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A REVIEW OF CRITICISMS OF THE QUANTUM MECHANICAL THEORY OF PSI PHENOMENA

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ABSTRACT: This paper reviews criticisms of the quantum mechanical (QM) theory of psi phenomena. Emphasis is given Millar's review of observational theories, Gardner's elementary treatment in *Science and the Paranormal*, and to a review of QM, especially to the various interpretations of QM that have contributed to misunderstandings of the psi theory. Additional attention is given to the dualism problem, Braude's criticism of causal loops, the divergence problem, and to the validity and relevance of Schmeidler's thermistor experiments. A synopsis of the QM theory of psi is presented to point out the central themes of the theory, to clarify the meaning of state vector collapse and observation, and to detail the connection of this theory to the QM theory of consciousness and mechanisms of synaptic functioning. It is shown that the theory as originally posited involves a minimum of postulates that are consistent with physical principles such as Lorentz invariance, the Ehrenfest theorem, and state vector collapse. Moreover, the theory is now supported by extensive experimental results, some of which were predicted.

> WATSON: When I hear you give your reasons, the thing always appears to me so ridiculously simple that I could easily do it myself, though at each successive instance of your reasoning I am baffled, until you explain your process.
>
> HOLMES: Quite so. You see, but you do not observe.
>
> Sir Arthur Conan Doyle, *A Scandal in Bohemia*

Several papers have appeared that have criticized the theory I introduced in which psi phenomena are attributed to quantum mechanical (QM) effects associated with observer-mediated state vector collapse. No single central theme exists for these criticisms; rather, they extend from philosophical issues (e.g., Braude, 1979; Isaacs, 1977) to arguments about the merits of the experimental work that supports the theory (Millar, 1978, 1979). Recently, Martin Gardner contributed a chapter to *Science and the Paranormal* (1982) to criticize this theory of psi phenomena. In addition, several alternative

QM theories have been proposed that suggest criticism of my work. This paper seeks to respond to the several issues arising in these criticisms.

Précis of the Theory

Quantum theory is based on the Schrödinger equation in which the complete description of any physical system is represented by the state vector, a vector in a special space called Hilbert space. The components of the vector in that space give the amplitude of the probability (the amplitude is to be squared to obtain the probability). According to Bohr, this vector is the *complete* representation of the physical system. Because the complete solution is thus a combination of the states represented by this state vector, there exists a disparity between the Schrödinger equation and our practical experience of the physical world. In our practical experience, combined states are never observed as such; only single states are observed. This problem is resolved in quantum theory by interpreting the state vector in terms of probabilities. When a measurement is carried out, the state vector representation is collapsed onto one of the component states (the component state corresponding to the outcome of the measurement). This means that a measurement interaction fundamentally alters the complete representation of the physical system. Even if we do not know what state the system entered on measurement, but only know that a measurement was performed, the representation of that system is altered. Thus, measurement alters the physical system itself.

However, in quantum theory the only characteristic to distinguish a measurement interaction from any other physical interaction is a certain pragmatic consideration. We know, as conscious observers, that one specific result occurred on measurement. When we see the result, we know state vector collapse has occurred. It is this pragmatic consideration that has led to the concept in QM that (conscious) observation causes state vector collapse. Thus, we are led naturally to the possibility that observation can alter physical systems.

The QM theory of psi phenomena is based on the hypothesis that because state vector collapse can be regarded as arising from the interaction of the observer with the observed system, there exists some correlation between the states of physical systems as they occur and the conscious states of the observer. In this theory, consciousness and state vector collapse are two sides of the same thing, being related much the way action and reaction forces are related in Newtonian physics. Details of the theory have been given elsewhere (Walker, 1975, 1979).

Philosophical Criticisms

A number of criticisms of the QM theory of psi have appeared since the theory's introduction in 1972 (Walker, 1973). These criticisms have involved the following points (see, for example, Braude, 1979; Isaacs, 1977; Rao, 1977): (a) There exist questions about whether the theory is monistic or dualistic. (b) The theory does not define observation unambiguously. (c) Causal loops implicit in the concept of retrocognitive PK effects run counter to causality. (d) The divergence problem poses irresolvable problems for the theory. (e) Experimental work points to an unconscious process as being causative of psi whereas the QM theory of psi attributes psi to conscious processes.

Bierman, Houtkooper, and Millar (1981) and I (Walker, 1981) have responded to these criticisms, but our responses bear further clarification:

The duality question. Duality is already a part of physics. This issue is the basis of the measurement problem in QM. But this problem for physics is actually a benefit for parapsychology because it provides a justification for introducing the observer as a causal agent. The use of physics in parapsychology does not mean, however, that we are obliged to resolve problems associated with the formalism of QM. Nevertheless, I have addressed a technical treatment of this problem in previous papers (Walker, 1979, 1983a). I have acknowledged (Walker, 1981) that the theory does incorporate a duality, but that this duality is not one in which a discarnate entity acts to "fetch" psi information or manipulate PK events ad hoc. The dualism enters because "observation" as it is used in quantum theory must have properties that go beyond those that can be represented in terms of material objects interacting by way of force fields (which is the way all of physics describes physical processes). The reason is that the observer is introduced in QM as a way to account for state vector collapse. The state vector is a collection of potential states generated by field interactions among the material objects. As a consequence, something more, something in addition to the interaction of material objects as they have been previously treated in physics, is needed to bring about state vector collapse. The concept of the observer is used in physics to designate this "extra-physical" interaction.

The duality represented by the introduction of the observer is also mirrored in a rather pragmatic characteristic of consciousness; and, of course, it is the presence of consciousness that distinguishes the "observer." Consciousness, at present, cannot be shown to exist in an object by the use of any strictly physical measuring device or instrument. Thus, consciousness has a physically nonmeasurable or nonphysical characteristic. This is a practical distinction to be made between consciousness and other aspects of reality. Thus, this duality is a pragmatic duality in that it depends on the meaning of physical measurement as opposed to the measure of associable quantities (Walker, 1970). The resolution of the measurement problem or the development of a direct procedure to establish the presence of consciousness in an object by direct measurement would remove this dualism. The term *practical dualism* therefore serves to designate the pragmatic nature of the approach I have taken.

I have shown one way to resolve the measurement problem in which the observer functions as a construct. The observer is replaced by a specific type of physical interaction, designated *complete measurement* or *measurement loops* (Walker, 1979, 1983a). This solution to the measurement problem would remove the philosophical "emergence" problem from the concept of the observer, but would retain "consciousness" as a negotiable instrument of reality that arises whenever specific physical interactions—measurement loop interactions—occur.

Whatever the resolution of this aspect of the problem, it is a problem that confronts physics as well as parapsychology. This philosophical problem serves to indicate areas for development of physics, not an argument against the concept of the observer as mediator of psi phenomena in parapsychology.

What is observation? The question "What is observation?" has been raised, particularly by Rao (1977). Braude (1979) has also argued along similar lines that the observation concept lacks explanatory power since it cannot specify why significant, as opposed to nonsignificant, scoring should occur. There are three important answers to this question. The first, and foremost, concerns the nature of observation, which has been dealt with in some detail. Observation is the interaction of mind with matter. I have pointed out that mind consists of two facets of the mechanical processes associated with brain functions or with other physical systems incorporating *complete measurement loop* interactions (Walker, 1970, 1973, 1979). These facets are (1) state vector formation, and (2) state selection on state vector collapse. In the case of brain functioning, we recognize the first facet as consciousness (conscious experience). The second is will (the will concept of philosophy). The will is characterized by a calculable channel capacity, W, of about 10^4 bits/s designating what brain states occur. Because of the nonlocal property, which physicists have now shown to be a property of quantum states (Robinson, 1983; Rohrlich, 1983), the will channel also ties state selection of consciously perceived states of brain functioning to external psi events. Conscious experience of the quantum processes is almost entirely noise relative to psi events because it is tied to the internal quantum states of the brain. Consciousness can also be shown to have a capacity, C, computed to be about 10^8 bits/s (Walker, 1970, 1973, 1979).

Thus, the mind consists of consciousness and will, both of which are tied to the QM processes in brain functioning. W, the will channel capacity, is the channel capacity for psi events. Because the channel capacity C represents state vector formation (i.e., the information content represented by all potential brain state events as an ongoing process), C is not, strictly speaking, the process that selects psi events. Instead, psi events are the result of the state selected by the W channel, which in turn serves as the basis for each subsequent state vector. Thus, C is tied to W. Because of this close connection, there is a signal-to-noise problem for psi phenomena that we seek to control by our conscious intent. Strictly, the magnitude of the consciousness channel not tied to psi functioning is C minus W, so that the signal-to-noise ratio, R, for psi functioning is

$$R = W/(C-W) \qquad (1)$$

(since $C>>W$, Eq. 1 becomes for practical purposes simply $R = W/C$). Thus, as C approaches the value W (i.e., if the level of our conscious experience were reduced to the point that it involved a pure psi channel alone, devoid of spurious concepts), the signal-to-noise ratio would become infinite. This states that if there existed a mind that had no conscious experience other than that associated with state selection, there could not exist the idea of an event occurring other than the "target" state. For such a consciousness, the event desired, the observation, and the event that occurs would all be the same.

The observation issue is not one of understanding how observation causes psi events. It is instead one of recognizing that observation is the same as state selection and one of understanding the process by which "noise" hinders the normal process of causing selection in the external world. In saying that *noise hinders*, I mean that it provides more potential QM states for "false" targeting (which must be present if we are to have a faculty of conscious abstract thinking). Thus, the theory does describe some of the detailed structure of observation. Moreover, the theory also points to the problems in learning to facilitate psi events: (a) the low signal-to-noise ratio means extinction may be favored over learning; (b) there can be targeting rivalry among observers; (c) there are problems associated with discriminating consciousness events that entail strictly brain states from events that are nonlocally tied to external target psi events; (d) there is the problem not only of reproducing external conditions in efforts to replicate psi events but of reproducing the associated internal states to repeat psi events.

Bierman et al. (1981) hold that it is premature to expect detailed treatment of observation until more is known about the mechanisms of brain functioning involved in observation. This is surely a reasonable position to take. The theory attempts to deal with the linkage between the most fundamental of physical processes, state vector collapse in QM, and the broadest range of physiological processes, those involved in brain functioning. This is too great a span, involving the most wide-ranging of scientific disciplines, to expect a complete elucidation at this time.

Finally, we come to the most central issue in regard to the charge that the psychological factors that define observation have not been elucidated in observational theories. Psychology as it presently exists omits the central phenomenology bearing on behavior and perception, that is, consciousness. With the exception of parapsychology, the significance and central nature of consciousness has been omitted from science. It should not be surprising, therefore, that psychological parameters characterizing perception should be less than adequate to characterize observation, a task required in parapsychology. Psychology is not the parent science of parapsychology. Ultimately, psychology must be recast in parapsychological terms. Part of our effort now should be addressed to that task.

Causal loops? The question of causal loops has been most extensively considered by S. E. Braude (1979). The question of causal loops, however, does not have its origin in parapsychology, but rather in physics. The question is found even in classical physics. There one finds that bodies move along teleological trajectories as determined by the *least action* principle. It can be shown that the path of an object in classical mechanics is the one with the least action; that is, it is the one that requires the least effort to get the body from its starting point to its destination. To obtain that path, one must solve a variational calculus problem that assumes the body tries all possible paths to obtain the one with the least action. It is as though the body moves in a virtual way along all the paths and then selects the easiest one as a totality. The body moves along path deflections now so that it will avoid excess action at a point it has not yet reached!

This same odd behavior of physical systems is also encountered in QM. One encounters this in experimental tests of the Bell theorem, which have yielded results in agreement with the predictions of QM that measurement alters remote systems with which there exist no causal links except at the time of the preparation of the system and at the time of comparison or observation of the final results (i.e., by way of such causal loops). Experiments with photons have demonstrated this effect even on the macroscopic scale.

The error in Braude's argument lies in his tacit assumption that physical systems have objective reality independent of the act of observation. This concept was shown to be unsatisfactory by Heisenberg in 1925. Since both position and momentum cannot be measured simultaneously, the concept of path cannot be a valid physical construct. Instead, one must substitute the concept of multiple simultaneous probabilistic paths or potentialities that assume one or another configuration as limited by the equation for the state vector (i.e., collection of states). When the system is observed, there occurs an action-reaction event. The system enters a component state (or subset of states), and there occurs a conscious experience of this change in probabilities. For mathematical reasons, I have postulated (Walker, 1979) that the collapse occurs on completion of a measurement loop. For each loop, there is state vector collapse. For state vector collapse, there is consciousness. Successive conscious events have a time order. But conscious events do not have an *internal* order. There is not early, mid, and late state vector collapse. However, loops that extend over other nested loops will be experienced as simultaneous with all the included loops. As a result, the conscious experience of such loops, which can include events of psi experiments, is such that a part of the observer's conscious experience arises out of state selection for the process as a whole.

Is there a divergence problem? Do future observers participate in state vector collapse so as to affect the outcome of a psi experiment? Observational theory requires the answer to be yes. Where multiple observers are present in the experimental situation, the theory requires all to be constrained to enter the same collapsed state. Physical constraints require that there not be a preferred or "first" observer (there can, of course, be subjects having different abilities). We are therefore forced to answer yes.

But if the answer is yes, does this mean that future observers change what has already been observed? From QM, the answer is no. Future observers can only be considered to be potential contributors to the psi information input. But this opens the door to an infinity of possible future observers. This would suggest that in all cases experimental results are dominated by future observers so as to always wipe out even the effect an exceptional subject has on experimental results. Rýzl's experiments (1962) with Stepanek are adequate to show that such a situation would conflict with experimental results. It therefore becomes necessary to resolve the problem that arises if one assumes an infinite number of observers. Hypotheses have been offered for resolving the problem by introducing an artificial cutoff or a progressive reduction in the maximum influence of successive observers. However, the unmodified theory, which places no restrictions as to the order, time, or number of observer interactions, appears to be in agreement with the broad range of experimental results in parapsychology. Moreover, I have shown (1977a) that the effect of even a million future observers is not expected to wipe out the results of a gifted subject. On the other hand, Houtkooper and de Diana (1977) have argued correctly that the variance increases with successive observers. Thus, although a million observers may not wipe out expected results, one will encounter wide variations in experimental results. But isn't that exactly what we observe?

We should keep in mind that there are constraints on future observers. Only those observers connected causally to the state preparation in such a way as to produce nondegenerate states (states distinguishable by measurement) can have an effect on experimental results. How much this constraint impacts on the divergence effects cannot easily be determined because of the complexity of the system we deal with. It is likely that most future observers have no connection with state preparation. Moreover, the results of an experiment must be expected to condition the expectations and normative values of those future observers who first come to know of the results through secondary reports.

Unconscious process? As for the argument that experimental work indicates that psi is due to an unconscious process, the theory explains that in most cases the "noise" encountered in experimental work is so severe that the signal is masked, thus giving the impression of an unconscious process.[1]

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[1] Note that consciousness is *not* defined by verbal or other reports of awareness, but is the experience itself. This is the basis for the distinction between consciousness and

Millar's Review of "Observational" Theories

Brian Millar (1978) has written one of the more extensive and influential reviews of the QM theory of psi, which he lumps under the heading of observational theories along with the works of Schmidt (1975b) and of Donald and Martin (1976). In Millar's presentation, these models consist of three elements: feedback, time-displacement PK, and pure chance. Millar extracts elements from several authors to obtain a model that seems to make a discussion of the complexities of quantum theory unnecessary. This has perhaps been the cause of some objections to the present theory of psi phenomena. It has given rise to the idea that the theory is self-contradictory; for example, as it is presented in Braude's (1979) work. A rigorous analysis of the logic presented in this survey of the model would find this feedback-time displacement PK rather circular, just as Braude argued. But it must be kept in mind that Millar's presentation is suited primarily to giving a rough picture of the nature of the phenomenon, which without some reference to *least action* principles in physics, or to the nature of state vector collapse in QM, will suffer on close examination. Thus, a rigorous presentation is necessary if one is to obtain a technically satisfactory understanding of the theory and not merely an abstract of the theory.

There also exists a question about whether my QM theory of psi phenomena is an observational theory as Millar has defined such theories. From Millar's statement, it appears that observational theories exclude the possibility of any coupling of observers other than as simultaneous witnesses of an event. Thus, Millar states: "Stanford's (1978) 'conformance behaviour' model shares virtually all the features of the observational theories. However, it differs in one crucial respect . . ., namely, in his model observation of the result by the subject is not necessary for psi to occur, in contrast to the observational theories" (p. 305). Or again, he says, "On this basis, the ambit of PK is well curtailed, for it can only affect events which the subject later

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perception. Conscious experience can contain data that can be processed by the brain so as to bring out special aspects of that information, even though that information may not have been included in the attention set. As such, information can occur in the stream of consciousness without there existing the possibility of its being verbally reported as a part of the conscious stream. The conscious stream runs at about 10^8 bits/s. Only a fraction of this is stored in memory to be available for future reference in a verbal or other report. Moreover, verbal reports are limited to less than 100 bits/s. Between the experience and the verbal (or other) exposition of conscious states lie mental processes. Things experienced consciously are not bound by the limit of the verbal report.

observes" (1978, p. 306). But in my theory, state vector collapse occurs because of consciousness.[2] Because several simultaneous observers causing state vector collapse must give rise to one and the same final observed state or outcome, we must assume that a part of their conscious experience, the part that was instrumental in selection of a particular state, had to be coupled so as to be constrained to the same outcome. Now the space-time independence of this constraint on state vector collapse means that not only are simultaneous observers coupled, but also observers of past events of the system are coupled and constrained.

Consider an experiment such as the one Schmidt (1976) conducted with prerecorded targets generated by using a QM random number (event) generator (RNG). Nothing is observed until the subject listens to the tape. Until that time, we can consider the recorded sounds to be represented by a state vector that includes all possible recordings. When the subject hears it, one sequence of sounds comes into being as an actualization of one of the potentialities. But what happens if we have an ESP task in which the experimenter who generates a target sequence, a subject who makes and records his calls, and the person who scores the results by comparing the target and call lists are each separate individuals, none of whom ever communicate the details of their task to the others? According to Millar's picture, ESP would be impossible. According to mine, however, that part of the conscious experience tied to overall state selection, which involves the subject's selection of calls as well as the subsequent observation of the experimental results, is space-time independent and consciously experienced by each of the individuals involved. It is only because this is such a small part of the total consciousness that we ordinarily do not realize the commonality of the state selection constraint on our conscious experience.

It is probably Millar's conception of observation that leads to his particular view of the divergence problem. Millar states, ". . . consider the ludicrous situation if a really strong psychic got out of bed on the wrong side: he could wreak havoc on all the work reported in the journals he read that day!" (1978, p. 321). Such is not a possibility in the QM theory of psi. The psychic does not alter the observed past but is instead a part of the overall constraint of the system in the process of state selection. As such, state selection represents as much a

[2] Or more properly, it occurs because of the will.

constraint on future observers as a capacity for future observers to influence a present event.

Millar does not state that the feedback-time-displacement PK is, in fact, time-reversed causation. However, his conclusion about what the theory predicts in specific situations represents such an interpretation, which is contrary to the QM theory of psi. In other aspects of Millar's presentation of the observational theory model, the details are substantially consistent with the QM theory of psi.

The Languages of Quantum Mechanics

But when Millar (1978) turns specifically to the QM theory of psi, some difficulties arise. They arise from his apprehension that the theory has undergone repeated revision. He states: "Walker's theory has undergone considerable modifications in the course of its development" (p. 310). Later, Millar refers to my "flights of fancy" (p. 313). These difficulties spring less from his specific summary of the theory (taken from my 1979 review paper published in *Psychoenergetic Systems*) than from his seeming failure to see the overall structure of the theory and the logical relationship of what at first appears to be many distinct parts. Millar cannot entirely be faulted for this, for much of this complexity, this confusion of descriptions of the theory, springs from this same complexity and seeming confusion in QM itself.

Quantum mechanics was discovered twice, first by Heisenberg in the form of "matrix mechanics" and then a year later by Schrödinger in terms of "wave mechanics." Subsequently, Schrödinger showed those two theories were merely different representations of one theory. The formalism used for this third presentation of the theory was in terms of vectors in a complex Hilbert space. Since that time, other representations of QM have been developed to serve other purposes (for example, the interaction representation used extensively in elementary particle theory). These representations each have their own language to describe phenomena, and each has aspects that make it superior for expositing certain concepts. The matrix mechanics and Hilbert space pictures of QM are much easier vehicles for understanding nonlocality in QM. In terms of these representations, the fact that nonlocality is an essential feature of quantum theory is immediately apparent. The wave mechanics representation is rather more useful for dealing with the spatial relationships of particles or bodies. In addition to those formal and complete representations of

QM, one can also use the wave "packet" terminology or indeed obtain good approximate treatments of physical systems by appealing directly to the Heisenberg uncertainty principle. A presentation of the quantum theory of psi phenomena in terms of any one of these pictures of QM will yield a presentation that appears vastly different from a presentation in terms of any other picture of QM. Each of these has been used by me in an effort to exhibit the many characteristics of QM that mirror psi phenomena. Such an effort, if not carefully considered, can lead to the idea that the theory has changed.

Because different physicists are more conversant with one picture of QM than another, and because the different pictures of QM highlight different aspects of the theory, I have presented the QM theory of psi from each of these points of view. Even this, however, does not exhaust the problems to be treated in any exposition of the theory. In the preceding, I discussed the several mathematical representations of QM. In addition, there exist about four major ways, interpretations, used by physicists to find an accommodation to the measurement problem in QM.[3] Thus, some physicists have believed in a subquantum level to QM that is responsible for state vector collapse (one that determines the relationship between the probabilities of QM and observed events). For some of those who would accept a subquantum interpretation, the idea that consciousness is the hidden variable that mediates quantum events finds support. But for other physicists who are aware of the shortcomings of conventional hidden variable theories such as those proposed by Bohm (1951) (in Bohm's use, hidden variables were supposed to represent some mechanics operating at the subatomic level), there is frequently a conceptual block against even the use of such language. In that case, the entire theory must be restated so that it uses the language of the particular interpretation these physicists feel comfortable with. Restating the theory in such differing ways can be confusing to those not familiar with the various mathematical representations and interpretations of QM.

As if these difficulties were not enough, a complete theory of psi phenomena must deal not only with the physical mechanism underlying ESP and PK but with the biophysical issues, psychological considerations, and the practical parapsychological details of the theory.

[3] These four major interpretations are the Copenhagen interpretation, the stochastic ensemble interpretation, the hidden variable interpretation, and the relative state interpretation.

Parsimony

Millar professes little interest in a theory bridging the gap between parapsychology and mainstream physics. His interest is directed primarily to an exposition of differences and similarities among "observational theories." He appears to feel that the measure of a theory is parsimony. He ignores the clear fact that the most parsimonious theory of psi phenomena will not prove satisfactory unless the theory can bridge the gap between conventional science and the findings of parapsychology.

Bertrand Russell was fond of a story about the intellectual chicken who observed each morning the farmer arrive to bring him food. The intellectual chicken duly noted this sequence of events and theorized a fundamental law of nature: Every morning a farmer appears; this appearance gives rise to food. Alas, one morning the farmer arrived and chopped off the head of the parsimonious theorist. The point is, of course, that parsimony is fine so long as it deals with an adequate range of the real phenomena. It must provide an overall understanding of nature. Where does the farmer come from? Where does he get his food? What motivates his behavior? We cannot accept a system of pure hypothesis in positing a phenomenological theory of psi phenomena. One cannot simply assume "the three basic ideas of the observational model ready made" and make "no attempt to relate them to the rest of physics." If we do this in parapsychology, we will ignore the most firmly established body of facts available to us, the knowledge embodied in the laws of physics. The observational theory taken alone without the interpretations and understandings that quantum theory provides is simply not a viable theory. Taken alone, the concept of observation's causing psi is simply too narrow, undefined, and insupportable. One must have the understanding of the nature of physical reality that QM gives us to see the role observation plays in relation to external physical objects.

But this is not simply a one-way street in which parapsychology benefits from quantum physics. Both psychology and physics presently encounter severe fundamental problems because they have ignored the phenomenology of consciousness. It is our opportunity to resolve these problems, not wait till the rest of science comes to *our* rescue. For these reasons, it becomes imperative for us to develop the most satisfactory foundations possible for understanding psi phenomena. That requires postulating a connection between QM and psi phenomena and exhaustively searching out all the implications of such a proposal.

Millar's Criticisms

The question of parsimony leads Millar (1978) to raise several issues. To Millar, bringing QM into the problem of the nature of psi phenomena seems to introduce the unnecessary subject area of atomic physics. Millar states that "Schmidt argues forceably that there is little point in speculating about microscopic systems so long as we are unable to observe psi in such systems: there is no way we can test our deductions" (p. 313). But again we encounter an area that has not only been misunderstood by Millar, but has been widely (but not universally) misunderstood in physics. This is the idea that QM has to do only with atomic processes. Were this strictly true, of course, we could not even know of the existence of QM. But we do know that classical physics works well on the macroscopic scale. To account for this fact, Ehrenfest (see any standard textbook on QM, such as Bohm's *Quantum Theory*, 1951) derived a theorem from the Schrödinger equation to show that under *special conditions* the laws of physics at the atomic scale approximated to Newton's equations of motion. However, Ehrenfest's theorem does not apply to processes that magnify the atomic processes so that they can be observed on the macroscopic scale. For the Ehrenfest theorem to hold, the physical system in question cannot be subject to strongly varying forces. Measuring devices that magnify microscopic effects do not satisfy this requirement, nor will brain processes, in general. More specifically, I showed that the conditions of the placement experiments used by Forwald do not satisfy the restrictions of the Ehrenfest theorem. Thus, Millar and Schmidt both miss the point when they say we need not be concerned with atomic processes. We do need to be concerned with the physics of atomic phenomena that has now shown that our entire prior conception of reality is not true. It is a physics that shows that macroscopic states are altered in the process of observation.

Again, in other areas Millar has felt the details of a theory of parapsychology need not be developed. He sees my efforts to treat extensively the various aspects of the QM theory of psi as simply a collection of unrelated ideas. Millar (1978) states:

> Walker's theory comprises a large number of ideas, some likely, while others seem quite implausible. At least three sub-theories are involved (1) the physical theory (2) the neurophysiological theory and (3) his phenomenological treatment. . . . It is worthy of note that Walker has derived all 3 basic items of the general observational model from theories (1) and (2) and has not merely arbitrarily adopted this picture. Walker's phenomenology, itself only rather loosely grounded on the above

theories, is based on information in the sense of information theory. (p. 312)

Now the postulate that observer-mediated state vector collapse in QM is the basis for the occurrence of psi phenomena has been shown to be compatible with the phenomenology of parapsychology. A discussion of specific tests of this theory will be given below. But the success of this immediately requires one to show how the QM is tied to the brain's functioning. This has been done in my paper "The Nature of Consciousness" (1970) and in subsequent updates of that paper (1977c, 1979). In these papers, quantitative tests of the consciousness theory have been applied. These tests support the validity of the theory. The theory of consciousness has some aspects that distinguish it from simply brain functioning, and, indeed, the theory provides a basis for understanding the origin of the concept of will that has existed unexplained in philosophical literature for centuries. Moreover, the theory provides the quantitative basis for *all* the numerical calculations and tests of the QM theory of psi I have made (the use of the signal-to-noise ratio, $10^4/10^8$, and of the channel capacity of psi, 10^4 bits/s, have formed the basis for calculations since the original work [Walker, 1970,1973]).

Two entirely distinct approaches to the theory of consciousness and psi have been taken, both yielding the same results. Starting with QM and the measurement problem in quantum theory, the logic of the phenomenology carries one irrevocably to the fact that QM must involve the consciousness of the observer and must allow the possibility of the existence of psi phenomena. Starting with the question of what is the nature of consciousness as it arises in the brain, it can be shown that there simply exist no other viable options than that consciousness springs from QM processes in the brain. From the development of this line of reasoning, it has been shown that we also uncover the mechanism that relates brain functions to psi phenomena. Thus, the theory is self-consistent, yielding the same overall picture regardless of where we begin.

But this theory of consciousness does assume one thing, that synaptic functioning involves the QM mechanism of electron tunneling. To see if this is in fact the case, a study of mechanisms of synaptic functioning was carried out. The firing of a synapse involves the release of the contents of vesicles into the synaptic cleft when a neural impulse arrives at the presynapse. The mechanism by which this occurs has been under investigation for some years. The generally accepted idea is that it involves "calcium gates." This proposal is of limited utility in accounting for many quantitative details of synaptic functioning. I (1977c) have presented a theory involving electron tunneling, and I have shown that the theory is in quantitative as well as qualitative agreement with all the available experimental data. No other theory so far proposed has achieved such a level of compatibility with the experimental data.

It must be reemphasized here that electron tunneling is not like a chemical reaction that may or may not be occurring in a cell. If the physical requirements for electron tunneling are met, as they are at the synaptic cleft on the arrival of a neural impulse, tunneling will occur. Electron tunneling is a fundamental physical process. Thus, the theory is far less speculative than it might seem. On the other hand, QM tunneling is not easily demonstrated directly. One can show the presence of a magnetic field with a magnetometer, but one cannot show the occurrence of electron tunneling in a complex system such as a synapse by a simple measurement with a "tunneling meter." Establishing the occurrence of tunneling requires subtle analysis of a number of quantities, including details of the current-voltage characteristics of the synapse. This can be and in fact has been done in the case of the electrical synapses or "ephapses." But the current involved in vesicle release is extremely small. Thus, one will not see these effects unless they are looked for.

Now let us look at Millar's charge that the "phenomenology" is not derived from the rest of the theory. This is not the case, although perhaps the discussion of the reasons for certain points in the theory may have been so cursory that it gave such an impression. For example, physical theory states that the observer has control over state vector collapse—that is to say, state selection. (We ignore here the issue of the signal-to-noise problem that reduces the rate of success in such tasks, because this is dealt with elsewhere.) If there are two potential states, ψ_1 and ψ_2, where $\psi_1^2 = \psi_2^2$ (i.e., the probabilities are equal), then the selection of, say, ψ_1 over ψ_2 represents 1 bit of information. It also represents the ability to select at will an event with a probability of .5. Ten such events in a row would require 10 bits of information and would allow the selection of an overall event having a probability of less than .001. Thus, the physical theory itself manifestly expresses a phenomenology that ties information measures to probabilities. Now the requirements of the physical theory (the requirements for state vector collapse to occur) were the reason the original theory of consciousness (giving rise to a consciousness data rate representing state vector "preparation" in synaptic interactions) had to also involve a will channel data rate, that is, a data rate representing state vector collapse as an ongoing process involving the brain. Thus, the fact that $W\Delta t$ is the amount of information involved in state vector collapse in synaptic interaction in an interval of time Δt is a requirement of the physical theory, the value of W simply being calculable from physiological data using the neurophysiological theory. With $W\Delta t$ as a given amount of information, the meaning of "bits of information" is all that is required to relate this to the probability of the state that can be selected by the will exerted during the interval Δt. Thus, we have immediately

$$P = -\log_2 (W\Delta t) \tag{2}$$

The problem of how to deal with Δt in Eq. 2 is another matter. Seldom do we encounter psi tasks that are time continuous. One might imagine a time-continuous psi task such as a psychic attempting to cause and maintain a displacement of a ball against gravity (where such a displacement arises out of QM processes). But generally, psi tasks are events taking place in such brief intervals that we might question that the time element Δt should play a role at all. But there already exists a completely analogous situation in psychology, namely, in learning theory as it is applied to maze learning tasks. The brain is a computer capable of data processing at a rate B (in bits/s). How do we deal with the problem of how the brain of a laboratory rat placed in a maze makes a choice at a decision point in the maze? Bush and Estes (1959) assumed the existence of a set of data consisting of a large number of "bits" of information (cues) that was processed on the occasion of the animal encountering a decision point. This data set (behavior cues) was then processed by the brain by an algorithm to generate the animal's response. Similarly, I use this treatment to handle the scoring rate on ESP tasks for the general population. (Incidentally, I evaluated the size of this data set. According to the theory, the magnitude of this data set should be approximately the same as would be determined for a human in a learning task. This is a testable prediction of the theory, readily verifiable using existing experimental data.)

Thus, Millar's charge that the theory consists of loosely related subtheories is not true.

Discussion of Millar's Treatment of Schmidt's Mathematical Theory of Psi and the Thermodynamic Theory of Donald and Martin

Millar (1978) also treats the theory of Schmidt (1975b) and the thermodynamic theory of Donald and Martin (1976) as equally viable alternatives to my theory. They are, however, not sufficiently grounded for that role. Schmidt's theory suffers from the fact that it is much more a *program* for a phenomenological study. One finds a phenomenology in which events occur at rates that deviate from their a priori probabilities, so a theory is proposed that psi phenomena are caused by psi-causing sources that alter the a priori probabilities of events. It would seem difficult to fault the logic. It is also difficult to see that this resolves the serious issue in parapsychology about the mechanism of psi.

As for the thermodynamic theory of Donald and Martin, one finds a similar fault. The entropy measure in thermodynamics is directly tied to the probability of events. For a given thermodynamic system one can calculate the probability associated with a given entropy change and the probability of changes in the physical system deviating from the predicted state of the physical system. For ordinary macroscopic objects—objects in the range of sizes from a millimeter to a few meters—one is concerned with a system containing from 10^{16} to 10^{30} atoms. To achieve an ordered behavior on the part of even one in a million of these atoms involves deviations from chance corresponding to some 100 to 10^9 standard deviations!

Thermodynamics does allow for the occurrence of "violations" (deviations) of the second law of thermodynamics. These deviations are tied to specific probabilities. Further, the deviation of the physical system from the state predicted by thermodynamics typically involves occurrence probabilities that are incredibly small, much smaller than any we encounter in parapsychology. Even the occurrence of a perfect run in an ESP card deck is rather likely compared to the chance deviation of a thermometer reading by a 1/1000th of a degree as a result of entropy decrease in violation of the second law of thermodynamics. The entropy measure is just another way of expressing probabilities. Therefore, a psi theory in such terms has no explanatory capacity of its own.

The effort to tie entropy decrease to future boundary conditions also cannot be justified. We have heard of how one can tie entropy increase to the direction of time, as the "arrow of time." To reverse entropy would be to reverse time. But when we look at the meaning of the Heisenberg uncertainty relations, it is clear that even reversing the direction of time cannot alter the progress of thermodynamic systems toward increasing entropy (Walker, 1982a). The unraveling motions of atoms necessary for time's arrow to run backward and give us entropy decrease are forbidden because the precision of position and velocity required of the motion does not exist in principle for physical bodies. The meaning of the Heisenberg uncertainty relations is that the simultaneous existence of precise position and momentum (both are necessary to compute paths) does not have physical meaning. Thus, time-reversal thermodynamic explanations of psi are not viable.

The quest for a thermodynamic theory of psi phenomenology is an appeal to little more than a tautology (relating entropy change to the probability of occurrence of psi). (Note: although the phenomenological equation given by Donald and Martin is the same as mine, the equation simply relates probability of an event to entropy change or information change. In my theory, this change is tied to specific mechanisms. In Donald and Martin's theory, it is not.)

There is a further difficulty with the appeal to thermodynamics by Donald and Martin. As Millar (1978) says, "Thermodynamics can now be largely derived from a consideration of the behavior of the constituents . . ." (p. 315). As such, where does any basis lie for showing a relationship between the observer's state and the occurrence of psi events? If the theory has merit as a physical explanation of psi phenomena, it must have the capacity to explain (causally relate) the phenomena of interest. The Donald-Martin theory does not succeed in this requirement.

Gardner's Criticisms

Martin Gardner (1982) has presented in some ways a superior treatment of my theory than has Millar. Gardner is more concerned with the relationship between the theory and the fundamental concepts in quantum theory. Unfortunately, Gardner seems not to understand the significance of recent developments in quantum theory. Gardner fails to appreciate fully the context of the theory; that is to say, the situation concerning the difficulties encountered by the physics community with the measurement problem in quantum theory, problems that have arisen exactly because of an effort to avoid the clear implication of QM that the observer, by collapsing the state vector, can affect the physical world. As a consequence Gardner, like Millar, sees postulates at every corner, postulates that are only direct statements of established facts in QM. Gardner states:

> Because, in Walker's view, all parts of the universe are connected on the subquantum level, he sees no reason why the human will cannot use this level to collapse wave packets of quantum systems outside the brain—regardless of how far they are away. (p. 62)

Although Gardner perceives this as a postulate made by me, it is actually a part of the physics of QM. The wave function is a solution of the Schrödinger equation, which embodies QM. The wave function provides us with a complete representation of any physical system. That system can be an atom; an atom being probed by some measuring instrument; an atom, measuring instrument, and a human observer; or any other part of the universe. Now, this solution incorporates all the possible results of a measurement on the system. For example, let us consider an atom having a spin, such that this spin can be oriented only either up or down with reference to a given magnetic field. Let the system be the atom, the measuring instrument, and an observer. The solution to this problem will contain two terms. The first will describe the atom with spin up, the measuring instrument indicating the atom's spin as being up, and the observer as having a brain registering an observation of the instrument in the "up" condition. The second term will be for the "down" orientation. These two terms have a property of orthogonality.[4] This means that only one *or* the other condition can ever be the consciously experienced condition of this physical system.

It is the universality of QM that means that any system no matter how complex or extensive can be represented by QM, and the orthogonality of the terms in the wave function representing the system that guarantees that the overall system will go into one state or another state as a collective event. When we consider the results of experiments to test Bell's theorem, this fact becomes even more remarkable. The tests of Bell's theorem prove QM to be literally valid and accurate in stating that this orthogonality applies even to systems no longer in contact, regardless of their separation. This proposition that a measurement observation, a *conscious* observation, causes state vector collapse (i.e., causes the system to go into one overall coordinated state regardless of the spatial separation of the various parts) is not a hypothesis offered by me, but a requirement of QM. This fact

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[4] The term *orthogonality* is used by physicists because it actually refers to the two states being perpendicular to one another. Thus, the wave function for the above spin problem would consist of two algebraic terms for the two possible states. To compute probabilities, one must "square" the amplitude (actually multiply by the complex conjugate) and integrate over all space. These algebraic terms have the property that the cross-term products (the first term times the complex conjugate of the second term, for example) when integrated over all space are zero. Thus, these algebraic terms behave as though they were perpendicular to one another (i.e., orthogonal). This is the basis for describing QM functions in terms of vectors (with orthogonal components) in Hilbert space. Note that the orthogonality of components of ψ (states of the physical system) is an orthogonality in Hilbert space. Each of these components in Hilbert space is expressed in terms of spatial quantities. Thus, an object may have spin "up" in real space as one possible state, and spin "down" as a second possible state. Since the object cannot be observed to be in both at once, the Hilbert space terms must be orthogonal. If one is in the Hilbert space plane in which the spin is "up," the projection of the "down" spin term will be zero.

rests on the orthogonality of the terms in the wave function solution of Schrödinger's equation. And this fact is among the most firmly established facts in physics.

Gardner states, "That the brain can do this [cause state vector collapse of the overall quantum state] . . . is, of course, pure speculation" (p. 62). It is, however, not my own speculation. As required by quantum theory, the brain, which is a physical object of itself cannot cause state vector collapse. It is rather that, according to the understanding of Bohr, von Neumann, and Wigner and, generally, of the preponderant view in the field of QM, state vector collapse is only definitively known to have occurred as of the time it is consciously experienced. Because physicists in the past have neither had any inclination to entertain the idea that consciousness exists nor given any physical theory for its occurrence or manifestation, statements clearly pointing to the fact that it is consciousness causing state vector collapse have been made either with timidity or by opponents of quantum theory. The hypothesis is not my own. My hypothesis is merely that we take literally this apparent requirement of QM to see what its implications are. The immediate implications are that consciousness causes state vector collapse and that the conscious state is correlated with the occurrence of events, some of which we call paranormal. Gardner's assertion that "in QM it is not the human observer who collapses wave packets but the observing instruments" (p. 62) and that "it is only after a long chain of macrointeractions (involving events that are irreversible) that a human mind 'sees' the tracks in the same way it sees a star or a tree" (p. 62) are statements that have not and, because of the linearity of the Schrödinger equation, cannot be derived from the Schrödinger equation. As Wigner (see Walker, 1982b) has stated, "There is no such thing as irreversibility in quantum mechanics." State vector collapse exists as a distinct hypothesis of QM. It cannot be incorporated into the Schrödinger equation without the complete reformulation of QM, and no theory has so far successfully achieved this.

Having passed off some of the basic tenets of QM as my own invention, Gardner states:

> Walker's second assumption is even more staggering. Not only does he suppose that the mind can alter wave packets of distant objects, but he also assumes that it can alter a wave packet in such a way as to bring about a desired value for one of the variables. (p. 62)

Actually, the postulate is that the conscious state experienced is correlated with the state into which the state vector collapses. Thus, consciousness is the reaction to the action of state vector collapse and vice versa. The conscious experience of an observed state is the only established rule in physics by which one can determine unerringly when state vector collapse occurs. Further, state vector collapse cannot be derived from the Schrödinger equation. For these two reasons, it seems appropriate to suspect a correlation exists between the consciousness experience and the physical state into which the wave function collapses. The existence of such a correlation is directly implied by the known facts regarding state vector collapse.[5]

Gardner calls this "staggering," however, because of its clear implication that psi phenomena should occur. Thus, deviating not one bit from the tenets of QM, but merely taking QM literally, requires the existence of psychic events.

We must thank Gardner for his, albeit, pejorative conclusion at this point. "It is obvious that if both of Walker's assumptions are correct, a scaffolding exists on which to hang a theory of ESP and PK" (p. 62). Gardner cannot accept this. Along with many physicists, Gardner holds to the postulate that "the value acquired by a variable, after wave-packet reduction, is the outcome of absolute chance" (p. 62). In this, he places Einstein and me on the same side of the issue. Einstein could not accept the idea that state vector collapse would be a totally random *uncaused* event. I take the position that to overlook the connection between consciousness and state vector collapse not only leaves physics dead-ended in the measurement problem, but also throws out the window a ready-made answer to the most central problem of philosophy, "What is the nature and origin of consciousness?".

[5] The *relative state*, or the so-called many worlds, interpretation of Everett and Wheeler is the only presently fully formulated interpretation of QM that satisfies the logic of QM without requiring state vector collapse or an equivalent concept. This interpretation, however, requires that every potential state allowed by QM does in fact occur. Thus, for every quantum event in the universe yielding two possible outcomes, this "many worlds" interpretation holds that they actually occur, the *whole* universe splitting into two exact duplicates, except each universe carries away its own value for the QM event that caused the split. That at first may seem only slightly horrendous, but then we consider that for many events there exists a continuum of subsequent states (measurement of the position of an object, for example). This requires that for every two particles, every two electrons, there is generated in any finite time increment a nondenumerable infinity of universes. It gets worse, however. In elementary particle physics, we learn that there exist virtual interactions. At each moment, an electron can create an infinity of interactions with virtual particles, even to interactions as complex and extensive as the entire universe. These, too, would have to be treated as real events, producing new infinities of universes, not merely mathematical constructions. Yet this hypothesis is so designed that it is untestable! Its sole virtue is that it allows an interpretation that removes from physics serious consideration of consciousness in QM. This relative state hypothesis is treated as a viable interpretation of QM. It is discussed in most texts on QM that have a section on the measurement problem.

Gardner also states, "Walker's theory clearly accounts for the seeming independence of psi from space and time constraints. Moreover, it accounts easily for the 'sheep-goat' effect so often invoked by parapsychologists" (p. 63). Gardner reviews, rather satisfactorily, the explanation of PK effects on dice and further states, "Walker's theory also accounts for the embarrassing fact that parapsychologists have been unable to detect a PK effort on a delicately balanced needle even when many minds are collaborating on the effect over a long period of time" (p. 64). Thus, whereas Gardner fails to appreciate that the basic postulates are already substantially a part of quantum theory, he does recognize that given these facts, the theory accounts for the reported phenomena.

In fairness, it should be pointed out that one of the reasons physicists have turned their backs on the consciousness option is that they have not seen how to make a science of such a topic. If one says, "Consciousness causes state vector collapse," isn't that something like invoking the Diety to explain, say, evolution? I, however, have presented a formal theory of consciousness that shows how to treat this phenomenology in a scientific and quantitative fashion. Although much needs to be done to further develop this theory, the theory at present is supported by quantitative experimental tests.

In Gardner's synopsis, he states:

> Walker regards the human mind . . . as an ongoing QM process. We possess, he says, a "will" that is continually reducing wave packets in the brain to bring about new mental states. This process, he conjectures, involves "electron tunneling across synaptic clefts." There is no experimental evidence of this. (p. 62)

Gardner treats this as a pure hypothesis, ignoring my published journal article detailing the evidence for this mechanism (Walker, 1977c).[6] Gardner further fails to appreciate the nature of QM

[6] In a recent article in *Scientific American* (Oct, 1982; p. 56), R. R. Llinas reviews the evidence for the calcium hypothesis of synaptic transmission. This theory states that a current of calcium ions triggers the release of chemical transmitters to cause the passage of signals from one nerve cell to another. Difficulties with the calcium hypothesis have been cited by prior authors (see Walker, 1977c, and Remler, 1973, for references). Two points should be noted, however. *First,* the experimental data in support of the calcium hypothesis have been obtained from experiments on a primitive and atypical synapse, the giant synapse in the squid stellate ganglion. As such, more sophisticated mechanisms involving electron tunneling may operate in less primitive organisms. *Second,* the experiments only support a calcium current as being concomitant with vesicle content release. Note, for example, that the charge carried by the calcium current required to fire the giant synapse is about 1.25×10^9 electronic charges as given by data in Llinas's article. The charge on the giant synapse can be indepen-

tunneling. In biology, that an animal has a leg may or may not mean it jumps. In physics, if the conditions exist for QM tunneling of electrons, then they follow those laws and jump. At the synaptic cleft, it is an easy task to show that the conditions exist. As I demonstrated (Walker, 1977c), electron tunneling must dominate in the functioning of ephapses (narrow junction synapses) and occur at a sufficient rate to mediate vesicle release in synapses. Electron tunneling will occur in the brain, to some degree. At least this is fact and not merely hypothesis.

Criticisms Against Experimental Evidence for the Theory

Let us now turn to the criticism Gardner and Millar raise against experimental tests of the QM theory of psi phenomena. Both center their objections on my use of Forwald's experimental data. In neither case is the theory faulted; it is only objected that I used Forwald's data. Unsupported, such a position is not acceptable scientific procedure. Gardner (1982) in his criticism charges:

> To bolster his theory that a single wave-packet collapse can start a divergent process that ends with a desired macroresult, Walker relies almost entirely on the published results of experiments made over a period of some twenty years by a retired Swedish electrical engineer named Haakon Forwald. Most of Forwald's papers were published in the fifties in Rhine's *Journal of Parapsychology*. They dealt not with dice but with unmarked cubes[in] . . . a "placement effect." (p. 65)

> In his lenghty paper "Foundations of Paraphysical and Parapsychological Phenomena," Walker devotes many pages to a detailed analysis of Forwald's confusing results. (p. 65)

> When I first learned that Walker relied so heavily on Forwald's work I was astounded. Most parapsychologists today have a low opinion of this work. For one thing, almost all of it was solo—that is, Forwald acted as both experimenter and subject. (p. 65)

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dently calculated to be initially about 10^9 electrons; within limits of calculational accuracy, this is the same result. Thus, the data given by Llinas simply support the theory that a charge transfer is required to activate the presynaptic vesicle gates, as I have argued. That this can be achieved by currents as well as by electron tunneling is consistent with my theory of electron tunneling. The calcium hypothesis does not account for this charge equivalence just noted. That calcium ions have this effect whereas sodium and potassium ions do not stems from the lower energy levels relative to electrons provided by calcium ions in solution.

This criticism does not address the fit of data to theory but addresses personalities and implies questions of fraud or deception. Gardner has in the past attacked Rhine on such grounds; yet, when it suits his purpose, Gardner uses the authority of Rhine to attack other parapsychology researchers as Gardner does in a lenghty attack that follows in his paper. Gardner has long attacked parapsychology on the grounds that there are no fraud-proof data. And here again, Gardner is not attempting to show some disparity between theory and data, but he simply attempts to discredit the data used by me as a substitute for treating the technicalities of the theory. But one should ask, "Why do we not have this same requirement for fraud-proof data in all areas of science?" Science has largely been built on the shoulders of researchers working alone in their laboratories. The reason is the agreement scientists obtain between theoretical concepts and experimental results. Here we have a theory, the QM theory of psi, that is so pristine in its assumptions that the entire procedure for checking the theory against Forwald's data is spelled out by the very initial statement of the theory, namely, that psi events arise from biasing QM state selection. There is nothing left but to carry out the calculations. Forwald, whatever his purpose, produced data. Those data could have been biased toward some prior expectation or desire. This certainly is a hazard. But unless Forwald secretly postulated and calculated the results for a QM theory, there is little chance ($p < .0006$) that the experiments and theory would have agreed. It is this agreement between theory and experiment that has freed science from any serious concern with fraud or legerdemain. Just as Forwald's data strengthen the theory, so the theory frees Forwald of charges of faking his data. As a result, Gardner's argument is irrelevant to the issue of the validity of my theory.

But what of Gardner's charge that "Walker relied so heavily on Forwald's work"? Gardner's argument is disputed by his own admissions.

> It is obvious that if both of Walker's assumptions are correct, a scaffolding exists on which to hang a theory of ESP and PK. (p. 62)

> Walker's theory clearly accounts for the seeming independence of psi from space and time constraints. Moreover, it accounts easily for the "sheep-goat" effect. (p. 63)

> Walker's theory also accounts for the embarrassing fact that parapsychologists have been unable to detect a PK effect on a delicately balanced needle . . . assuming the subject is not a superpsychic. (p. 64)

These points of agreement summarize the results of many experiments, and these are significant experimental results bolstering the theory. As I told Gardner, Forwald's data were singled out for detailed treatment because they serve to show how to handle mathematical details of the QM theory of psi, to show that with Spartan assumptions one can go directly to specific calculations that can be compared to experimental results.[7] Moreover, in the same paper criticized by Gardner, there is an appendix giving additional tests of the theory. Thus, Gardner's charge that my test of the theory depends largely on Forwald's experiments is false.

Millar is also concerned with this issue of the use of Forwald's data. Millar (1978) argues:

> We may find ourselves wondering whether this dependence was merely the result of Forwald's psychological expectation. This opinion is rather reinforced when we find out that the determination of physical dependence was Forwald's intention in these experiments and that the data fitted his own theory rather satisfactorily too. (p. 319)

Millar's argument, however, does not stand up under an examination of Forwald's work. We should note that Forwald had no prior theory. Much of these data were developed to find possible characteristics or trends in the data that might suggest a theory. Forwald's theory did not rest on any acceptable physical principles. (He proposed that psi phenomena resulted from a radiated gravitational field produced by mentation that acted only on the neutrons in atoms and could be shielded by thin layers of metal—a theory that can be faulted on each point either by appeal to general relativity or to the experimental data regarding the independence of psi from metal shields.) The theory was modified from experiment to experiment as he proceeded. The theory was replete with adjustable parameters. (Assuming only that Forwald influenced one of his six cubes on each trial, my results

[7] In a letter to Gardner (June 14,1978), I wrote the following: "On p. 22 [of a draft for the 1982 book], you state 'Walker relies almost entirely on the published results of experiments made over a period of some 20 years by a Swedish electrical engineer, now retired, named Haakon. . . .' I feel this distorts my position somewhat. I think if you were to phrase this a little differently I would not object. 'At the time Walker began his work on his theory, there were very few experiments reported in the literature that had been conducted to test the effects of systematic variation of physical parameters or the results obtained in PK experiments. One such series of experiments made over a period of some 20 years by a Swedish electrical engineer, Haakon Forwald, did seek to ascertain the existence of systematic effects associated with the variation of physical parameters, and the results of these experiments figure prominently in Walker's efforts to check his theory that QM and psi have a common basis' . . . You can then present a discussion of the pros and cons of this work, but please don't pretend I feel that Forwald's work is the superlative example of 20th century research."

are obtained in agreement with experiments with *no* adjustable constants!) Finally, the theory was tested by only one trial series (one data point), which yielded a level of confirmation within the limits of chance. Such results do not support any of Forwald's theoretical conclusion, but Forwald's independently generated data might illuminate some aspects of the physical characteristics of psi functioning. A check of those data against a theory of psi based on tested physical principles (that already had spawned the measurement problem in QM because no less than Einstein had complained that the effects implied telepathy) showed agreement between data and theory. The results support both the theory and the legitimacy of Forwald's experiments. This is the stuff on which firm scientific principles are based. Arguments over personality and possible motives on the part of experimentalists do not lead to scientific advancement.

Unfortunately, Gardner makes many additional disparaging remarks about Forwald's work. Referring to efforts to replicate some of Forwald's earlier placement experiments in the Duke University laboratory and in R. A. McConnell's biophysics laboratory at the University of Pittsburgh, Gardner states, "A disappointed Forwald returned to Sweden. . . . I know of no PK research, over so long a period, that was consistently uncontrolled" (p. 66). The facts are that replication in Pittsburgh was not successful; replication in Durham was. Had J. B. Rhine not been satisfied that Forwald's results were significant, it seems unlikely he would have continued to publish Forwald's work in the *J. P.* Here, we must state that Gardner uses unsubstantiated, unprofessional, rhetorical charges when he fails to make his case scientifically.

> Walker offers nothing resembling a scientific theory. (p. 66)

> To readers unfamiliar with QM, Walker's papers seem enormously impressive because they swarm with equations and scientific jargon that only a physicist could understand. (p. 67)

These comments reflect the personal prejudgments and biases of Gardner as an individual. He is an amateur scientist and popularizer. He is neither a physicist nor a formally educated scientist. His comments must be seen as reflecting such a limitation. Gardner concludes:

> But when it is all translated, and you discover exactly what he is saying, his "theory" turns out to be only a collection of pious hopes. *If* our mind operates by quantum jumps, *if* all parts of the universe are connected on a subquantum level, *if* the human will can alter wave packets of

> distant objects, and *if* it can alter the packets to bring about desired states, then we have an "explanation" of how Uri Geller can bend a spoon. (p. 67, his italics)

Does Gardner suggest that the brain is not governed by the Schrödinger equation? *Does* Gardner really mean to say that the tests of Bell's theorem showed QM to be local? *Does* Gardner really believe that a wave packet can enter one state here and another elsewhere in the universe? (Such would violate conservation of energy, momentum, spin, and so on.) *Does* Gardner really mean to say he has a solution to the measurement problem in QM that frees us of any need to say consciousness (the observer) exists? On all these, Gardner embraces what has already been shown to be wrong.

Millar's criticisms (1978) of experimental tests of the QM theory of psi deal with issues that go beyond the Forwald controversy. We find:

> Walker makes a number of retrospective calculations for particular experiments. In each case they seem rather unsatisfactory. For example, he calculates the distribution of ESP ability in the population and compares this with a sample of about 1,000, culled from the literature, finding at least suggestive agreement. We know well enough that to be published in the early literature results had to be significant and we may reasonably consider that then, as now, there were more insignificant experiments than significant ones, thus the results are not likely to be typical of the general population. Secondly, we know that the role of the experimenter is as critical as the subject's: one experimenter may obtain significant scoring from most groups that he tests, while the majority simply confirm the binomial theorem. Since Walker takes no notice of this in his calculations I cannot take his result very seriously. (p. 319)

But let us examine Millar's reasoning. His first observation is not especially appropriate because all the data for the 1,002 subjects (this was the extent of *all* unscreened group-study data available to me at the time) came from data obtained for group tests. Further, a number of different experimenters were involved in the 13 group studies, 17 in all: Pratt, Woodruff, George, Bond, Sharp, C. C. Clark, A. J. Clark, Price, Pegram, Martin, Stribic, Humphrey, Shulman, Stuart, Carpenter, Phalen, and L. E. Rhine. This is not the sample of selected subjects all tested by, say, J. B. Rhine, as one might have gathered by reading only Millar's comment. Even if the experimenter effect were of the nature Millar implies, the work provides a sufficient sample of experimenters to have a balancing effect for a star exerimenter.

Moreover, in none of the 13 experiments do we find evidence that the experimenter dominates these results. Only a few individuals in each of the group experiments showed significant extrachance scoring. The role of the experimenter is not that of the experimenter versus the total group (unless no breakdown of data on an individual basis is made). Instead, the experimenter-subject (where the subject is one member of the group tested) represents the "cognitive" or "observer unit." This is the reason one does not see in group tests in which there is an overall .05 level of significance, all individuals scoring at the same rate. There will be variations, and these variations can include individuals who score at a much higher level of significance than the average.

To invalidate my results, which are based on a best available data basis, a scientist cannot simply give an opinion. He must establish a fact based on data. This would mean showing by some quantitative argument that the data are misused, or showing better data or more recent data that do not agree with the theory. Millar does neither. His arguments are based on a misunderstanding of the experimenter's role in the group-test situation and a failure to ascertain that a sufficient sample of experimenters existed in my data sources to compensate for the effect of any exceptional psychic experimenter. Indeed, the approximately 100 extrachance-scoring subjects in the population of 1,002 were found to be scattered throughout the reports cited. Millar's contention that probably some insignificant test results were omitted from the published literature is a possible correction tending toward improving the fit between theory and experiment.

In a more recent article, Millar (1979) has written a paper to discuss this issue of the distribution of psi in the population. In his 32-page paper, after criticizing other efforts to obtain the characteristics of the distribution, Millar fails to provide a revision of my estimate of the distribution. It is certainly hoped that someone will improve on the distribution data for the population. Criticisms such as those given by Millar are simply counterproductive.

Another inappropriate charge brought against my work by Millar (1978) is contained in the following excerpt:

> Walker also examines the experiment of Schmeidler with Ingo Swann in which he tried to influence the recorded temperature of an isolated thermistor. He finds adequate scope within his theory to allow the size of effect found: this is rather a pity since the present author has demonstrated that these results were more likely due to use of inappropriate statistical evaluation than to PK. (p. 319)

The charge concerns my treatment (Walker, 1975) of Schmeidler's work with Ingo Swann on PK thermistor effects (Schmeidler, 1973;

see also Schmeidler, Mitchell, & Sondow, 1975). The charge is in error on all accounts:

1. I did not claim to have obtained agreement with Schmeidler's findings.
2. Millar's criticism of Schmeidler's experiment is not valid (see Appendix).
3. Moreover, Millar's conclusion that Schmeidler's statistical procedures would affect the statistical significance to be applied to her results would not have a bearing on the observed magnitude of the temperature effect reported, which was the parameter computed by me. That is to say, statistical significance and magnitude of the physical effect are different parameters.
4. Millar's conclusion is a non sequitur in any event!

Let us examine each of these, taking the last one first. It is obvious that if the theory yielded a result that agrees with experiment, though not obtained by adjusting coefficients, this would strongly point to the theory's validity. But the converse does not hold; it is not a valid logical deduction.

As for Item 1 above, Millar errs when he says I claimed agreement between his calculations and Schmeidler's results. My treatment of Schmeidler's experiment involving thermistors appears in "Foundations of Paraphysical and Parapsychological Phenomena" (Walker, 1975). There the statement concerning the calculated result is:

> In terms of actual indicated temperature the result is still small. For $T = 300°$ K, $N = 10^{12}$ electrons, $\epsilon = 1$ eV, $\tau = 0.1$ sec, $W_{\psi} = 10^4$ sec^{-1}, and $Q = 1/100$, δT_i will be only 2.5×10^{-3} ° C. Again, we must remember that this result depends on noise that has a normal distribution which, of course, omits many realistic effects in circuits. (pp. 40–41)

Agreement was not obtained with the approximately 0.1° C temperature rise Schmeidler reported. Even though agreement was not obtained, Millar, however, apparently felt if it was mentioned, it must be because agreement was obtained. For this reason, Millar assumed that the calculation in the next paragraph applied to the Schmeidler experiment. Specifically, he assumed that the calculation following Eq. A21 (Walker, 1975) referred to Schmeidler's Beckman type R dynograph. It was not so intended. Instead, it was merely given as an example of the use of Eq. A21. However, Millar claims that the data used in this example do fit those for the Beckman dynograph and that the calculated result is in agreement with Schmeidler's experimental findings.

Millar (1975) claims that Schmeidler's experimental design for the handling of her statistics was flawed. Millar makes the following observations:

> (1) The use of a rigid target order, ABBA BAAB BAAB ABBA (where A is randomly chosen to be either "hotter" or "colder") while immune to effects of a linear temperature drift, is susceptible to changes, the rate of increase of which vary with time. A thermistor, especially when enclosed in a thermos flask, is peculiarly prone to just such changes, because the thermistor itself produces heat, and (2) the thermistor-flask combination acts as the temperature analog of a simple RC (resistor-capacitor) integrating circuit, so a temperature step outside the flask results in exponential changes inside.

Millar's verbal charge is not backed up by any calculations. Since nothing in nature is strictly linear, the first charge is simply gratuitous. It could be leveled in any use of a rigid ordering of alternating targets. The question is whether the deviation from linearity is significant. As proved in the Appendix, the deviation from linearity is not significant. Millar's second point seems to imply the existence of a mechanism to cause violent instability in the "circuit" (thermistor-thermos system). In fact, the "RC" behavior simply restores the system to stable equilibrium gradually along an exponential *damping* curve. This is detailed in the Appendix.

There is a further point. Millar's objection to my work was that it agreed with Schmeidler's results. But as pointed out, my result was not calculated to fit Schmeidler's data. Indeed, it is Millar's observation that my derivation accounts for Schmeidler's result. If Millar is correct in stating that paragraph 4, page 41, of my 1975 paper accounts for Schmeidler's result, this has the force of an independent determination of the fit between theory and experiment. If not a predicted agreement, this agreement is something more than a "retrospective calculation," as Millar would have it. Again, the theory appears to be in agreement with experiment, though confirmation of Millar's observation would be required.

From a review of the criticisms by Gardner, and by Millar, we find little to justify the negative conclusions drawn by these critics. It is appropriate, however, to examine the present status of experimental support for the QM theory of psi. First, however, let us look at some issues that arise from the fact that there now exist several alternative proposals for a quantum theory of psi.

Variant Quantum Theories of Psi

There currently exist a number of variations on the QM theme in parapsychology. The works of Costa de Beauregard (1975,1979), Schmidt (1978, 1982), Jahn (1982), Lawden (1979, 1981, 1983), Kornwachs and von Lucadau (1979), Hasted (1979), and Chari (1978), who was perhaps the first to be concerned seriously with the problems confronting a quantum theory of psi, are particularly well known in this regard. The significant number of parapsychologists who have adopted a quantum theoretic approach to the question of the nature of psi phenomena is indicative of the general growth in their acceptance of this approach. However, this very plethora of quantum theories can be as confusing to people in parapsychology as the criticisms already addressed in this paper are. It is therefore appropriate to consider the ways in which these theories represent independent and formal theories, consistent with QM and backed by experimental data, rather than being based on speculations, digressions, and repetitions of concepts already in the literature. Costa de Beauregard has concentrated largely on the significance of paradoxes in quantum theory that highlight the connections between QM and parapsychology. In this he has given that vital support needed for these ideas to take hold and develop.

Both Costa de Beauregard (1979) and Schmidt (1978) have been concerned with the temporal relationships implied by observational theories, the problem raised by Braude (1979). Both Costa de Beauregard and Schmidt take a similar tack in introducing a kind of signal that can move forward or backward in time to show that no intrinsic contradiction need exist in the quantum theoretic treatment of retrocausative effects. This approach, however, is not necessary to the theory. The problem of temporal order disappears in the Heisenberg formulation of quantum theory in the same way that the necessity for nonlocality to be a part of quantum theory is apparent in the Heisenberg formulation. Let us look at the reason for this.

To show that the Schrödinger and the Heisenberg formulations of QM are equivalent, one takes the Heisenberg equations that relate to specific measurement events (not necessarily involving space-time relationships explicitly), introduces the time and location of these measurement events, and sums over all space. This gives a new equation, the Schrödinger equation, that pictures events as occurring in space and time (i.e., solutions are expressed in terms of functions of position and time).

"Wave functions," solutions of the Schrödinger equation, are constructed of orthogonal functions, such that the existence of separate states is maintained within the mathematics. But it is now much more difficult to exhibit the original nonlocality of the basic equation, or the fact that individual states persist as separate states independently of time. One must now exhibit explicitly that two points in space and time are "correlated" to establish that state selection of a component orthogonal wave function here and now will cause the occurrence of a particular value at another place and time. Thus, a simple situation in one formulation of QM can become a formidable problem in another.[8] The point is not to downplay either Costa de Beauregard's or Schmidt's contributions, but to make clear that these neither constitute new theories nor cover an area that would otherwise leave the original theory incomplete. Instead, they provide an alternate way to understand the time independence of state selection.

In a 1982 paper, Schmidt presents a theory dealing with the mechanism of state vector collapse as mediated by a psychic factor, or observer. As far as the principal interests of parapsychology are concerned, this paper does not introduce a new theory of psi phenomena. The paper deals instead with a special technical area of the overall QM theory of psi somewhat differently from my treatment (1979). This subject concerns the actual form the equation for state selection takes. In both our papers, state selection is still caused by the consciousness interaction in state vector collapse; psi results are still limited to the range of states allowed quantum mechanically. One still requires a mechanism to account for psi-missing and a theory of consciousness as I have given. Schmidt's theory is therefore restricted to a special aspect of the overall problem of the quantum theory of psi. Thus, it is not a new or independent theory of psi phenomena. Schmidt (1982) states:

> The first attempt to link PK problems explicitly with basic questions of quantum theory was made by Walker who based his discussion on hidden variable theories. . . . In this model the mind can affect the hidden variables which in turn determine the outcome of the random processes of quantum theory. . . . In the following we will not use hidden variables but rather study how the Schrödinger equation might be modified in the most simple manner such as to allow for a PK effect and an automatic reduction of the state vector under an observation. (p. 568)

Several points must be made to correct what is stated there.

First, my theory is not a hidden variable theory of the type proposed by Bohm, whose purpose was to reintroduce into QM the classical physics concepts of locality and causality, and to remove the concept of the state vector (later included in the Bohm-Bub theory [1966]), all of which my theory requires if psi phenomena are to be explained. Since state selection was to be attributed to something going on in the observer's consciousness, however, it seemed appropri-

[8]This point was the subject of a discussion involving Costa de Beauregard, Mattuck, and me at the Iceland Conference, Nov. 1977, in which agreement was obtained that this problem of temporal connections disappears in the Heisenberg picture.

ate to adopt and modify this term *hidden variable* so that it simply becomes a specific quantifiable reference to consciousness (as I detailed in 1972 [Walker, 1973]). In 1971 in the journal *Physics Today*, I stated: "I refer to a paper 'The Nature of Consciousness' (Walker, 1970) whose results appear to have an experimental basis and suggest a solution to the [measurement problem, which is] . . . essentially a combination of Wigner's conscious observer and Bohm's hidden variables. . . . It indicates that conscious events are associated with, and serve as, hidden variables that cause the collapse of the state vector of every quantum-mechanical event." It should be clear from this that I do not suggest some hidden mechanism that somehow can be affected in turn by the consciousness, but instead that consciousness is the hidden variables that cause state vector collapse. The distinction may seem small, but it is vital to the theory (otherwise, one is obliged to introduce some super-psi concept to explain how the consciousness can control the correct hidden variable to cause the desired event).

Second, contrary to what Schmidt says, the theory he proposes is a hidden variable theory, and moreover, very similar in formulation to that proposed by Bohm and Bub (1966). In the Bohm-Bub theory, two quantities are introduced, one a scalar parameter controlling the rate of growth of the selected state, and the other a vector governing which state is selected. Schmidt also has two parameters: κ, which measures the speed of the reduction process; the other, ϵ, which represents the psychic's control of PK for determining which state is selected. Schmidt hides this selection mechanism by use of only two possible states chosen according to the algebraic sign, $+\epsilon$ for the state selected and $-\epsilon$ for the state not selected. If there exist more than two possible states, a vector parameter would be required to replace ϵ for the scheme to work, and in so doing become more like the Bohm-Bub theory.

Unlike Bohm and Bub, however, Schmidt does not use his equations to actually carry out state selection to completion, but only to produce a bias in the states. This is, however, a fault. The only justification for introducing the observer into the process is that quantum theory requires some "mechanism" to produce state vector collapse. Schmidt introduces the observer with the postulated capability to alter state probabilities, but leaves the problem nebulous as to what further process must be postulated to carry out state vector collapse. Parsimony would demand more of our hypotheses.

Finally, Schmidt's Eqs. 24 and 26 are not compatible with the requirement of Eq. 17a. Thus the theory has some mathematical difficulties.

Chari's (1978) position has previously been discussed by me (Walker, 1978).

Hasted (1979) discusses another variant quantum theory. In his "parallel universe" model, the relative state interpretation of QM attributable to Everett (1957) is used to account for psi. Everett showed that QM could be understood without introducing an observer by assuming that every potential state actually occurred physically, as a multiplicity of simultaneous noninteracting universes. In Everett's theory, there is no need to introduce observers, and no account is made regarding consciousness, although he does show that observations would be consistent with the predicted results of the conventional interpretation of quantum theory. For Hasted to reintroduce consciousness, however, is simply to take us back to the usual Copenhagen interpretation. Is there a real difference between saying that the observer selects states from a list of potential states and saying that the states are all real but that the observer selects which state he is to observe? To this extent, Hasted's theory is really the same as mine.

However, to account for teleportation of objects, Hasted has modified Everett's interpretation to allow time delays as objects enter other universes. In this way, an object might make the transition to another universe at a different time from the object's container, allowing for a translation of the object to the outside of the container walls. The effect would look like teleportation of the object through the container walls. Such effects can occur in my theory, but they should be much more rare than Hasted expects. The ease with which one can account for more frequent teleportation effects in Hasted's theory, however, may be entirely due to the fact that his theory has not been developed in a mathematical form. The requirement that such a mathematical theory retain the conventional predictions of the Schrödinger equation might impose such conditions on the mathematics of a theory with time-delayed transitions that no significant advantage could be achieved. Without an explicit exposition of the mathematics, Hasted's theory remains a variant description of mine.

Lawden (1979, 1981, 1983) has offered an interesting approach to modifying the Schrödinger equation to account for psychic phenomena. Lawden's approach has much in common with mine, but he introduces a different modification to the Schrödinger equation from my change (Walker, 1979) to formalize the mechanism of state vector collapse. As with Schmidt's approach (1982) to this problem, these mathematical issues, though of fundamental importance to the physics of the problem, represent somewhat of a side issue to the basic interests in parapsychology. In the long run, predictions stemming from a study of this aspect of psi phenomena might lead to physical devices that manifest psi effects. They will, however, amplify the connection between QM and psi phenomena, not alter the fundamental basis of that theory.

Robert Jahn (1982) presents still another approach to how consciousness should be brought into QM. Fundamentally, Jahn considers the same quantum theory of psi as I do, but with a totally different slant as to the method for incorporating consciousness in the overall phenomenology. He proposes that consciousness be treated by the same equations used to compute measurable quantities for physical systems. Thus, if we wish to know the energy in the ground (lowest energy) state of an atom, we would write:

$$H\psi_1 = E_1\psi_1 \tag{3}$$

where H is a mathematical "operator" that can be constructed for specific physical systems such as a hydrogen atom. More generally, the ith energy level is given by

$$H\psi_i = E_i\psi_i \tag{4}$$

If I wanted to obtain an expression for the angular momentum carried by an electron in an atom, there exists another equation in QM very similar to Eq. 3 above:

$$A\psi_i = a_i\psi_i \tag{5}$$

where again A is an operator that can be constructed for the particular problem, ψ_i is as in Eq. 4 above, and a_i is the ith constant giving the angular momentum for the ψ_i configuration of the atom. Jahn proposes a similar approach be taken as regards consciousness, in which for some state ψ_i describing the consciousness of an individual there will be a measurable parameter (such as a "call" in an ESP experiment) s_i that can be generated from the appropriate operator S:

$$S\psi_i = s_i\psi_i \tag{6}$$

Jahn then points out that for many strongly interacting systems, as in the bonding of one hydrogen atom, a, with a second hydrogen atom, b, the resulting energy levels for the combination molecule, ψ^{ab}, of the two atoms will differ from simply the sum of the two energies of hydrogen atom a and hydrogen atom b. Jahn then speculates that a similar situation may arise in an ESP experiment in which Eq. 6 is used to describe the states of the combination of the percipient and agent. Here, Jahn states, "If the percipient and agent are strongly enough interacting to require a new 'molecular' wave function, ψ^{pa}, 'paranormal' terms will appear in their response pattern" (p. 160).

This *program* for developing a quantum theory would be more meaningful if Jahn had specified that Eq. 6 referred to brain states rather than states of consciousness. Although it would be difficult to construct any operator S for the whole brain, at least there the fundamental principles exist for understanding what his statement means. For S to refer to a consciousness operator leaves Jahn's approach totally undefined. Further, the reason the combined hydrogen atoms yield significantly different energy levels is because of the strong electromagnetic forces (or signals) that interact between particles in atom a and particles in atom b, which do not occur when these atoms are widely separated, and because of "indistinguishability," the fact that hydrogen atom a cannot physically be distinguished from hydrogen atom b. But in an ESP experiment, we specifically set things up so that there will be no signals (electromagnetic and other interactions) between percipient and agent. And further, percipient and agent certainly do not satisfy the requirements of indistinguishability. Thus, the analogy breaks down. Jahn's program for the development of a new quantum theory of psi does not satisfy any of the basic requirements for such a theory, and Jahn does not carry his proposal far enough to consider any quantitative results of his theory, or experimental tests of the theory.

Mattuck (1979) has presented a modified version of my theory. It differs principally in its method for estimating the amount of psychic information available to select a quantum mechanically available target state. I have been more inclined to use only the values of $\sim 10^4$ bits/s for the psi channel data rate and $\sim 10^8$ bits/s for the noise source, letting the results fall where they may as to the adequacy of fit between theory and experiment. Mattuck has used various calculational methods having a less clear connection with the subject's mental processes in order to obtain numbers large enough to account for some PK effects. It is not clear that any well-documented laboratory PK effect exists requiring a psi channel data rate greater than about 10^4 bits/s, however; so the need for other approaches to obtain high data rates is unclear. Otherwise, Mattuck's approach is substantially the same as mine.

Kornwachs and von Lucadau (1979) present a theory that is not necessarily in conflict with the QM theory, but could serve well as an adjunct theory to bridge concepts formulated at a very fundamental physical level to psychological theories such as those offered by Stanford and Braud. Kornwachs and von Lucadau do recognize that "Walker understands the hidden variables which determine the collapse [of the state vector] at the moment of measurement as representative of consciousness" (p. 327). But they then err in their understanding of how "hidden variables" work. Physically, my "hidden variables" are truly hidden and unmeasurable (as these terms are used in physics). But these "hidden variables" are the stuff of which consciousness is made, and therefore knowable, and quantifiable. Consciousness is not "observable" (as this term is technically used in physics), but is the stuff of the observer that causes state selection. Contrary to Kornwachs and von Lucadau, the interpretation can be verified directly. Indeed, this is exactly what we do in an ESP experiment, compare intention of the will with events that actually occur.

Kornwachs and von Lucadau (1979) also state:

> If we assume that paranormal phenomena have to do with the so-called human "psyche" it is justified to ask what is the difference between human being and another physical system like a quantum mechanical apparatus? Using Walker's conception of consciousness, represented by hidden variables and deciding each measurement process, it seems to be difficult to find an answer, especially if one assumes that physics works in absence of living beings and their consciousness. (p. 328)

These writers then refer to the work of Smythies (1967), who proposes a complexity field in order to get a carrier process for the transfer of paranormal information. But Kornwachs and von Lucadau's words can be used against this proposal as well. Are we not to assume that the laws of physics will work on complex systems as well as simple systems? But the real answer to Kornwachs and von Lucadau's question appears in the same journal and issue as their paper. I show (Walker, 1979) that the real mechanism for state vector collapse springs from a certain complex (as it were) process of measurement completion, that is, the comparison of two separate measurements, each being carried out on the process conjugate to the other (for example, in an Einstein, Podolsky, Rosen experiment, the measurement is complete when one experimenter compares the results from the two separate measurements used to demonstrate the remote correlation of states). Because such a process occurs rarely in nature, but on a vast scale in the brain, this process, not complexity, distinguishes human observation as being unique to physical processes. Thus, though the text was not available to Kornwachs and von Lucadau at the time they were writing, the theoretical basis on which to answer this question had been developed by me. It is not appropriate to review this matter here, but the measurement loop explanation of state vector collapse (in which state selection still resides with the consciousness of such loop systems) provides a natural physical basis for understanding this effect within the formalism of quantum theory, whereas the "complexity" idea is ex post facto. More objectionable, however, is Kornwachs and von Lucadau's statement, "In extending the theory of Walker to the experiment of H. Schmidt one encounters a lot of difficulties" (p. 336). What is bothersome is that Kornwachs and von Lucadau then give on the same page exactly the same logic (state selection from the linear superposition of states) as given by me (Walker, 1973) to account for the PK phenomena.

Overall, we find that the many writers who have contributed to growth of the quantum theory of psi have, for the most part, dealt with the same basic theory. They have often illuminated or contributed new insight to the theory, but we should not view these efforts as conflicting. For one writer's idea to be accepted does not mean all other ideas are to be rejected. Indeed, two apparently different approaches to the same issue may both prove valid.

Present Status of Experimental Tests of the Theory

The QM theory of psi phenomena is compatible with physical theory and the results of prior experiments. Further, it has been tested by experiments conducted since its introduction. Let us first state the status of agreement between theory and experiment existing at the time the theory was introduced and then discuss experimental results anticipated by the theory. In this survey, let us include the theory of consciousnes and of the nature of synaptic functioning, for these provide general support for the theory as it specifically relates to parapsychology.

A. Retrospective Agreement Between Theory and Experiment

Listed below are specific points of agreement between theory and experimental data. For more details consult the referenced papers.

Consciousness Theory (see Walker, 1970)

1. Synaptic cleft thickness. For the energy available at the synapse, 70 meV, and materials of which it consists, QM does not support tunneling over distances greater than about 180 Å. Neurons of the central nervous system have synaptic clefts of about 180 Å.

2. Morphology of synapses. For QM tunneling to cause synaptic firing, patches of donor and acceptor molecules must face each other across the synaptic cleft. Such a structure occurs in synapses.

3. Characteristic time. The temporal extent of conscious experience is not infinitely divisible. The minimum temporal extent of a consciously distinct event lies in the range from 0.001 to 0.1 s. The theory accounts for this temporal extent.

4. Characteristic length. Our consciousness incorporates brain data-processing functions that take place in different parts of the brain. For example, color discrimination takes place in an area of the brain quite distinct from the area in which the corresponding object's shape is discriminated. Conscious experience brings together as an entity information contributed by brain processes separated by some 10 cm (i.e., the dimensions of the brain). The theory accounts for this "spatial extent" of consciousness.

5. *Intersynaptic tunneling constraint.* The requirement that synapses at different locations in the brain are coupled quantum mechanically can be satisfied only under special conditions. That these conditions are satisfied by the brain can be checked quantitatively using the theory. The test requires that there be a sufficient supply of large molecules (soluble RNA) to allow intermolecular tunneling in picoseconds. This condition is satisfied.

6. *Consciousness onset mechanism.* Consciousness involves some mechanism associated with brain functioning that initiates as a precipitous onset process (as when we wake up). The change in the level of neural and synaptic activity associated with consciousness onset is less than 50%. The theory accounts for the small change in the level of brain activity required for onset of consciousness.

7. *Consciousness data rate.* The consciousness experience involves a data channel capacity that can be quantitatively determined either by determining the rate of sensory data input that can be consciously experienced, or by appeal to an introspective examination of consciousness information content. Both approaches give a value of about 10^8 bits/s. This data channel capacity can also be calculated based on the characteristics of the consciousness theory. The calculated value is also about 10^8 bits/s (see Walker, 1979).

In addition, the theory yielded the "will" data rate that has since been used in the QM theory of psi. In the above list, no adjustable coefficients were used. All required coefficients were evaluated by reference to measured data (such as the number of synapses in the brain, concentrations of soluble RNA, and so forth).

Although the QM theory of psi is not dependent on the validity of this consciousness theory, it does require some theory that relates consciousness to QM processes involved in the data-handling brain functions.

Synapse Theory (Walker, 1977c)

1. *Unified theory of synapse-ephase junctions.* Morphologically, synapses, which use chemical mechanisms for transmission, and ephases,

which use electrical coupling, are nearly identical. The theory accounts for similarity and morphological difference between these types.

2. *Cleft thickness of ephapse, conductivity of the ephapse.* The detailed properties of the ephapse can be calculated using the theory.

3. *Energy involved in spontaneous vesicle release in mammalian (cat) synapses.* The energy expended in vesicle content release has been measured and agrees with the theoretical value.

4. *Energy involved in spontaneous vesicle release in amphibian (frog) synapses.* The value of the energy expended in vesicle release in amphibians differs slightly from that in mammalian synapses. The theory accounts for this difference.

5. *Miniature end-plate potential (MEPP) frequency as a function of hyperpolarization and depolarization.* At a synaptic junction, there is an ongoing stochastic release of the chemical transmitter contents of the vesicles. The average rate of the resulting MEPPs is accounted for quantitatively for both hyperpolarization and depolarization driving potentials applied across the synaptic cleft.

6. *MEPP frequency variability.* Most hyperpolarization-depolarization curves are linear; others are not. Variation both in kind and in magnitude are accounted for by the present theory.

7. *Osmotic pressure effect.* Addition of solutes alters the osmotic pressure, causing changes in the MEPP frequency. The theory accounts for this process qualitatively and quantitatively.

8. *Functional relationship between vesicle release probability and delay time.* Depolarization of the synaptic cleft leads to MEPP quantal events (vesicle release). The time-delay probability distribution is accounted for by the theory.

9. *Temperature effect on time delay for vesicle release.* The effect of temperature on the vesicle-release probability versus time-delay function is given by the theory.

Both Items 8 and 9 are given without introducing arbitrary adjustable coefficients. Only two constants had to be evaluated from the experimental data to allow the computation of all the other derived results listed above. These were an integer giving the number of macromolecules forming a vesicle gate (the value obtained was 9) and an evaluation of the potential energy barrier height V_0 in Item 5 above. The result, 0.118 eV, however, can be independently checked, for it must fall in the range of values allowed for dielectric materials. The resulting value of 66,000 volts/cm is typical of good dielectrics.

The validity of neither the QM theory of psi nor the quantum theory of consciousness depends on the validity of the above theory of synaptic functioning. Nevertheless, there must exist some QM process that involves in an essential fashion those brain functions tied to data processing.

Psi Theory (Walker, 1973, 1975, 1979)

1. Telepathic coupling of observers. Telepathy is accounted for by observer state selection of a state having an experiencible correlation between target lists and calls. There is also a further requirement that all observers enter the same observed event, which serves as a coupling constraint on all observers.

2. Clairvoyance phenomena. Since the theory does not require any radiated signal from one brain to another, a single observer can cause selection of a correlation between a stochastically prepared state and the observer's target state. Thus, states exhibiting correlations between items describing physical objects (symbol lists in a deck of cards) and brain states (giving rise to symbol calls) can be biased to occur by observation. Again, this process does not depend on spatial separation.

3. Precognition phenomena. Since the process of state selection has spatial independence as a characteristic (see Item 8 below), the physical principle of Lorentz invariance in physics requires a corresponding temporal independence. This simply means that the time at which the target configuration within a state is prepared and the time at which the various observer events bring about state selection do not enter as a physical parameter determining the state selected. The difference between telepathy and precognition is primarily an arbitrary labeling distinction.

4. PK phenomena. These phenomena result from observer selection of a QM state containing the targeted physical configuration from the allowable physical configurations as described by the state vector for the system. Whereas in telepathy the states describe possible brain states that result in the call sequence, in PK the states describe physical arrangements or conditions of objects from a quantum mechanically allowed collection of physical configurations.

5. Stochastic character of psi phenomena. This aspect of psi phenomena stems from the basically statistical character of QM. More specifically, it is tied to the signal-to-noise constraint affecting psychic performance.

6. Psi effect "penetration" of Faraday shields. Because psi effects occur on state selection upon observation, as, for example, when a target list is compared with the call list, the presence of a shield separating, say, a "sender" from a "receiver" (in a telepathy experiment) can have no effect. Thus, the absence of a shielding effect from a Faraday cage is accounted for.

7. *Psi effect "penetration" of magnetic field shields.* The absence of a magnetic shield effect is accounted for in the same way as the absence of an effect from Faraday shields.

8. *Spatial independency in psi experiments.* Space independency follows directly from the nonlocality property in QM. The process of state selection is not dependent on the spacial separation of parts of the QM states, which has been established by tests of Bell's theorem.

9. *Goal-directed character of psi phenomena.* Complex targeting procedures, such as use of calculations (as in REGs), can be used without inhibiting psi capability. The state selection process is dependent on the observed state and not the procedure for setting up the potential states (state vector).

10. *Improvement in PK dice experiments with increasing numbers of dice.* Larger numbers of dice increase the chance that dice collisions will have occurred a sufficient number of times so that the Heisenberg uncertainty in dice orientation will have led to macroscopic potential states. The resulting state vector is collapsed by the observer to obtain the target state.

11. *Distribution of psi ability in the population.* The theory incorporates the existence of signal-to-noise effects that determine the overall level of success in psi tasks. By the application of principles that have been successful in learning theory, it is possible to show that there should exist a distribution for psi success for task-naïve subjects. The theory accounts for the overall success rate and the type of distribution of psi ability that occurs in the population.

12. *Forwald placement experiments.* The theory accounts for the existence of placement effects, their magnitude, and detailed characteristics of these effects.

13. *Sheep-goat effect.* The sheep-goat effect is essentially a corollary to the goal-directed character of psi (Item 9 above) in that state selection is correlated to consciousness states (differences in biasing attitudes can give rise to different conscious states to effect state selection differentially). Where conditions are met such that "sheep" yield positive correlations, those with a negative attitude toward achieving target selection will attain their target by biasing against the experimenter's target.

14. *Magnetometer experiment at Stanford University.* The magnitude of the PK effect on the Stanford University magnetometer observed by Puthoff and Targ (1975, pp.130–132; see especially their Figures 2

and 3) with Ingo Swann agrees with theoretical calculations, as I have previously shown (Walker, 1975).

15. Equivalence of ESP and PK. Because both processes are explained by the same mechanism of observer state selection, under equivalent conditions of observation subjects should perform as well at one task as they do at another. For detailed discussion of this point, see Walker (1977a). See Schmidt and Pantas (1972) for specific evidence of this requirement of observational theoretic results.

B. Predicted Results

Now let us turn to results predicted by the theory. Unfortunately, there have been no further experimental studies that would have led to tests of either the consciousness theory or the theory of the synapse. This is especially unfortunate in the case of the synapse theory because a list of predicted experimental results was specified in the original paper. However, there have been several parapsychological tests of the theory. In some cases, the predictions have been made in private correspondence with the experimenter, and in other cases they are specifically reported, as is preferable. Nevertheless, in all cases the predicted results are based on the overall requirements of the theory as they were first presented in 1972. The predicted tests of the psi theory are as follows:

1. Feedback. Feedback to a subject is not necessary for success in an ESP experiment (Braud, 1978; Houtkooper & de Diana, 1977; and private communications to D. Biermann, 1972, and Puthoff, 1977).[9] The prediction comes from the requirement that all observers are constrained to enter the same state on state selection (state vector collapse); this constraint constitutes a space-time independent coupling of observer consciousness states.

2. Signal-to-noise ratio. Results obtained by a subject tested for PK ability on various devices will yield constant signal-to-noise ratios (Walker, 1975, Eq. 28 and discussion in Sec. VII; confirmation reported by Puthoff & Targ, 1975, p. 139 par. 3; predictions and experimental results both presented at the 1974 Geneva conference). The prediction is based on the fact that psi does not depend on

[9] This point was discussed with Hal Puthoff, SRI, in 1977. Subsequently, Puthoff and Targ carried out remote-viewing experiments with H. Hammid, 1977, with no feedback, which yielded positive results. A subsequent experiment by Puthoff and Targ with computer-generated targets that were immediately destroyed without feedback yielded significant results for two of three subjects. A remote-viewing session with Pat Price also yielded a good target description, although he died before receiving any feedback.

energetic effects or mechanisms as such, but only on state selection. Signal-to-noise in state selection, however, does depend on state of consciousness.

3. Retro-PK. Retro-PK is the first entirely new phenomenon discovered in parapsychology that was predicted by theory. The observational theoretic character of the theory based on the proposal that the observer causes state vector collapse is the source of the prediction and dates from 1972 (Walker, 1973). It was stated as follows: "Psychokinesis is a process in which the hidden variables of the observer determine the collapse of the state vector for a quantum mechanical system with macroscopically diverse potential states" (p. 52, par. 3). This was subsequently observed experimentally for the case of macroscopic potential states, which were represented by QM-generated acoustic events recorded on audio tape by Schmidt (1975b; see also Schmidt, 1976).

4. Pseudo-RNG tests. The theory predicts that, except for the selection of a "seed" number or the selection of the algorithm, pseudo-RNGs cannot be influenced by psi, whereas QM RNGs are susceptible to psi influence. The prediction follows from the theory, which states that it is only QM states that are subject to psi selection. Given the seed number, a pseudorandom number sequence has only one state and is thus not subject to differential state selection. A further discussion of this is made in my treatment (Walker, 1977b) of the Hardy-Harvie-Koestler (1973) experiment. I discussed this specific point with Dr. Ed May (private communication) prior to experiments to test this effect. The experiment using QM RNG gave positive results (May, Humphrey, & Hubbard, 1980). Their experiments using pseudo-RNG gave results attributable to seed number selection (private communication, 1982). Experiments by Schmidt (1981) have also given results in keeping with the predictions of the theory. As Schmidt's theory (1975b) is not based on QM, these experimental results fail to give support to his "mathematical theory." That theory only requires that the target sequence be statistical; according to the mathematical theory, a psi source will alter the statistics observed, whether of QM origin or not, contrary to present experimental results. Note that if nondeterministic seed numbers are selected frequently, or involve a sufficiently long string of numbers, psi effects can still arise. Adequate pseudo-RNG experiments with the variable of seed number selection entirely eliminated have not yet been carried out.

5. Future observer effects. The requirement that nonlocality be made Lorentz invariant (i.e., the requirement that space and time appear in a theory in an equivalent manner) means that the nonlocality of Bell's theorem must be matched by a temporal independence for observers of a given system. This requires one to treat secondary observers as potential psi sources as much as one does the original observers. Thus, reobservation by a psychic can have an effect on a parameter as yet unanalyzed (an orthogonal parameter). This prediction is explicit in my 1975 paper as it relates to state vector collapse and particularly as it concerns the time-independent coupling of multiple observers. A specific discussion with Bierman preceded an experimental test of this prediction conducted by Weiner and Bierman (1979). Weiner (1982) has written a survey study of eight experiments bearing on this theoretical prediction. Four of the eight give significant results deviating from the null hypothesis and in favor of the theory. Two more yield results in agreement with the theory, but all parts of the experiment did not yield the statistical significance adequate to discriminate the results from certain alternate explanations. A seventh experiment, which used a pseudo-RNG to generate events, yielded no statistically significant results, which agrees with the predictions of the theory (see Item 4, above). An eighth experiment yielded only chance results. Complete falsification of the theory (or an appeal to a general lack of psi ability on the part of the "subjects") would have been required had all experiments except that with the pseudo-RNG failed to give extrachance results. (Note that agreement with the theory here does not mean that these experiments explored and tested all aspects of the future observer issue. Much certainly remains to be tested. Overall, however, these results give evidence that there is a future observer effect).

In a recent experiment, Schmidt (1984) gives a new experimental test of the future observer hypothesis. The experiment was designed to discriminate between predictions that multiple observers are involved in state vector collapse as opposed to observational theory predictions (see Schmidt, 1982) that only the first observer causes state vector collapse. The hypothesis that only the first observer collapses the state vector (so that a second observer's bias plays no role in state selection) limits the kinds of psi phenomena predicted to occur. In particular, precognition could not occur for any pseudorandom or nonrandom targets. The subject would obviously be the first observer for his calls, collapsing the state vector to a single random sequence before the target state is available to make correlations possible. Further, since the target sequence is pseudorandom, no state vector would form that could be collapsed by the checker. Therefore, statistically significant precognition on pseudo- or non-random targets is not permitted according to a first observer interpretation of the

QM theory of psi or observer theories. Similar arguments could be made to show that a first observer limitation is just as restrictive for the phenomena of telepathy and clairvoyance. Thus, we are led to predict that Schmidt's (1984) results will not be confirmed, that precognition is not possible for pseudorandom or nonrandom targets, or that QM theories of psi are not valid.

6. *Decrease in scoring with increasing feedback rates.* See the detailed discussion of this effect in my 1976 paper. Experimental support is given by Schmidt (1973); see also Millar (1978, p. 319). Millar is to be credited with matching the experimental data to the theory.

7. *Linear increase in scoring with increased time (repetition) of observation.* See my prior discussion (Walker, 1975; time dependence in Eq. 28). Confirmation is given by Schmidt (1976); see also Millar (1978). Contrary to Millar's claim, the result to be expected from Schmidt's formula requires more detailed consideration.[10]

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[10] Schmidt's formula for psi source enhancement of event probabilities (Schmidt, 1975b) is not time dependent. However, Schmidt can interpret each observation as contributing a psi factor θ_i so that n repetitions of an observation would yield

$$p' = \theta_1\theta_2 \ldots \theta_n p/(q + \theta_1\theta_2 \ldots \theta_i \ldots \theta_n p) \tag{fn1}$$

where $q = 1-p$. If we set

$$\theta_i = 1 + e \; ; \; e << 1 \tag{fn2}$$

we obtain, approximately

$$p' = p + neq\, p \tag{fn3}$$

which shows explicitly the increased effect owing to the successive observations. Here p is the psi unmodified probability, and p' is the observed probability at the end of the sequence of observation. The enhancement, proportional to the number n of observations, thus, can give Schmidt similar results to those predicted by me. However, if there is a *second* reference tape that is not subject to repeated observation, that tape should yield on checking a probability p_{ref} given by

$$p_{ref} = \theta_{ref}\, p/(q + \theta_{\mathrm{ref}}\, p) \tag{fn4}$$

which should not equal p', a result contrary to the predictions of the QM theory of psi. Note that the sequence of observations giving

$$p_1' = \theta_1\theta_2 \ldots \theta_m\, p/(q + \theta_1\theta_2 \ldots \theta_m\, p) \tag{fn5}$$

and

$$p_2' = \theta_{m+1}\theta_{m+2} \ldots \theta_n p_1'/(q_1' + \theta_{m+1}\theta_{m+2} \ldots \theta_n p_1'); \; q_1' = 1-p_1' \tag{fn6}$$

is permitted by Schmidt's formula and must hold if Schmidt is to be able to account for repetition enhancement without appeal to QM.

This says that m repetitions of observation by a subject of a given tape in Schmidt's

8. Measurement of psi channel data rate. The will channel capacity can be measured experimentally by use of *pseudo*remote viewing (or *pseudo*ganzfeld) testing procedures in which degraded images (photographs) are used to elicit target description transcripts. This procedure has been used successfully by Schlitz and by me (see Walker, 1983b).

In addition to the above list, we should add some of the experimental work by Stanford (e.g., Stanford, 1978). This theory, couched in psychological terms, is quite in conformance with the basic principles of the QM theory of psi, and thus should not be thought of as a competing theory, but rather as a model of the psychological aspects of goal-directed state vector selection by a conscious entity.

Similar comments are also appropriate for Braud's work "Lability and Inertia in Psychic Functioning" (1981a). The term *lability* as an indicator of a physical system's susceptibility to alteration is a more elegant expression than *stochasticality*, the equivalent term from the QM psi theory point of view. Braud (1981b) also offers some interesting data on the relationship between psi performance and autonomic nervous system activity that relate directly to the question of the signal-to-noise ratio (*W/C*, measure of overall psi success in the QM psi theory; Walker, 1975) for various levels of arousal. The ratio *W/C* is a function of arousal (as measured by the consciousness data rate capacity *C/C* for a given conscious state), specifically being propor-

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retro-PK experiment should yield a result p_1'. With additional repetitions, $m + 1$ to n, the enhancement should *differ* from that *already* observed, so that

$$p_2' \neq p_1' \qquad \text{(fn7)}$$

This must hold in Schmidt's theory even where Schmidt requires the effect of an observation to be time independent. Only if Schmidt modifies his theory so that it embraces the restrictions imposed by QM can this result be avoided (see Schmidt, 1982, in which such a revision is offered); in that case, however, the theory becomes a variant QM theory with predictions contingent on ad hoc assumptions. My theory is not compatible with the result in Eq. fn7. Such a future observer effect is possible in my theory only if an orthogonal observable is measured in the second observation. If p_1' and p_2' are pobabilities measured in the same way on the same tape, the Walker theory requires

$$p_2' = p_1' \qquad \text{(fn8)}$$

This result for the QM theory is easily understood in terms of the requirements of QM because state vector collapse is global (nonlocal) affecting all correlated elements of the system. Because Schmidt's theory is not a theory tying QM to psi, it does not have such a reference state. Therefore, according to Schmidt's 1975 theory, each tape must be treated as though a separate experimental run had been conducted, and these should yield, according to Schmidt's theory, differing results. This is readily testable experimentally.

tional to log *(C)/C*. Thus, in an ordinary dream state in which the value of *C* is lowered from that in the waking state, the signal-to-noise level *W/C* rises. These statements refer specifically to comparisons between different levels or modes of conscious activity and do not refer to heightened consciousness owing to increased synaptic functioning above that required for consciousness onset; for details, use my equations for *W* and *C* to compute the signal-to-noise ratio (1975, p. 16).

I have just listed 39 tests of the overall QM theory of consciousness and psi phenomena.[11] I am not aware of experimental results that would contradict the theory.

For those of us who recognize that not only psi phenomena but consciousness as well require scientific understanding, we know a successful theory must tie these phenomena to the rest of science. At the very heart of the impetus to develop general relativity was the recognition that the inertial mass (the mass in Newton's equation $F=ma$) was identical to the gravitational mass (the mass that appears in Newton's equation for the gravitational force). This simply could not be a coincidence. In parapsychology, we discover "observer effects" as a general way of talking about psi phenomena. In physics, we encounter the "observer problem" in which QM imposes the incredible conclusion that the observer has an effect on QM systems. This simply cannot be coincidence! This must lie at the heart of the solution to the problem of psi phenomena; and, indeed, an understanding of psi phenomena and of consciousness must provide the basis for an improved understanding of QM.

It is equally clear that if QM is tied to psi phenomena and to consciousness, QM processes must be at work in brain functioning. Given that the processing of information in the brain, the information that makes up our conscious experience, is handled by those data-processing switches of the brain's computer, the synapses, it becomes equally clear that QM must play a role in their functioning. Thus, we see that just as the equivalence principle is tightly linked to general relativity, so too are the requirements linking QM, synaptic functioning, consciousness, and psi phenomena.

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[11] It may be worth noting that general relativity became accepted based on the results of only four experimental tests: the Eötvos experiment for the equivalence of the gravitational and inertial mass of objects, the gravitational red shift, advance of the perihelion of mercury, and the deflection of light by the sun. Two of these effects were well known before the theory was proposed, and a third, the gravitational red shift, is regarded as a test of the equivalence principle, a principle common to several theories, as opposed to a specific test of Einstein's theory.

Conclusions

There are at present a large number of "theories," hypotheses, concerning the nature of psi. The number can grow essentially without limit, and avail nothing. Theories have a value only to the extent that they provide a consistent understanding of experimental data. In this regard, most theories have explained almost none of the detailed data developed by parapsychologists, and few have even attempted to provide a tie-in with the established body of science. The QM theory of psi has done both.

The complexity of the theory, however, has led to considerable misunderstanding about what the theory says. I have made some effort in the present paper to clarify the theoretical concepts and resolve these misunderstandings. I have also made an effort to clarify the situation regarding Millar's arguments against Schmeidler's thermistor experiment with Ingo Swann. Finally, it has been pointed out that the overall QM theory of psi enjoys substantial support from experimental data.

Critics who have long argued that the experimental evidence for psi cannot be accepted because it conflicts with scientific theory have now been answered. But the professional critic who is less interested in scientific progress than in promoting his own bias now must argue that theoretical endeavor should be postponed until there is acceptable experimental evidence for the phenomenon. Gardner (1982) quotes Sherlock Holmes' admonition that theory making should be delayed until one has data. But the data are there for those who have availed themselves of the facts—the facts of physics and the data of parapsychology. QM is well understood by those educated in physics, and psi phenomena have been elucidated in a thousand experiments. The only thing that has been lacking is a rapprochement between parapsychology and the main body of science. That is what the present theory provides.

But it requires more than telling someone what the facts are. A critic like Gardner must bring something more to the scientific enterprise than a sharp tongue. The habit of merely seeing must be augmented by the ability to observe. One must have a knowledge of what is already established in science, or explanations of new phenomena will enter blind eyes and fall on dumb ears. One must have a faculty for observation. It is with this faculty of observation that we build scientific understanding.

Appendix

The equation governing the thermal effects as driven by thermistor heating from inside a thermos bottle is the following:

$$C_H \frac{dT}{dt} = V^2/R - A[\alpha\sigma(T^4 - T_o^4) + k'(T - T_o)] \qquad \text{(A1)}$$

where:

- C_H = heat capacity of the interior elements of the thermos (glass, air, thermistor, and attachments to the thermistor)
- T = temperature inside the thermos (°K)
- t = time
- V = voltage on the thermistor
- R = resistance of the thermistor
- A = area of the thermos inner bottle
- α = reflection coefficient of the inner bottle
- σ = Stephan-Boltzmann constant
- k' = heat conductivity constant for the inner wall of the double-wall thermos bottle
- T_o = initial temperature at time $t = 0$.

Writing

$$T = T_o + \delta T \qquad \text{(A2)}$$

we have on expanding the radiative term

$$T^4 - T_o^4 = (T_o - \delta T)^4 - T_o^4 = 4T_o^3\delta T + 6T_o^2\delta T^2 + 4T_o\delta T^3 + \delta T^4 \qquad \text{(A3)}$$

Comparing the quadratic term to the linear term gives a ratio of 1.5 $\delta T/T_o$, which for the conditions of Schmeidler's experiment is always less than 1%. Therefore, we may neglect the terms higher than the linear term. Defining

$$k = k' + 4\alpha\sigma T_o^3 \qquad \text{(A4)}$$

we can write

$$\frac{dT}{dt} = (V^2/R + kAT_o)/C_H - kAT/C_H \qquad \text{(A5)}$$

The temperature dependence of the thermistor itself can similarly be incorporated into the linear term.

The solution to Eq. A5 is

$$T = T_o + \frac{V^2/R}{kA}\left(1 - e^{-kAt/C_H}\right) \qquad \text{(A6)}$$

Experiments were conducted with a thermos of the type used by Schmeidler giving the thermodynamic constants of importance, yielding

$$kA = 0.0626 \text{ J/s °K} \quad \text{(A7)}$$

and

$$C_H = 151.5 \text{ J/°K} \quad \text{(A8)}$$

Thus, the characteristic time for the damping of the temperature is

$$\tau = C_H/kA = 2419 \text{ s} = 40.3 \text{ min} \quad \text{(A9)}$$

In the bridge circuit used by Schmeidler, a voltage of from 1 to 10 volts was placed across the target thermistor† of resistance 15 Ohms at ambient temperature in series with a 1000-Ohm resistor outside the thermos bottle. Thus, the power into the thermos was from 1.46×10^{-3} to 1.46×10^{-5} watts. Since the largest effects occur for the higher impressed voltage, we calculate the remaining results for 10 volts impressed across the resistor-thermistor combination in the bridge circuit.

At equilibrium (eq), the temperature increase reached will be

$$T_{eq} = \frac{V^2}{kAR} = 0.024°\text{K} \quad \text{(A10)}$$

By integrating Eq. A6 over a time interval Δt, we can obtain the temperature rise the thermistor contributes to any given trial. Summing over the entire experimental run, using a sign coefficient S_{gn} defined according to

$$S_{gn} = \begin{cases} +1 & \text{“A” trial} \\ -1 & \text{“B” trial} \end{cases} \quad \text{(A11)}$$

in the ABBA - BAAB - BAAB - ABBA sequence of trials, each of length Δt with pauses between trials of length t_p, we obtain for the average bias temperature on *each* trial, T_{bias},

$$T_{\text{bias}} = \frac{1}{16\Delta t} \sum_{n=1}^{16} S_{gn} T_{\text{eq}}[\Delta t + \tau(e^{-[t_o + (n-1)(t_p + \Delta t) + \Delta t]/\tau} - e^{-[t_o + (n-1)(t_p + \Delta t)]/\tau})] \quad \text{(A12)}$$

where t_o is the time the first trial begins.

† Four thermistors were used, each connected into a bridge circuit in this same fashion. Bridge circuits are used to bias out the constant background voltage drop across the thermistor to increase sensitivity. Only one thermistor served as target at any time. In some sessions, the target thermistor was outside the thermos.

With $\tau = 2400$ s, $t = 45$ s, $t_p = 45$ s, $t_o = 300$ s, and $T_{eq} = 0.024°$ K

$$T_{bias} = -0.001° \text{ K} \qquad \text{(A13)}$$

The range of temperature deviations in Schmeidler's experiment was about ± 0.1° K. Thus, the probability p that the temperature bias contributed to *changing* the result of a particular trial would be about .001/(2 × .1) or

$$p \approx .005 \qquad \text{(A14)}$$

Since there were 16 trials in each run (half session), there exists a probability $p' = .080$ that any biasing effect occurred in scoring the run. Of the seven runs using target thermistors in thermos bottles, there exists a 50% chance that one of these runs contained a single trial bias in the statistics. However, the experimental results show that four of these seven gave statistical significance below the .001 level. Even assuming a single trial bias occurred in one of these seven runs, the effect on the overall statistic is minimal. Millar's criticism of the Schmeidler-Swann thermistor experiment, although certainly a valid issue to consider, nevertheless proves on close examination to be irrelevant. Millar's conclusion in regard to Schmeidler's experiment is invalid. Schmeidler's experimental results must be considered to remain valid, although the interpretation that the effect was of a thermal nature rather than PK on a very labile Beckman dynograph remains an open issue.

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A WALK AMONG THE TREES IN SEARCH OF THE FOREST

COMMENTS ON JEFFREY MISHLOVE'S *PSI DEVELOPMENT SYSTEMS*

BY REX G. STANFORD

In his preface to *Psi Development Systems*,[1] Jeffrey Mishlove states that his goal as a student of parapsychology has been to foster through his work a new synthesis of subjective and objective approaches to psi phenomena. He readily admits that the present work cannot do more than lay some groundwork toward that goal. Such a synthesis, he feels, is still premature. He holds as a "major premise" that a holistic consideration of the components of prescientific systems of psi development "can lead to entirely new, and potentially valuable, academic perspectives on an approach to psi training" (p. 70). He holds similar views on the potential usefulness of studying the contemporary popular systems that make claims in the area of psi development. Though the present volume falls short of demonstrating the truth of that premise and though its author does not claim that it does, he clearly hopes to encourage his readers in the quest of discovering where the premise can lead. The content of this very compact little volume should encourage the reader to do just that.

The major topics he discusses are (a) prescientific traditions, mainly religious ones, that have something to say about the emergence, development, or appearance in their adherents of what would today be termed ostensible psi phenomena; (b) contemporary popular systems alleged by their promulgators to train psi abilities or, at least, to foster the internal states in which psi events are thought to occur; and (c) experimental parapsychological studies that Mishlove regards as having relevance to the training, development, or emergence of psi performance in individuals. These topics are considered in chapters II, III, and IV, respectively. Chapter I is a long, rather rambling introduction that discusses a multitude of topics. In line with Mishlove's systems approach, chapter V delineates a series of systems characteristics and briefly discusses how each applies to certain psi

[1] Jefferson, NC, and London: McFarland, 1983. Pp. 299. $24.95, cloth.