin, we find Heisenberg (1958, 185) with his suggestion that we must admit a new class of physical entity into our theories: *potentia* (Shimony 1978, 1986; Stapp 1979, 1985a). Bell (1984) has made a similar suggestion in speaking of the "*be*ables" of quantum field theory.

One of the most interesting philosophical questions, perhaps, concerns the relations among empirical adequacy, explanation, and understanding for quantum phenomena. Are explanation and understanding really possible when a detailed causal explanation is in principle impossible? In Bas van Fraassen's (1985) terms, are the EPR correlations a mystery? Paul Teller has suggested that relational properties of physical objects may not simply supervene wholly upon nonrelational properties of localizable individuals, but that a type of "*relational holism*" is essential in which the objects have inherent relations among themselves. He claims that this is "a holism we can understand" (Teller 1986, 73). But is it? A central issue is whether or not we can truly *understand* such descriptions of our world. These problems are forced upon us, of course, when we take the present formulation of quantum mechanics as exactly correct, needing no modification.

[14]I thank André Goddu for this insight, although he is not responsible for my interpretation of it.

In the same vein, we can even ask whether all the *desiderata*, which we may want in a theory that accords with the phenomena of the real world, can be mutually compatible. Peres and Zurek (1982) set up a triad (figure 5) involving the three "wishes" of determinism, verifiability, and universality and argue that no theory can, *even in principle*, satisfy simultaneously all three demands. (By "verifiability" here they mean the freedom of choice of an observer or experimenter to fix a given setting on, or orientation of, a measuring device to test the predictions of a theory.) We can have at best just any two of the three. In the end, quantum mechanics may be the best theory it is possible to have.

We might question the value of discussing the implications that essentially *nonrelativistic* quantum mechanics (say, the usual Schrödinger equation) has for such issues, since the more complete (and problematic) theory in use today is relativistic quantum field theory. I have argued elsewhere in some detail (Cushing 1988) that the root of quantum paradoxes is the superposition principle and *that* remains in any quantum theory. Michael Redhead and Paul

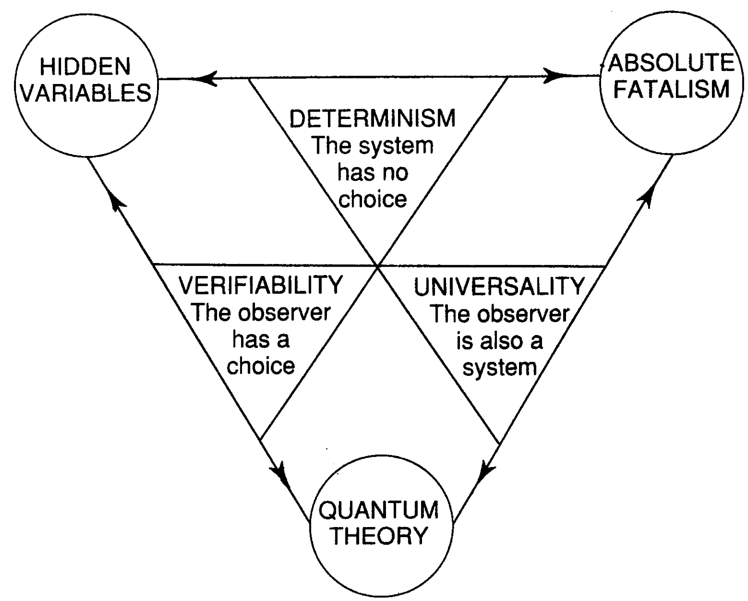

Figure 5.

Teller (cf. Brown and Harré 1988) do believe that quantum field theory introduces new philosophical problems which must now be faced. These would, of course, be in addition to, and not solutions of, the interpretative difficulties already presented here.

This introductory essay may at least establish a *prima facie* case for the relevance of quantum mechanics to general philosophical issues related to epistemology and ontology. There are serious problems here, not simply questions of mathematical formalism. Hence, the rationale for this volume. Several years ago John Bell (1975, 98) made an observation about the understanding of our world which quantum theory gives us:

> The continuing dispute about quantum measurement theory is not between people who disagree on the results of simple mathematical manipulations. Nor is it between people with different ideas about the actual practicality of measuring arbitrarily complicated observables. It is between people who view with different degrees of concern or complacency the following fact: so long as the wave packet reduction[15] is an essential component, and so long as we do not know exactly when and how it takes over from the Schrödinger equation, we do not have an exact and unambiguous formulation of our most fundamental physical theory.

In our search for a new understanding, we face the challenge characterized by Costa De Beauregard (1983, 515–516) in this way:

> Hard paradoxes . . . are resolved only by producing a new and adequate paradigm, in Kuhn's words. In physics, this implies the production of a new mathematical recipe (e.g., Copernicus's heliocentrism, or Newton's inverse-square law) and tailoring an explanatory discourse exactly fitting the mathematics (e.g., Einstein's interpretation of the Lorentz-Poincaré formulas; or still better, Minkowski's).
>
> This sort of "explanation" is usually felt (and often for a long time) as itself paradoxical. Newton's action at a distance, Einstein's "reciprocal" interpretation of the Lorentz contraction, have very often been deemed "hardly explanations at all."

[15]The reader unfamiliar with the expression "wave packet [or state] reduction" can take (an example of) this to be the following. A system in a state ψ, which is a linear superposition of eigenstates (φ_j) of an operator (observable) *A prior to* a measurement of that observable, is in a specific eigenstate (say φ_f) immediately *after* the measurement. The (discontinuous) "transition"

$$\psi = c_1\varphi_1 + c_2\varphi_2 + c_3\varphi_3 + \ldots \rightarrow \psi' = \varphi_f$$

is (an example of) state reduction. It is commonly the case that $\psi' \neq \psi$. This reduction is *not* governed by the Schrödinger equation. See d'Espagnat (1979) for a full discussion of reduction.

> What occurs in the "paradox and paradigm" peripateia (or, in Kuhn's words, in a "scientific revolution") is a victory of formalism over modelism. In the EPR case we do have, since many years, the formalism. We are at home with it for performing calculations, but not yet for viewing our world, and our relation to it.

The papers in this volume are attempts to fashion an explanatory discourse with a view to producing an understandable view of our world. The ultimate goal is to construct a framework that is empirically adequate, that explains the outcomes of our observations, and that finally produces in us a sense of understanding how the world can be the way it is. These are three linked but distinct goals. It remains an open question whether all of these are simultaneously attainable.

5. *Conference papers*

This essay has been an informal and somewhat loose summary of the situation prior to the Notre Dame Conference in October of 1987. Let me conclude with come comments on the remaining papers in this volume and on the interrelations among them.

Abner Shimony discusses the various components of a consistent world-view and assesses the general implications of quantum theory for our world-view *provided* we take the present formulation of quantum mechanics to be a complete one. His paper sets the stage for the detailed analyses of the subsequent papers. Shimony argues against a (mere) instrumentalist interpretation of quantum mechanics and in favor of a realist interpretation. As one possible means of producing a coherent realist picture (to "close the circle" between metaphysics and epistemology), he suggests that the formalism of quantum mechanics may have to be modified (in a way which he specifies). This could serve as an example of adjusting a physical theory to meet certain metaphysical requirements.

David Mermin (1981a) has given a clear, nontechnical representation of the essential features of the Bohm-EPR "experiment." (This is reprinted as an appendix to his conference paper.) He shows convincingly that the emitted "particles" cannot simply be considered as individually carrying the information that determines the outcome (i.e., which color light flashes) once the particles interact with a particular detector that has been set. He argues that the particles cannot possess definite values for the measured quantities (prior to the measurement). In his conference paper, Mermin then goes on to give the novice an acute sense of the mystery of these correlations by suggesting the (apparently obvious or at least innocuous) validity of his "Strong Baseball

Principle." The reader feels the rug has been pulled out from under him or her when the "data" refute this principle.

Jon Jarrett in his paper also uses a "Mermin device" for purposes of illustration. He first defines the general framework of *local realism* and shows that it could not survive the "empirical" results which would be produced by a (real) laboratory version of a Mermin device. He then discusses his important (and by now well-known) result that locality (parameter independence) and completeness (outcome independence) together imply factorizability (which in turn implies a Bell inequality). Jarrett draws out more of the philosophical implications of the Bell-EPR analyses and experiments.

Linda Wessels spells out in careful detail the assumptions (both explicit and implicit) that are made in derivations of a Bell inequality. Given the failure of that inequality, she examines which of those assumptions most likely need to be given up. In the end Wessels singles out three of these assumptions, one or more of which is probably the culprit.

Bas van Fraassen argues that the outcomes of Bell-EPR type experiments cannot, even in principle, be given a common-cause explanation. He discusses in detail the notion of a causal mechanism and the reasons some philosophers have given for requiring such in any explanation that allows us to understand correlations between events (or "outcomes"). In an appendix to his paper, van Fraassen analyzes various types of explanation that can be employed to account for statistical correlations. Since the concept of a common cause figures prominently in this and in several of the later papers in this volume, let me here indicate the basic idea involved. Suppose that, for some sample, the outcomes x and y are *not* statistically independent,

$$p(x,y) \neq p(x)\, p(y). \tag{11}$$

Suppose further, though, that when we condition (or restrict) the sample upon another factor z then the correlation does factorize as

$$p(x,y|z) = p(x|z)\, p(y|z). \tag{12}$$

In such a case, z is said to be the *common cause* of x and y.

In Jeremy Butterfield's paper, as in Michael Redhead's which follows, the reader encounters the concept of *screening-off*. The basic idea is this. If two events x and y are correlated, as in Eq. (11) above, but there exists a third event z such that conditionalization upon it produces stochastic independence, as in Eq. (12), then z "screens off" x and y. Butterfield focuses on the principle of the common cause according to which, if two events are correlated and one does not cause the other, then a common cause for both events can be found in their common past. His contention is that a principle of common cause and special relativity are *both* required to obtain Jarrett locality and/or Jarrett completeness. He presents locality and completeness each as

screening-off conditions. Butterfield shows that, even under the weaker assumption of an extended domain for the common past of the state of a system, the Bell inequality follows. That is, even this extended (weakened) version of the principle of the common cause is refuted by the EPR correlations.

Michael Redhead introduces the criterion of *robustness* as a necessary condition for events to be connected even by stochastic causality (by which we mean that Eq. [12] can be made to obtain).[16] The motivation for such a requirement is that the events (or outcomes) x and y should be stable against some class of perturbations affecting the conditions surrounding x or y. Explicit calculation shows that the Bohm-EPR correlation (for the experiment of figure 3 above) is *not* robust. This result and special relativity together allow Redhead to rule out direct causal links and/or common causes as accounting for these correlations. He suggests using Shimony's expression, "passion at a distance," for this property of the quantum-mechanical state responsible for the correlation.

A central issue on which several of our authors divide is whether or not EPR-type correlations stand in need of an explanation, as opposed to being simply irreducible brute facts about nature. Henry Stapp begins by considering, and by and large rejecting, several possible ontologies currently on offer as underlying quantum phenomena. But he does see part of the explanation of quantum correlations as lying in the nonlocal connections of those phenomena. Stapp's thesis is that the quantum phenomena themselves require nonlocal influences in nature. Much of his argument is concerned with justifying assumptions (free from counterfactual definiteness) that can warrant this conclusion. Here counterfactual definiteness refers to a claim that the actually occurring outcome would still result under certain different conditions, had they instead obtained. Stapp's requirement is weakened from what *would* still occur in wing A when a change is made in wing B to what *could* still occur under those conditions.

Arthur Fine begins with a summary of his work on the conditions under which a common-cause explanation is (in principle) possible for a given set of data. Like van Fraassen, he forecloses the possibility of a causal explanation for the EPR correlations. He also comments upon the effectiveness of "would" and "could" distinctions of the type made in Mermin's Baseball Principle and in Stapp's analysis (respectively). But, then Fine turns the tables on the reader and makes the challenging suggestion that, in a truly indeter-

[16]Stochastic (local) hidden-variables theories have the advantage of avoiding any assumption of determinism (and this is desirable since deterministic local hidden-variables theories have been refuted by experiment). But, a Bell inequality follows for these stochastic theories as well (because only the factorizability of the joint probability is required for that; cf. Eq. [3]). Redhead's considerations hold for deterministic theories too.

ministic world, such correlations stand in no more need of an explanation than does a random string of outcomes of measurements made at a single (fixed) location. This is a dramatic and radical move that takes the correlations as primitive givens which may themselves be employed as basic elements in explaining other phenomena.

R. I. G. Hughes is one of those who feel that the EPR correlations *do* require an explanation. He proposes a "*structural*" explanation based on the terms used in the (mathematical) models which the general theory of the phenomena (i.e., quantum mechanics) employs. Hughes analyzes the (mathematical) components common to a general quantum-mechanical framework and then describes the EPR experiments in terms of four "theses" he has identified. He also discusses possible objections to the structural explanation sketched there.

Paul Teller's basic thesis is, like Arthur Fine's, a radical one, but in a very different direction from Fine's. Teller offers an explanation for (or perhaps better, a lesson to be drawn from) the EPR correlations in terms of an *ontological* relational holism. He takes the nonseparability of the quantum-mechanical formalism quite seriously and interprets it as a warrant for a modified ontology in which not all relational properties need simply "supervene" (or be impressed upon) those of independently specifiable individual entities. Teller argues for the need for such a move on the basis of an analysis of the role relativity plays in proofs of a Bell inequality. For him, the source of our difficulty in understanding the implications of the failure of the Bell inequalities is our implicit (perhaps unconsciously made) assumption of *particularism* (roughly, separability in the EPR context). Teller trades in particularism for relational holism to rid us of the interpretative conflicts we otherwise face.

Don Howard provides careful statements of the separability principle and of the locality principle, and then offers a proof of the Bell inequality based on locality and separability conditions. He examines the role these concepts played in Einstein's view of physical reality. His paper indicates that the tension between quantum theory and relativity becomes more severe when the general, rather than just the special, theory of relativity is considered. In fact, Howard questions the consistency of quantum field theory and of quantum mechanics itself as a *fundamental* theory.

Henry Folse's essay is a retrospective study of how Bohr's ideas (and to some extent, Einstein's as well) relate to the discussion surrounding the EPR correlations and Bell's theorem. He sketches Bohr's position on realism and argues that Bohr should not be seen as a positivist. Folse shows us that Bohr appreciated early on the conflict between a space-time description of physical phenomena and quantum reality. We also learn the philosophical basis of the differences between Bohr and Einstein, with separability being a key issue.

This type of philosophically sensitive analysis of the writings of Bohr and of Einstein on questions associated with quantum theory and physical reality has recently engaged the attention of several philosophers of science (Fine 1986; Folse 1985, 1987; Howard 1985; Krips 1987; Murdoch 1987).

The volume concludes with Ernan McMullin providing some perspective, from the history of science, on how scientific practice and regulative principles (to use current terminology) have in the past interacted to produce an explanation within an accepted (but possibly evolving) worldview. He shows that there are precedents in science for the present situation in which empirical adequacy obtains without any accompanying causal explanation. McMullin focuses on the history of planetary motion and argues that prior metaphysical commitments (or constraints) were responsible, at least in part, for the sense of bafflement which existed until certain of this baggage was jettisoned. We see that there are precedents for the type of crisis resolution suggested by, say, the dramatic moves made by Teller or by Howard in their essays.

APPENDIX

In order to illustrate the algebraic simplicity of some later proofs of Bell-type inequalities and the gradual realization and unpacking of the significance of the assumptions made, let us consider Stapp's 1971 proof. Although algebraically even simpler proofs are now available (e.g., Peres 1978; Redhead 1987a), Stapp's has the virtue of being one of the earliest of these "obvious" proofs and it has had an interesting subsequent history of its own. In accord with the variables employed in Eq. (1) of this introduction, let us agree upon the notation:

$r_{A_k}(\theta,\psi)$—the result (or outcome) at station A for the k^{th} run ($k = 1, 2, 3, \ldots, N$) when the parameter ("choice") θ has been selected at station A and the parameter ("choice") ψ has been selected at station B.

$r_{B_k}(\theta,\psi)$—the result (or outcome) at station B for the k^{th} run ($k = 1, 2, 3, \ldots, N$) when the parameter ("choice") θ has been selected at station A and the parameter ("choice") ψ has been selected at station B.

The choice (θ) at A and the choice (ψ) at B are made *independently* of each other. Each choice could be made so late (i.e., just before the "electrons" in figure 3 arrived at their respective detectors at stations A and B) that a light signal (or causal influence) could not travel from A to B (or vice versa) before each detector registered its determination of the spin of the electron passing through it. That is, we have already stated that the first signal principle of special relativity (cf. figure 4 again) is sometimes formulated as the requirement that an event at A cannot produce an effect at B (a distance l away) before a time of at least l/c has elapsed. Such a principle would seem to warrant the "locality" requirements that r_{A_k} can depend only upon the choice θ made at A (but not upon the choice ψ made at B) and, similarly, that r_{B_k} can depend only upon ψ (but not upon θ):[17]

$$r_{A_k}(\theta,\psi) = r_{A_k}(\theta), \tag{A.1}$$

$$r_{B_k}(\theta,\psi) = r_{B_k}(\psi). \tag{A.2}$$

The experimentally determined correlation $\langle r_A r_B \rangle$ is defined as

$$< r_A r_B\,(\theta,\,\psi) > \equiv \frac{1}{N}\sum_{k=1}^{N} r_{A_k} r_{B_k}. \tag{A.3}$$

In terms of the joint distributions (or probabilities) $p^{AB}(r_A, r_B|\theta,\psi)$, this can also be expressed as

$$\langle r_A r_B(\theta,\psi)\rangle = p^{AB}(++|\theta,\psi)(+1)(+1) + p^{AB}(+-|\theta,\psi)(+1)(-1) + p^{AB}(-+|\theta,\psi)(-1)(+1) + p^{AB}(--|\theta,\psi)(-1)(-1). \tag{A.4}$$

With the *empirical* distributions of Eqs. (5), we can express Eq. (A.4) (by means of an elementary trigonometric identity) as

$$\langle r_A r_B(\theta,\psi)\rangle = -\cos(\theta - \psi). \tag{A.5}$$

Stapp's argument is now essentially the following. We have from Eqs. (A.1), (A.2), (A.3) and (A.5) for the choice $\theta = \psi$

$$\frac{1}{N}\sum_{k=1}^{N} r_{A_k}(\theta) r_{B_k}(\theta) = -1. \tag{A.6}$$

Since each r_{A_k} and each r_{B_k} can be only *either* $+1$ *or* -1, Eq. (A.6) can be true if and only if

$$r_{B_k}(\theta) = -r_{A_k}(\theta), \text{ for all } k \text{ (for any given } \theta). \tag{A.7}$$

That is, we can (for a fixed θ) exchange a station index B for a station index A at the price of a minus sign. In simple succession, we then obtain at once the sequence of equalities and inequalities:

[17]The requirements of Eqs. (A.1) and (A.2) are in the spirit of (but *not* identical with) the Jarrett locality (Shimony's parameter independence) of Eq. (8).

$$\left| < r_A r_B\,(\theta,\,\psi_1) > - < r_A r_B\,(\theta,\,\psi_2) > \right|$$

$$\equiv \left| \frac{1}{N}\sum_{k=1}^{N} \left[r_{A_k}(\theta) r_{B_k}(\psi_1) - r_{A_k}(\theta) r_{B_k}(\psi_2) \right] \right|$$

$$= \left| \frac{1}{N}\sum_{k=1}^{N} r_{A_k}(\theta) r_{B_k}(\psi_1) \left[1 - r_{B_k}(\psi_1) r_{B_k}(\psi_2) \right] \right|$$

$$\leq \frac{1}{N}\sum_{k=1}^{N} \left| \left[1 - r_{B_k}(\psi_1) r_{B_k}(\psi_2) \right] \right| = \frac{1}{N}\sum_{k=1}^{N} \left[1 - r_{B_k}(\psi_1) r_{B_k}(\psi_2) \right]$$

$$= \frac{1}{N}\sum_{k=1}^{N} \left[1 + r_{A_k}(\psi_1) r_{B_k}(\psi_2) \right] = 1 + < r_A r_B\,(\psi_1,\psi_2) >.$$

Therefore, we have the general inequality among the correlations

$$|\,\langle r_A r_B(\theta,\psi_1)\rangle - \langle r_A r_B(\theta,\psi_2)\rangle\,| \leq 1 + \langle r_A r_B(\psi_1,\psi_2)\rangle. \tag{A.8}$$

For the experimental correlations of Eq. (A.5), this inequality becomes

$$|\cos(\theta - \psi_1) - \cos(\theta - \psi_2)\,| \leq 1 - \cos(\psi_1 - \psi_2). \tag{A.9}$$

For the choices

$$\theta = 0,\ \psi_1 = 45°,\ \psi = 135°, \tag{A.10}$$

this reduces to

$$\sqrt{2} \leq 1. \tag{A.11}$$

It may seem reasonable to claim that this proof demonstrates that experiment (i.e., the empirical correlations of Eq. [A.5]) requires nonlocality since only locality (but not determinism) is assumed (cf. Eqs. [A.1] and [A.2]). However, Stapp himself (1971) pointed out that statements such as those of Eqs. (A.1) and (A.2) are counterfactual definite ones, since they are equivalent to

$$r_{A_k}(\theta,\psi_1) = r_{A_k}(\theta,\psi_2), \tag{A.12}$$

which is a claim that r_{A_k}, which has some specific numerical value when $\psi = \psi_1$ is chosen at B, *would* have the same value (or outcome) if the choice $\psi = \psi_2$ had instead been made on the k^{th} run. However, there is no way empirically to verify a claim of this sort since the k^{th} run can never be repeated, although, of course, the $(k + 1)^{\text{st}}$ run can be performed. *If* the underlying theory were a deterministic one, then it could serve as a warrant for the statements of Eqs. (A.1) and (A.2).

While there are other difficulties with these (algebraically) ''simple'' proofs of Bell-type inequalities, we do not pursue them here. (See Redhead, 1987a, for an engaging discussion of the subtle difficulties involved in these ''self-evident'' proofs; also Redhead 1987b, 90–96; Clauser and Shimony 1978, pp. 1898–1900.) Our purpose in this example has been to indicate the type of unpacking of assumptions that has followed upon the simplification of the mathematical technicalities of the Bell arguments. Without using either determinism or counterfactual definiteness or any other ideas alien to orthodox quantum theory, Stapp (1985a, 1988a, 1988b, 1988c, and the contribution to this volume) has recently presented arguments that one can deduce from the simple arithmetic contradiction given above (or an equivalent one) the incompatibility of quantum theory with the locality claim that causal influences cannot act outside the forward light cone.