# PSI RESEARCH METHODOLOGY: A RE-EXAMINATION

PROCEEDINGS OF AN INTERNATIONAL CONFERENCE

HELD IN CHAPEL HILL, NORTH CAROLINA

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Edited By

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## INTRODUCTION

LISETTE COLY: How do you do Ladies and Gentlemen. My name is Lisette Coly and as the Parapsychology Foundation's Vice President I call to order this our 37th Annual International Conference. This conference is now in session. On behalf of the Board of Trustees of the Parapsychology Foundation I welcome you all to this year's conference entitled "Psi Research Methodology: A Re-examination."

Our title reflects the fact that the Foundation sponsored twenty years ago—in 1968 to be exact—a conference devoted to "Methodology in Psi Research" at what was then the Foundation's European headquarters located in St. Paul de Vence, France. As Dr. A.R.G. Owen stated so well at the 1968 conference, "Methodology can be thought of in a restricted sense as a theory of experimental design or more broadly in terms of imaginative choice of avenues of approach." It shall be good for us to learn over the next two conference days if the intervening 20 years have indeed brought about a change—or perhaps not in some cases—in our experimental design and certainly interesting to hear if we parapsychologists have continued to choose imaginative avenues of approach in our methodologies.

We are very pleased to hold a Parapsychology Foundation conference in the Durham area and certainly one devoted to methodology is appropriate. Our founder, Eileen Garrett, was glad to participate way back in 1934 in some of Dr. J.B. Rhine's early experimental work at Duke University. The time spent in Durham was the start of a fruitful association of the Rhines and Garrett as well as the two organizations they would later come to found, Foundation for Research on the Nature of Man and Parapsychology Foundation. J.B. Rhine's obituary in the *Journal of the Society for Psychical Research* (1971) described Eileen Garrett and the Parapsychology Foundation most eloquently:

> The experiments with Mrs. Garrett at Duke were a turning point in parapsychology as a science . . . Mrs. Garrett blazed her own trail and made the Parapsychology Foundation by all odds the greatest achievement of her life. Its record speaks for itself. One of its unique services was the annual international conferences . . . These gatherings filled an educational need the more scientific meetings naturally could not provide for. One of Mrs. Garrett's most laudable aims

> was to establish a first-rate library for parapsychology and this became another of the Foundation's accomplishments of lasting value. Her publication program too went far to fill the gap between the scientifically edited publications and the frankly popular range . . . But to many people the Parapsychology Foundation's greatest gift to parapsychology has been in dollars. There has been nothing else to compare with the generosity of this Lady Bountiful of Parapsychology as she willingly poured out the financial aid needed by isolated workers in many countries struggling to do something in or near the field of psi research. She helped new centers to get started, stimulated publication of reports, and opened up neglected branches of inquiry. Who will ever do the like again? . . . Eileen Garrett has had a hand in the development of parapsychology in our culture that in its way will not likely be equalled. Because of her originality, her initiative and her independent spirit much of what she did was unconventional as the academic and scientific institutions regard matters. But in the way she could best make her contribution she did it inimitably and with a sparkle of enjoyment that is good to remember. (pp. 60–61)

Well, Garrett was indeed a hard act to follow, but I am sure most of you here will agree that our present President, Eileen Coly, has met the challenge well. She has continued to pledge the Foundation's resources to the support of the field albeit not having quite the financial wherewithal available to be referred to as Lady Bountiful! Mrs. Coly has nevertheless paid her dues and come a long way from 1934 when, while awaiting the finish of one of Garrett's experimental sessions with Rhine, she asked Karl Zener if he was familiar with those novel little five cards with their various symbols . . .

Ladies and Gentlemen, it gives me great pleasure to introduce to you the President of Parapsychology Foundation, Eileen Coly.

# GREETINGS

EILEEN COLY: I am very pleased, both professionally and personally, to welcome you all here today. Thank you for all your efforts to join us at what we feel sure will be a very informative and valuable two conference days.

I find it hard to grasp the fact that my first visit to Durham, as Lisette has already told you, was over 50 years ago, while accompanying my mother on her initial series of experiments with Dr. J.B. Rhine. I am sure you will agree that Garrett and Rhine or as she affectionately called him "The Boss" represented quite an "Odd Couple." However, despite their disparate methods they shared a common goal. I am sure if, along with Dr. Louisa Rhine, they were present here today, they would be gratified to acknowledge the growth and continuance of the subject each did so much to shed light upon.

The list of participants at our 1968 conference on methodology in psi research is no less impressive than today's roster. Naming just a few of those who presented papers—John Beloff, Jan Ehrenwald, Stanley Krippner, Karl Pribram, Charles Tart, Montague Ullman, Robert Van de Castle—it is worthwhile to note that they have all continued to contribute to the field. I am especially pleased that Ramakrishna Rao—a long time friend of the Parapsychology Foundation who actively participated in the 1968 conference—has consented to present a paper at this 1988 conference.

Before I turn you over to our very able conference moderator, Hoyt Edge of Rollins College, Florida, well known and respected by all of us, I would like to share with you a very apt statement made by Charley Tart at the 1968 conference:

"Our effort must be directed towards making psi phenomena function at a much higher level of significance than they currently do. The phenomena are either too weak or too sporadic and they remain trivial anomalies which do not seriously challenge the current paradigm . . . I agree that this is a pessimistic view of the current situation in psi research, but I am afraid that it is realistic. We cannot make psi work because we do not know enough about it. We cannot learn enough about it because we cannot make it work." Tart continued: "Let me close with a perverse sort of optimism, however. A few years ago, Burke Smith and I carried out a survey of research effort in parapsychology.

We found that the amount of time and money expended on psi research each year is negligible, compared to most other fields of science. Yet consider the smoke it raises. It must be a very powerful phenomenon, indeed."

Ladies and Gentlemen . . . do we smell smoke? All right then let's get down to work. I turn the proceedings over to Dr. Hoyt Edge.

# OPENING REMARKS

HOYT EDGE: Thank you, Lisette; thank you, Eileen. It is fitting that the Parapsychology Foundation return again after 20 years to the topic of methodology in psi research and particularly fitting for us to have these participants so many of whom began their research in the middle and late 60s. Progress that we have made in parapsychology, and particularly in methodology—and we have made progress—is to a great degree attributable to them.

Roberto Cavanna laid out the platform of the conference 20 years ago. At that conference on psi research methodology, he pointed out that parapsychology had gone through two stages—the era of mediumistic investigations and the era of Rhine's card-guessing experiments. New approaches were needed, he said. A new set of methodologies was required for parapsychology to change so that it would enter into a third stage.

And change there has been. Beginning in the late 60s a plethora of approaches blossomed and we are today the heirs of these changes. But as I look over these 20 years I see change within continuity. For instance, we are still interested in the relationship between psi and altered states of consciousness, but rather than focusing on the dream state we engage in ganzfeld research. Further, parapsychologists have always been interested in the appropriate statistical approaches, but we are necessarily becoming more sophisticated in how we use statistics. For instance, we will hear papers in our program on the use of meta-analysis and on our progress in analyzing free-response data. The final example is the more sophisticated ways in which living target systems are being used (as target) in PK research in a paper by William Braud.

And yet there is a darker side to the last 20 years. The optimism of the late 60s and early 70s that a significant breakthrough was just around the corner in how to explain psi has not materialized. Many of the same questions from 20 years ago still exist today. What do we do with the ubiquitous experimenter effect? It is possible for us to obtain real psi in the laboratory? Have we conceptualized psi in such a way that our methodology is not appropriate to finding it? Is psi not the sort of phenomenon after all that can be understood scientifically? And thus will any methodology be adequate? These are serious questions. They call for serious analysis and we will get some of that in the papers in this conference.

Twenty years ago Roberto Cavanna in his introduction to the program remarked that, "Basic methodological assumptions, explicit and implicit in the experimental design, are rarely, if ever, questioned. A stagnant and sterile situation is therefore perpetuated, and the scientific status of parapsychology cannot improve." I believe that this is no longer the case for parapsychology, if it ever was the case. We are a field perpetually asking questions about our methodological assumptions and their implications for experimental designs. We are a self-reflective community, more so perhaps than any scientific community, and it is in that spirit that we turn once again to examine our field, its assumptions and the appropriateness of its methodologies. I am asked to be a hard taskmaster in my role as moderator. There are some rules . . . Having summarized our conference procedures we now begin the session officially with Ramakrishna Rao, whom we invite not only as the first speaker, but back to the United States to work full time.

# ARE WE THROWING THE BABY OUT WITH THE BATH WATER? A PLEA FOR A NEW LOOK AT OUR RESEARCH STRATEGIES

K. Ramakrishna Rao

In his Presidential Address to the Society for Psychical Research in 1942 Dr. R. H. Thouless suggested a shift in the focus of parapsychological research from one of proof to one of understanding. As he put it, "Let us now give up the task of trying to prove again to the satisfaction of the sceptical that the psi effect really exists, and try instead to devote ourselves to the task of finding out all we can about it. With fuller knowledge of its nature, the difficulties of believing in its existence may appear less formidable than they do now" (1943, p. 171).

As Thouless saw it, the changed objective would give new perspective to research. Since the objective would be to elucidate the character and the conditions of the phenomena, research should aim at answering specific questions. Bold speculations are in order if we are properly cautious about drawing conclusions. Negative results are meaningful to the extent that they define the boundary conditions. "It does not matter," Thouless observed, "if a speculation is wrong; if so it will be proved wrong by experiment and that will be a step forward" (p. 168). It follows, then, that there is neither the imperative for implementing extraordinary security measures nor the necessity to accumulate massive odds against the chance explanation. What is needed are understanding of and insight into the psi process itself. In other words, instead of being obsessed with the fear of possible error somewhere, psi research, like all scientific research, should pursue progressive research programs, looking forward instead of backward.

Nearly 20 years later Thouless (1960), without changing his conviction that the reality of psi is evidenced by psi research, confessed that he was "over-optimistic about the extent to which the evidence for psi was enough to convince everybody." "I think now," he said, "that there is an irreducible scepticism; that is, irreducible in the present state of evidence" (p. 217). Thouless went on to suggest a threefold strategy to overcome the skepticism: (a) that we create a pool of successful subjects who would be available to skeptical scientists to work with, (b) that we encourage replication of successful results, and (c) that we get psi to work by employing such techniques as repeated guessing.

During the past 25 years, efforts along the above lines were made in various measure. The strategy of pooling outstanding subjects for testing by skeptical experimenters is the one least practiced, for obvious reasons. Such subjects are hard to come by and, when they do appear, understandably the priorities shift in favor of those experimenters already in the field and fortunate enough to have discovered them. Also, the interests of the subjects themselves cannot be overlooked. All these factors point to the intrinsic difficulties of providing star subjects to skeptical experimenters. A notable exception in this connection, however, is the work with Pavel Stepanek (PS). Milan Ryzl, who discovered and trained PS, took the extraordinary step of inviting interested researchers from various parts of the world to work with him. Many of them, were able to observe firsthand the successful performances of PS with binary psi targets (Pratt, 1973). But then the results of work with PS carried no more credibility with the skeptics than other significant studies in the field (Hansel, 1980; Gardner, 1989).

The importance of replication is fully recognized by parapsychologists, as may be seen not only from the numerous attempts to replicate each other's work, but also from the recent upsurge in the meta-analytic reviews of literature. Indeed, a serious and sustainable claim can be made that certain parapsychological efforts are replicable in a statistical sense (Rao & Palmer, 1987). It can be argued also that when an effect is measured and identified with the aid of statistical analysis, the only replication we could have of such an effect is statistical replication. This fact is not sufficiently appreciated by those who demand absolute replication. Even parapsychologists themselves do not appear to be always clear on this issue.

I do not believe that replication of this sort would be any more convincing to the skeptics than the individual experiments. Charles Akers (1985), for example, had already criticized the application of meta-analytic techniques to parapsychology as premature. If precognition for a meta-analysis is data agreed-on by parapsychologists and their critics, then we may be left with no data. If there could be a perfect experiment whose results were acceptable in principle to all, then those results would be sufficient to justify the acceptance of the phenomena. Replication in parapsychology, then, takes a back seat, as in other sciences. It is precisely the lack of agreed-on data that renders replication so important in controversial areas.

It is not difficult to see why such crucial data are hard to come by in practice. Inasmuch as one can always find in retrospect something that could have been done differently to control for imaginary artifacts, it is in principle impossible to specify in advance totally foolproof conditions. That the present critics of psi research consider certain conditions sufficient as adequate tests of the psi hypothesis is no assurance that a future critic may not demand further improvements and set totally different experimental standards. Mere replication of results is, therefore, unlikely to satisfy a critic who can always think of an alternative explanation, however improbable that may be, as long as he can afford to ignore the phenomena.

One would hardly be expected to ignore a phenomenon if it is seen to be working in life. Application of psi, to get it to work, may have a more compelling influence on one's perceptions of its reality than low-level replications. J. B. Rhine (1965), while recognizing that the practical application of psi ability would do more than anything else to break down resistance to its acceptance, cautioned however that it "is not reliable enough in its present state for dependable, practical use" (p. 48). One might with some justification argue that Rhine was overcautious. Ryzl (1962) claimed that his subject, JK, was able to identify a winning lottery number. James C. Carpenter (1975), using ordinary college students as subjects, employed a highly complex and ingenious psi procedure to correctly identify a hidden word. More recently, Russell Targ and Blue Harary (1985) claimed to have applied psi in speculating profitably on silver futures. Stephan Schwartz (1983) used psychics to locate ancient archaeological sites for successful excavation. Similar claims were made earlier by Goodman (1974) and Emerson (1974). But the work of Schwartz or of Targ and Harary did not appear in a refereed journal. The full account of Carpenter's "peace" study is yet to be published. In short, the credibility of applied psi studies is low even in parapsychological circles.

Parapsychologists themselves are open to skepticism when highly significant claims of psi occurrences are made. There does appear to be that "irreducible skepticism" even among those who believe in the existence of psi phenomena, when it comes to believing in strong psi effects because we are used to a very low-level occurrence, conspicuous by its inconsistency and evasiveness rather than by its consistency and persistence; and we are constantly reminded of the possibility of fraud and occasional exposure. We would, therefore, be wrong to think that "irreducible skepticism" is limited only to those who are committed to a particular worldview which the paranormal challenges. Lingering doubts of possible error somewhere have haunted some of the most enthusiastic supporters of psi research. For example, Gardner Murphy (1961/1970), who had repeatedly expressed his belief in the existence of psi and the vitality of psi research, made it clear that he could not accept the simple statement that "men of integrity and good will do not deceive themselves, do not get caught in ethical traps, do not withhold data, do not give false impressions." "On the contrary," wrote Murphy, "my impression is that normal human beings get involved to some degree in just such complications" (p. 284).

During the past 40 years parapsychologists have developed a methodology and a set of standards that are based on certain assumptions, which, with few notable exceptions, most experimental parapsychologists seem to share. The assumptions are (a) that psi is an ability like perception, (b) that it functions independently of our sensory-motor systems, (c) that it manifests even when the subject is shielded from all other modalities of subject-target interaction, and (d) that it can be detected and measured as distinct from and independent of other modalities. To the extent that we succeeded in obtaining laboratory evidence for the existence of psi by pursuing methods presumed to exclude all other modalities of subject-target interaction, the above assumptions are indeed supported. But there are other factors which make us wonder whether a re-examination of these assumptions and the experimental methods based on them may not be in order now. First, there is the continuing controversy over whether all the alternate modalities of subject-target interaction are indeed excluded as claimed. Second, the low level of psi in terms of its effect size in laboratory tests and the notorious unreliability of results have been constant impediments to a proper understanding of psi and its place in the order of things. Third, psi, as it manifests in real-life situations, does not seem to be congruent with the assumptions mentioned above. All these observations point to the possibility that our testing procedures themselves may be psi inhibitory and that they may mask or filter out psi to the point of effectively reducing it to a trickle that we can afford to ignore. They may also indicate the need for looking at alternate models that promise stronger effects.

Rex Stanford (1977) called our attention to the fact that parapsychologists by and large have implicitly or explicitly subscribed to what he described as a psychobiological model which assumes, for example, that ESP is basically a form of cognitive-perceptual experience. The assumption, as he pointed out, would seem to be that ESP is perceptual in its basic character and that it somehow struggles for expression in just that form, often, however, finding obstacles to expression in its 'true' form" (p. 5). Stanford argued that there are various examples of ESP from life experiences as well as laboratory results "which do not share this cognitive characteristic." He therefore proposed instead a conformance behavior model. Whether or not one agrees with Stanford that his conformance behavior model is a true alternative to the psychobiological model, he is perfectly convincing in his arguments for re-examining the implicit traditional assumptions that have for so long prompted our research questions.

William Braud wondered whether psi may best only provide information that our regular senses do not. "One reason for our failure to replicate," he said, "may be that we are attempting to replicate the wrong thing. However, if we consider larger *relationships* into which events may enter, information about histories or about futures, perhaps dimensions other than those revealed by formal physical properties, perhaps those are the places where we can find some unique contributions of psi and maybe then our replication rate will increase" (Shapin & Coly, 1985, p. 42).

A somewhat different view was expressed by Murphy (1964), who argued that "there is *no new information* ever conveyed by the paranormal process; there is only a transposition. It merely makes information accessible to us under conditions when it would not be ordinarily accessible, according to our present knowledge." "There is a kind of reality underlying psychic phenomena," according to Murphy, "which is a substantive reality, a reality regarding a medium of communication. But it is not a content reality; that is, *it does not give us any specific information other than that which we could ordinarily achieve through the usual sensory means*" (p. 244).

Our research methods have always attempted to exclude the operation of our regular senses and other information retrieval possibilities in any psi testing situation. All our experimental controls are intended precisely to show that the acquired information could not have been obtained by other nonparanormal means. For example, Gertrude Schmeidler (1977) writing about dowsing says that "two factors need to be controlled. One is familiarity with the topography and microclimate of a region. Someone who has lived for a long time in the hills of Vermont may, for example, be able to infer the location of underground water from the plant growth and the dips of the ground. . . . The second factor extends to the cutting edge of research on sensory sensitivity: it now seems possible that humans may share with other animals some faint sensitivity to electromagnetic changes of the kind that could be produced by running water" (p. 149). Therefore it is recommended that all testing of dowsing may be done without taking the dowser to the actual site and by merely presenting the subject with a map. Similar constraints are suggested for research on paranormal healing and other psychic practices.

All this is proper because we are concerned with the problem of demonstrating the reality of psi as distinct from other modalities. Being distinct, however, does not necessarily imply that psi is independent and that it can function without the aid of other modalities of our normal cognition and action. What if psi functions, as Gardner Murphy (1961/1970) suspected, in juxtaposition or in coalescence with other modalities including the sensory? What if the normal and the paranormal blend and function in fusion reinforcing each other rather than in isolation and independently of each other? What if the sensory and other normal processes are needed to sustain, guide, channel, trigger, or focus on the paranormal? And what if psi merely supplements rather than supplants the sensory and motor functions? If we give any credence to these possibilities, we would be in a totally new ball park, playing a different game with a completely new set of rules.

Gardner Murphy (1961/1970) referred to special cases in which the normal may call upon the paranormal for aid and the two kinds of functions may be blended. "The faint sights and sounds may offer a matrix upon which paranormal information may be grafted. . . . We may be able to see what will happen when normal and paranormal occur in juxtaposition or in *coalescence* or *reinforcement*, one of another" (p. 278). I would like to argue that it is precisely this way that psi functions generally and not merely in very special cases. Psi as it manifests in human experience may not occur in a vacuum. It occurs, to use Stanford's phrase, in disposed systems. Disposed systems, I submit, are not merely those "with a need, wish, or want of some kind." They are also systems that can creatively link the normal and the paranormal. The normal may be the fuse that ignites the paranormal or simply the base on which the paranormal is mounted. ESP may be more like creativity in problem solving than perception of hidden phenomena. It does not merely interpret what is given, it builds on it. If so, what could be more important in parapsychology than studying the conditions under which the normal and the paranormal interact? How can we study such interactions if we are bound by a methodology which basically attempts to exclude the normal so that the paranormal can be observed? I believe our obsessive concern to isolate psi from other human functions has provided minimal opportunity for psi to manifest. Either psi is inherently evasive and therefore unreplicable and uncontrollable or it is essentially masked and passes mostly unrecognized in our lives as well as in our laboratories. If the latter is the case, as I have begun to suspect, our current research strategies appear to be largely inadequate and probably irrelevant to the task of obtaining psi in a measure that is hard to ignore.

What I am pleading for is not just one more turn in the shifting scenes and changing fashions in psi testing that we have seen over the years as we moved, for example, from using restricted response materials to free response targets, from testing unselected subjects to preselected subjects, from group testing to individual testing, and so on. It is more radical than that. I am suggesting that we devise testing procedures in which the subject is provided with sensory as well as extrasensory information with the objective of discovering whether the sensory awareness somehow helps to expand the extrasensory intake, whether the normal tends to enhance the paranormal. I am urging a new strategy for studying normal-paranormal interaction which, if successful, would yield results that could not be ignored, because they would be too striking and significant in their import.

Anomalous results with low effect size and high rate of inconsistency can and will be ignored, however stringent the controls may be and whatever precautions one may take to avoid error and deception. Again, such results render process-oriented research very difficult indeed, as we have seen in the history of the field. But a tangible and consistent effect, even when obtained under conditions that may not have the best of controls, could be very valuable in understanding the phenomenon—an understanding that would lead to greater control and more progressive research programs. Controls become irrelevant when the demonstrated effects are of practical value. Suppose we are investigating the dowsing capabilities of a subject or his ability to forecast weather. If this subject is able to locate water, oil, or whatever he is divining more consistently than the geophysicists employing the state-of-the-art technology can do, or if he predicts weather better than professional meteorologists do, who cares whether or not he had available to him geological data about the terrain or the weather patterns of the region? With the low level information that is mediated through ESP is it reasonable at all to expect the dowser to perform better than the geologist without the geological information?

It is possible of course to test one's dowsing abilities by *excluding* all relevant information from the subject, as we have done in the past, so that when he does make a correct identification we may say that he was able to do so by ESP or some paranormal ability. We could also test his ability by *providing* all the available information and see whether he could do any better in identifying the correct location than others who have the same information and no less professional skill in making use of that information. We have been doing mostly the former with less success. I am pleading that we do the latter as well and see if we meet with more success. We have been testing in essence the *exclusion* hypothesis. We may now begin to test the *fusion* hypothesis, namely, that psi functions in unison with other abilities, building and adding on the information that normally becomes accessible.

The basis for my confidence in the fusion hypothesis is my conviction that psi plays a significant role in many of our successful activities. The business intuitions of successful corporate executives and the creative genius of outstanding scientists and inventors may involve a healthy mix of normal and paranormal inputs. Let us consider, for instance, the case of scientific discovery. There are usually two ways in which hypotheses occur to scientific thinkers. Sometimes these scientists are patiently and consciously led, step by step, by their own observations of phenomena and the results of other studies, to generalizations concerning them which are also predictive of phenomena to be ascertained in the future. But scientists also report that sometimes a sudden insight into the nature of certain phenomena occurs to them. Their further work then consists of systematically developing the idea and obtaining evidence for it. It cannot be reasonably maintained that the insight itself is caused by the awareness of the problem by the scientist, because he does not report any such awareness. To argue that he must have "noticed" the relationships at the level of the unconscious adds little to our understanding of his insight.

We know that the scientist requires something beyond mere intellect. Writing on the intellectual abilities of six great scientists, Crowther (1955) tells us that the factor common among the great scientists "was the imagination to conceive a great idea" (p. 9). Introducing the English edition of Poincare's book *Science and Hypothesis,* Professor Laumor says, "The aspect of the subject which has here been dwelt on is that scientific progress, considered historically, is not a strictly logical process, and does not proceed by syllogisms. New ideas emerge dimly into intuition, come into consciousness from nobody knows where, and become the material on which the mind operates, forging them gradually into consistent doctrine, which can be welded onto existing domains of knowledge" (Poincare, 1952, p. xviii). Does this not remind us of how psi manifests in spontaneous experiences?

An interesting case of a great scientist whose discoveries, emerging from little or no formal training, baffled the commonsense canons of scientific process is that of the Indian mathematician Ramanujan. Professor Hardy (1959) who took Ramanujan to England and worked with him for a number of years, characterized Ramanujan as:

> A man whose career seems full of paradoxes and contradictions, who defies almost all the canons by which we are accustomed to judge one another, and about whom all of us will probably agree on one judgment only, that he was in some sense a very great mathematician. . . . He was, at the best, a half-educated Indian; he never had the advantages, such as they are, of an orthodox Indian training; he never was able to pass the "First Arts Examination" of an Indian university, and never could rise even to be a "Failed B. A." He worked, for most of his life, in practically complete ignorance of Modern European mathematics, and died when he was a little over thirty and when his mathematical education had in some ways hardly begun. He published abundantly—his published papers make a volume of nearly 400 pages—but he also left a mass of unpublished work which had never been analysed properly until the last few years. This work includes a great deal that is new, but much more that is rediscovery and often imperfect rediscovery; and it is sometimes still impossible to distinguish between what he must have rediscovered and what he may somehow have learnt. I cannot imagine anybody saying with any confidence, even now, just how great a mathematician he was and still less how great a mathematician he might have been. (p. 1)

There are several instances in Ramanujan's life that suggest a psi source for his mathematical genius (Rao, 1972). In the case of many other scientific discoveries a case can also be made for the operation of psi, even though it is manifestly less spectacular, being shadowed in most cases by the scholarly and logical synthesis of purported discovery with empirically derived data and rational argument.

The model I have discussed makes a strong case for studying psi in life situations where the normal and the paranormal appear to operate co-existently. It is also readily amenable for conducting applied psi research. I am of the view that it is neither premature nor unethical to conduct applied psi research at the present juncture, as long as we are cautious in our conclusions and do not espouse more optimism than what is warranted by the data. In fact, applied research appears to be the need of the day.

The implications of this model for laboratory psi research may, however, appear to be more tenuous inasmuch as laboratory research is usually tied to controls, and controls in psi tests essentially involve excluding the normal. Again, my argument may be misinterpreted as justifying loose conditions and incompetence and even chicanery in psi research. Am I justifying the view that the mediums may be allowed to cheat so that they can produce some psi effects? Am I pleading for loose experimental conditions where subjects may successfully perform a psi task by using normal means? The answer is clearly "No." I am suggesting that we set up experimental conditions that permit the interaction of the normal and the paranormal with the expectation that a stronger effect may be registered. Obviously we need to bring creativity and freshness to bear on the development of new research strategies and evaluation procedures to test the model I am pleading for. But I believe we already have available to us test procedures that we can adapt with ease to test the fusion hypothesis at different levels of complexity. I shall briefly describe a couple of areas of research that have relevance to my current interests so that I can benefit by the discussion here today.

Kreitler and Kreitler (1972) carried out a series of important experiments to determine whether ESP could influence subliminal perception. In one series, for example, the subjects, who were completely unaware of the ESP component of the experiment, attempted to identify subliminally projected alphabets. Unknown to the subject, during half of the trials an agent in another room concentrated on the target alphabet and attempted to "transmit" it to the subject. The procedure of target presentation was such that each target was presented to the subject twice, once with the agent "transmitting" and another time without the agent. The Kreitlers reported that the subjects correctly identified significantly more letters when the agent was "transmitting" them than when he was not. In 102 cases, the letters which were incorrectly identified with no senders were correctly identified with the sender. In the opposite case, 76 letters which were incorrectly identified with the senders were correctly identified with no senders. The difference is statistically significant.

The above result is of some interest because it does support the authors' hypothesis of telepathic influence on subliminal perception. But the total number of correct identifications in the two conditions do not differ significantly from each other. The total number of hits with an agent transmitting is 286 as against 260 hits obtained without the agent.

Such a comparison, as the Kreitlers recognized, would not be very appropriate in the present case. Their reasoning was that "this method is based on raw numbers inflated through the inclusion of 184 correct identifications common in the two conditions" (p. 12). But there is a more compelling reason against such a comparison. At best, a significant difference between the two is indicative more of the relative success of SP (subliminal perception) target identification under GESP and clairvoyance conditions rather than a true comparison of SP and SP plus ESP conditions. We may recall how Coover (1917) mistook GESP and clairvoyance conditions as telepathy and control conditions, respectively, and erroneously concluded that there was no ESP in the data.

Interestingly, the Kreitlers tested their subjects in the same session to determine their subliminal thresholds and found that they averaged 3.34 hits per 12 trials, which gives a success rate of 27.83%. In comparison, the percentage of correct identification in the experimental trials under both the conditions combined is 39%. The difference between the baseline hit rate and the hit rate in the experimental trails is thus highly significant. The authors very casually dismiss such a comparison as "based on the false assumption of comparing observations with fictional rather that empirical values" (p. 12). Such a dismissal seems to me unwarranted unless the experimenter was not careful enough while testing subjects in that condition, or the slides used in experimental trials were different in crucial respects from those used in the preliminary trials. There is nothing in the report to suggest either of these was in fact the case. Therefore, greater attention should be paid to the highly significant difference between the success rate in baseline and experimental trials than the Kreitlers did.

While I can conjecture a number of possible artifacts for increased correct identifications in the experimental trials, I cannot share the view of the Kreitlers that we are comparing any fictional values here. Surely neither of the values is fictional. Both are empirical values and if there are any other variables such as learning or adaptation to the experimental set-up that are conceivably relevant to enhanced scoring in the experimental trials, they could be identified and controlled. But the true comparison would be the one in which the baseline scoring rate is compared to scoring in the ESP plus subliminal condition and not between the telepathy and clairvoyance conditions.

The above line of research, in my view, is important and deserves to be explored further. If these results are any indication, we can expect a stronger effect when an opportunity exists for psi to enhance or build on sensory information. The above experimental paradigm can be adapted to memory-ESP studies, ESP-examination studies, and numerous others that link normal psychological abilities with ESP. For example, in a memory-ESP study, the subject may be asked to recall paired associates which he has learned under conditions when his learning is reinforced with ESP. These scores may be compared to baseline scores obtained without ESP reinforcement. Reinforcing may be effected by presenting the correct response words as ESP targets concealed in sealed envelopes or by other procedures such as the use of agents. We may compare the memory scores in both the conditions with the expectation that the scores obtained in the ESP-reinforced condition would be higher than the baseline memory scores obtained without ESP reinforcement.

I anticipate one line of criticism for the above design. How can we really control ESP in the baseline condition? Even in simple recall tasks when the learned information is not available, the subject may obtain the information through ESP. Therefore, there can be no true baseline score. Such an argument has some merit and it is logically irrefutable. But in practice it can be ignored, I believe, for good reasons. First, psychological tests including tests for recall seem to work pretty well in practice. This is either because subjects in such situations do not use their ESP or because the ESP use is so randomly distributed in the population that it makes little difference except as random noise which can be ignored. Second, while testing for ESP, we make some basic assumptions, the most important one being that the subject's volition is somehow relevant and that the act of participating in a psi test triggers psi. This does not necessarily rule out the possibility of psi manifesting in a nonintentional way, but in laboratory tests the subject's performance is, by assumption, linked to intentions, his or those of someone else who is connected with the experiment.

This brings us to the apparent differences between the assumptions we make about spontaneous psi and laboratory psi. In spontaneous cases, it just happens that someone has an experience that warrants paranormal explanation. The person as far as we can tell is not seeking the experience and in no sense has he any control over it. In laboratory psi, the situation is somewhat different. Where we are testing for intentional psi, the subject is presumed to exercise his psi. Even in experimental studies of so-called nonintentional psi, there is someone in the experimental situation, the experimenter, the agent, or other persons associated with the experiment, whose intentions are presumed to relate to the experimental outcome.

Are these differences so crucial that we need to postulate two different kinds of psi? Or is it possible that at a higher level of organization, these are integrated and that we can speak of an essential unity between them? I believe the analogy of dreaming may be relevant to a discussion of these issues. Dreaming, like psi, is an experience that spontaneously occurs to people. No doubt it is nearly universal and more pervasive, regular, and predictable than psi. But it is all the same an experience over which we ordinarily make no claims of control. Yet we know that the contents of dreams can be manipulated by a variety of means, which suggests that we have a measure of control over what we dream about. Similarly, what is happening in the laboratory tests of psi is that we are attempting, with varying degrees of success, volitional control of psi. Therefore, instructing the subject to use his ESP, placing him in or creating for him a psi-conducive situation, or attempting to influence the outcome psychically are legitimate and meaningful manipulations by the experimenter. The resultant scores in comparison to the baseline (control) scores may be regarded as a function of the strength of the manipulation. Thus I find the transition from ESP in life to ESP in lab is no more different in crucial respects than the one from "home" dreams to dreams induced in a laboratory.

My second proposal relates to free-response material and is probably a more direct test of the assumptions I am making about psi manifestation. I recommend that we consider presenting to the subject some aspects or parts of the targets subliminally or supraliminally during ESP orientation with the expectation that those aspects or parts of the targets that were not presented to the subject at all will also find a place in the subject's mentation. For example, after the necessary ganzfeld preparation, we could provide carefully selected auditory subliminal cues representing certain aspects of the target while the subject reports what is going on in his mind. Or alternatively we could mix these subliminal cues with the "white noise" during the ganzfeld preparation. If my hypothesis has any validity, I would expect that the mentation of the subject would be a lot richer and that ESP would be seen in the manifestation of the other aspects of the targets that were sensorially unavailable to him and were not logically inferable from them. The subliminal cues provide the matrix on which information mediated by psi may be grafted, resulting in a sufficiently strong and replicable effect.

Of course, there will be new problems in judging and quantifying the data of this sort, but I do not believe they would be insurmountable. Unless I am missing something, I do not see any serious methodological pitfalls that would give us spurious data which we might mistake for psi. In any case, the principal reason for presenting this paper here is that I would like to have your considered comments and creative suggestions for translating these ideas into viable and progressive research programs.

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## *DISCUSSION*

MORRIS: First of all, thank you very much Ram, for a very interesting paper raising several different issues. One of the most interesting aspects is whether it is better to have a situation in which the participants are not easily able to identify the extent to which there is actual psychic functioning, along Batcheldorian lines. Some designs might encourage that ambiguity and others not. I think it is important to fit this into programmatic research, for instance, in dowsing. Suppose a dowser does better than a geophysical survey team. What happens next? At what stage do you have the attention of a broader pragmatically-oriented audience? Can we then go further in ways that build toward a real understanding of whatever is actually responsible for the dowsing success? The earliest part of your paper, however, was setting things within a context of interaction with a critical community having a fair amount of power. I think this raises questions about the extent to which some of what we do places us in a position of being advocates playing off against a set of counter advocates, not a set of people engaging in an act of communication with a neutral, but intelligently critical group. I wonder if we are in fact setting ourselves up as people engaged in an act of rhetoric whose goal is to persuade. It may seem as though we have a belief system that we are trying to recruit people. Yet hopefully, we really are trying to communicate with neutral individuals who have access to resources and who right now are listening to other people who engage in rhetoric a bit more.

RAO: Thank you, Bob. I think there are several issues that you touched on which are important. First with regard to the separation of field research and laboratory research, where do we go once we succeed at the field level? It seems to me that if we can convincingly demonstrate that a dowser is better than a professional scientist in locating water, minerals or whatever you are dowsing for, we have won, I think, a very important first round in the battle for recognition and support. If we do that we would have an enormous opening of resources that would make it possible to expand our research efforts. That is to me very important at this stage where we are really struggling to have any research effort going, for the simple reason that we are not attracting the necessary funding that we need even if our people are competent and we have viable research projects. Secondly, once you begin to find these successful dowsers here you have a group of people with certain characteristics, certain bases of functioning which may themselves give us insights into the laboratory type of research. We may not have such insights at this point. Therefore, in addition to focusing on stronger, noticeable, non-ignorable effects this very study will also give us information about the people who are succeeding. I do not think every dowser is going to succeed, as obviously they have not. The truly successful ones, as well as those who are not successful, would give us enough information about the circumstances surrounding field success which might lead us into laboratory techniques. At the same time I have also been advocating other areas of laboratory study, such as the ganzfeld and you can think of many others, such as ESP testing in life situation. There is more room for a programmatic research effort to go step-by-step to a stage where we will, I think, be able to produce the phenomena for someone who is critically looking at exclusion of those that may have contaminated our results.

With regard to your second point, Bob, if I understand you correctly it is a question of strategy. I have always thought that we can do research dispassionately, we can communicate dispassionately, foundations will support you dispassionately, work will go on objectively and the truth will ultimately triumph. Probably it does, but I think it is going to take a very long time. What I am saying here is that the small effect size and the legendary elusiveness of the phenomena we are dealing with call for a strategy that will enable us to effectively communicate with those outside of our field. Our ability to communicate is made many fold difficult when our results lack consistency and the effect size is too small and can be ignored without feeling uncomfortable. Now, this concern for having just one clean experiment from which you have excluded all possible alternate explanations may be very good in a field where the phenomena manifest consistently. In a field like ours with a small effect size, however, the scientific community in general can and will ignore our results, especially where they are perceived to be inconsistent with some of its basic assumptions. So what I am pleading for is to give some thought to a possible change in our methodology which hopefully would yield stronger results which are hard to ignore without feeling uncomfortable.

SCHOUTEN: For various reasons I find your contribution very attractive. One is that I am interested in spontaneous cases. Your suggestion that psi might function in conjunction with other modalities and not in an isolated way appeals to me. That is exactly, what happens in real life. Another reason is that you say implicitly that if one isolates psi in a laboratory situation that might indeed be quite a different matter from psi which you see in real life. In your paper you gave the example that in dowsing to make a clean experiment people ask dowsers to locate sites on a map instead of what they normally do in real life. I must say that that is one of the things that often bothers me about parapsychology. Clearly if a dowser is successful in locating water in the field, for instance, I never understood why people assume that then this dowser can also be successful in locating water on a map. That seems to me such an enormous jump. Perhaps it is of interest to mention the work on dowsing by Professor Betz in Germany. He obtained a substantial grant from the government over there and carries out exactly this type of research. He runs his experiments not in the laboratory but in the field. It is very cleverly done, I think. Although I feel attracted to your ideas, I still have one problem. I think that your idea would be that psi functions together with other modalities, but, clearly to me at least, that only applies to those situations where those other modalities are relevant to the situation. For instance, you mentioned Honorton's types of targets. You can do an experiment like that, but the problem is then that the information you give to the subject about the target is not relevant to the other aspects. I think that is the real problem here. It only works when the other information is relevant to the issue. I think you really have a problem in establishing a baseline, because you can expect then that information from the target will create associations. I think it is very difficult to sort out what is the psi component and what is not. I wonder if you have any ideas about that?

RAO: Well, I think you have a very valid point. The example I gave with regard to the slides and the ganzfeld would make a very clean experiment. Again we have grown up in this atmosphere of having a methodologically clean experiment, so that given the result you say that this is psi and it is not confounded by the sensory information. But I do recognize the difficulties involved in such an experiment. The information you give must somehow be related and in this light it may not be so organically related. There may not be that kind of a gestalt that one could perceive. But I do think we cannot just throw it away without first testing it. It is not an ideal experiment to test my hypothesis; it is just a beginning point. I would indeed like to have suggestions for improving on it.

STANFORD: I quite agree with what Dr. Schouten says, that it might be especially interesting when this additional material in the slide is presented tachystoscopically or subliminally, that is in some way related to the rest of the material. Now how do you cope with that? Actually this is not an insurmountable problem; in fact this problem has a principle that is already addressed in the parapsychological literature. In some of my early work with nonintentional psi, we were looking at the interaction of psi effect with ordinary memory effects, whether they contradict the ordinary information or whether they go along with it. The way to do that is not by relying on theoretical probabilities, but by getting your baseline from the empirical data in your own study. If the material is in some way or another logically or inferentially related to the rest of the material, if the target material is related to the rest of the picture in some way or another thematically, you can find out very easily whether there is any side effect by some trials with some subjects randomly having an agent attempting to influence that information while others do not. That is where you get your basis of statistical inference but you can compare the rate of incorporation when such information is there, with when there is no such information supplied. So I do not think there is a problem here. We already have evidence of psi operating in this situation. While I am fully sympathetic to them, I think your remarks may underestimate the degree to which we already have evidence about the interaction of psi with normal processes in the literature—perceptions, cognitions, memory material and so forth. Your own work is one example, I have done some work along the same lines, but I think if we review that very carefully we may have some very useful leads to follow on this important suggestion that you are making here.

RAO: I agree with you, Rex. I think we may already have some leads in the literature which we have to look at. But I do see some methodological problems, which may not be totally insurmountable, in getting the kind of effect we are looking at. I have not been able to cope with it. In my paper I refer to the Kreitlers' experiment, where their baseline scores (they seem to be baseline scores, but they are not very clear about that) are much lower than their ESP trials with the subliminal perception rate, when someone else is looking at the target compared to what they were able to successfully identify under a purely subliminal situation. The difference is highly significant compared to the very marginal significance they found in the kind of an assessment they have made. There probably are some artifacts there (I can think of a number), but if there are no artifacts that could be the kind of thing that we must be looking for. My main problem in those studies is when you have a situation where you have only subliminal perception without ESP, how do you control for ESP? This is my main methodological worry in any kind of test that I am going to do now. Even with a pure subliminal perception experiment, since we do not know any constraints for ESP, it is always possible to say that maybe precognitively or in some other way, the subject got accessed through ESP. Somehow we must operationally impose certain constraints on ESP and say that in this kind of situation ESP does not occur or, if it occurs, it does so at a low level which we can afford to ignore. That is my main problem for which I would like to have some feedback—how to control for ESP.

BRAUD: I very much appreciate your view that we are not being quite fair to psi in attempting to test it in isolation. We are removing the usual tools that it uses. I agree that it is embedded in a very intricate way in our everyday conventional activities. I would like to suggest an alternative strategy, which is to attempt to remove psi from its normal role in conventional activities to see whether those activities decline. This would tell us whether psi may have been present all along. That is an extremely difficult strategy. I have thought about it a little bit. We are aware of some psi antagonistic conditions and if we could employ these psi blocking or antagonistic conditions to conventional sensory skills or motor actions, then observe some deficits, perhaps we could attribute that difference to the psi component. The trick will be to pick some situations in which the psi antagonistic factors do not affect the conventional activities as well through some very direct action. But even that would be very useful information in that it will tell us something about the tools that psi uses.

RAO: I think that is a very interesting idea. The only problem I see is that many of the psi antagonistic factors seem also to be antagonistic to normal abilities. How to separate these two might be a problem, but if we could think of something I think it is a very interesting suggestion.

HONORTON: I have a couple of comments. The first is a complaint that I will probably repeat several times throughout these sessions. It has to do with what I think is a failure to look at the magnitude of effect size in parapsychology in relation to adjacent areas. I think this is a topic that Sybo is going to talk about to some degree tomorrow. We have a tendency to look at our own success rates in isolation, rather than looking at them in relation to what goes on in other related problems areas. We have to be very careful not to confuse effect size and inference level. It is well to remember that this spring the public health service prematurely terminated a major study of the effects of aspirin on heart attack rates in healthy, male physicians. The study involved some 22,000 physicians. It was terminated prematurely because the investigators believed that it would be unethical not to stop at the point where it was quite implicit that aspirin had a highly significant effect on the prevention of heart attacks. Now this was widely publicized in both the popular and the scientific media as representing virtually an absolute proof of the efficacy of aspirin in the prevention of heart attacks. When you calculate the effect size associated with that finding in the way that we calculate effect size in parapsychology, you find that the effect size is about .28, which is exactly the effect size in the meta-analysis of the ganzfeld work. Indeed it is still a small effect, but that calculates to roughly about a quarter of a standard deviation on the average. While we need to do everything we possibly can to increase the magnitude of our effect sizes we also need to be aware of the fact that these are competitive with what is being produced in many other areas of the social sciences that have had much longer periods and more resources to deal with their problems than we have had. I would suggest that the problem is not really one of getting larger effect sizes as it is getting more consistent effect sizes of the magnitude that we are currently getting. The other point I want to make is also another suggestion for you in terms of your experiment. I would love to see some more work done with the 1,024 targets that we spent a full year putting together some 15 years ago. But I do not think that you would be testing the hypothesis that you want to test for reasons that have already been mentioned. It would be equivalent to doing a forced-choice experiment with dual aspects—ESP cards where you have colors and symbols in no particular relationship between the two. There is another way of doing this that has occurred to me for some time that presumably gets into the general area that you are interested in and also touches on some other aspects. I think one of the central points that you are making that I very much endorse is that we need to increase the ecological validity of our experiments to bring them more in contact with what happens in real life situations. All the free response work has moved in that direction anyway. What I would suggest is that you do an experiment where the target has superimposed over it at various times either the subject's name or a photograph of the subject and agent together, so that the target is related in a meaningful, individual way to the subject in the way that is true in spontaneous ESP experiences. I think that might be a very productive approach.

# TAKING PSI ABILITY SERIOUSLY

Richard S. Broughton

Traditionally, there have been two principal philosophies that have guided parapsychological research. The original underpinning of our research was largely derived from religious beliefs and dualistic philosophies. This view held that psychic phenomena were brought about by another order of consciousness—the mind, spirit, soul—which existed independently of the physical brain and body of a living individual. Although there were a number of variations of this view involving greater or lesser involvement by the living individual, the essential feature was that the causal agency of psi phenomena lay outside the sphere of an individual's physical body.

As the experimental approach to psychic phenomena became dominant it began to look as though the causal agent lay more with the living individual than with some non-material entity. In other words, psi phenomena began to look like the end products of *human abilities*, just like the ability to see or hear, or the ability to lift or move something, only in the case of psi phenomena the mechanisms of reception and of action remained obscure. To be sure, many early experimenters, most notably J. B. Rhine, believed that psi ability involved some non-material aspect of the living individual, although the causal, or at least the initiating, agency was now considered to reside in the living individual. Thus the guiding philosophy of the experimental approach to psi phenomena has come to regard the production of psychic phenomena as a human ability. Within the experimental tradition this point of view is simply assumed in the routine use of expressions such as "psi ability" throughout the professional literature.

There has always been an undercurrent of dissatisfaction with the experimental approach, however. This tends to wax and wane in reciprocal proportion to the perceived successes or lack thereof in experimental research. At the core of this dissatisfaction is the belief that perhaps psi is not really an ability at all. It only looks like an ability because our simple-minded experiments force it into that mold. But since psi is not really an ability, the reasoning goes, the experimental approach is doomed to failure.

Most would agree that the experimental approach has been less fruitful than we might have hoped. Many reasons have been adduced to account for this state of affairs—everything from a simple and obvious fact that, compared with other sciences, there has been a pretty small amount of experimental research in parapsychology, to suggestions that elusiveness is in the very nature of psi. Most common, however, are those suggestions that psi is not an ability and we will never come to understand it by trying to treat it as one (e.g. White, 1985).

It is possible that this view is correct, or at least partially correct, holding true for those psi phenomena that seem very much unlike the product of an ability—haunting and apparition phenomena, for example—that are lumped together with our very ability-like laboratory phenomena simply because of our ignorance. I think, however, a far more likely reason why the experimental approach has been disappointing can be found in precisely the opposite direction. Psi *is* a human ability, but our experimental progress has been slow because we have neglected to take it seriously as an ability.

Although the experimental approach to psi more or less assumes that psi is an ability, we have not thoroughly considered the implications of what having psi as an ability means. For the most part, we have either not thought about psi as an ability at all, or we have accepted a naive view that ESP is some sort of extended communication ability and that PK is an extended motor ability. But, everything we have seen of ESP shows it to be a particularly unreliable means of communication and, by the same token, PK appears to be a decidedly erratic way of effecting change in the environment. Our problem seems to be that we have not been asking—really asking—the very basic questions: What is psi ability for? Why do we have it? What is the true purpose behind the somewhat eccentric communication and action functions that psi appears to constitute?

There have been many speculations as to what the purpose of psi might be in an abstract sense, but there have been very few that have taken psi as a human ability for their starting point. The only speculative foray in this area which has had practical significance for experimental research has been Stanford's Psi Mediated Instrumental Response (PMIR) model of psi functioning (Stanford, 1974a, 1974b). The basic idea behind PMIR is that an organism uses psi to accomplish something (the instrumental response) which fulfills certain needs of the organism. Following Eisenbud's speculations (Eisenbud, 1966–67), Stanford argues that psi may be far more common in daily life than is immediately apparent, but psi accomplishes its "tasks" very subtly and, quite likely, without the conscious awareness of the individual.

Illustrations of how PMIR might be operating in life abound: A fortuitous meeting with someone that, say, results in a new and better job, or the "unusual" missing of an airline flight that subsequently crashes. It is not just anecdotes which buttress the PMIR concept. A growing number of experiments, principally of the covert or non-intentional type, have supported Stanford's ideas. The PMIR model comprises some 18 "assumptions" which describe how psi should be expected to operate, but the most important ideas can be summarized in just a few points.

1. Psi (as PMIR) operates in an individual's daily life far more than is commonly realized.
2. The chief function of PMIR is to accomplish certain goals or fulfill certain needs of the organism.
3. PMIR operates for the most part *unconsciously*. Not only is the operation of psi not normally under the voluntary control of the organism, but the needs which PMIR serves may not even be consciously recognized.

Stanford's PMIR remains the most important consideration of psi as a faculty in service of an individual's needs to date. While it has been hailed as something of a conceptual breakthrough on the theoretical level it has not had the impact it should have had on experimental parapsychology. With some notable exceptions—mostly Stanford's own research—experimental parapsychologists have done little more than pay lip-service to psi's need-serving character. As Weiner (1987) has noted, this all too often arises as an unsatisfactory *post hoc* search for who had the greater motivation to use psi in an experiment that yielded unexpected results.

Trying to discover who has the most reason to use psi after we have finished an experiment is going about our research backwards. If psi is an ability then it makes no sense at all unless it is fundamentally need-serving and, if we want to capture psi in our experiment, the *first* thing we should be doing is thinking very long and hard about how a need-serving psi might be operating, *before* we design the experiment.

### *Psi as an Evolved Ability*

Given the little that we know of psi at present, it is clearly premature to attempt to identify particular needs or specific ways psi may help to fulfill these needs. We can, however, begin the process by considering the overall framework within which we should be looking. If psi is like all of our other many abilities, than our framework comes from evolutionary biology. Unless we want to take the position that psi ability is something conferred directly upon us by the gods then we must recognize that psi ability as we see it and we exercise it is the result of evolution. Psi ability has been molded and shaped by the same selective evolutionary pressures that have shaped our other abilities.

Evolutionary biology provides us with a fairly simple and straight forward answer to the question of psi's purpose. Indeed, contemporary interpretations of Darwinian theory have a very basic "bottom line" for the explanation of any ability or behavioral pattern: it serves to help the organism survive and pass on its genes to the next generation. The bottom line is *survival*, but it is the survival that the biologist speaks of, not the parapsychologist.

The generally accepted position among evolutionary theorists is that selective pressures of evolution operate at the levels of individual genes. The British biologist, Richard Dawkins (1976), has argued that the true survivors in natural selection are the genes rather than the species. The genes insure their survival by enabling the host organism to acquire whatever abilities and characteristics are necessary to insure successful rearing of offspring and, in turn, leaving them well positioned for successful reproduction. Whether Dawkins' "Selfish Gene" model of survival proves to be the best fit, the general schema of evolutionary biology does provide a starting point from which we can begin to understand the function of psi. To the extent that psi ability is a product of human evolution then its function is to help insure the individual's *biological* survival. Psi is need-serving and those needs are going to be important ones which contribute to the individual's health and well-being so as to make that individual better able to reproduce.

I think we must be prepared to recognize that much of the psi that originally attracted the attention of researchers—the D. D. Home's and Palladino's of the world—may well be aberrations. They are, of course, aberrations worth studying—in the way we study persons capable of great feats of memory or mathematical and musical prodigies—but they are not representative of *normal* psi. Any attempt to understand the nature of psi based on such individuals may be misleading and not at all relevant to the ordinary persons toward whom our experimental efforts are usually directed.

As a survival-related product of evolution there are several characteristics that we could reasonably expect of the psi ability. First and foremost it would be need-serving, but those needs would necessarily be non-trivial. The primary function of psi is probably to help the individual survive when faced with serious threats to health and safety, *but it is also to gain a competitive advantage* in the struggle for survival. Fortunately, for most of the human race survival is not the physical struggle it was centuries ago. However, life remains full of competition for success, not only personal success in continuing to survive, but also reproductive success and success in rearing offspring and leaving them well-positioned in a competitive society. To a large extent for *homo sapiens*, physical competition has been replaced by psychological and emotional competition. Evolutionary psi may not only be "missing" an airline flight that crashes, but also so called "intuitive" business decisions which contribute to personal success, or perhaps "chance" encounters with persons that result in some benefit coming to the individual.

A second characteristic that we would expect of evolved psi ability is that the organism will recognize—though not necessarily consciously—those situations which have sufficiently serious consequences that the application of a little psi could well benefit the organism. Conversely, and very important for those who design experiments, the organism will recognize when there is no need to use psi. This is simply fundamental to the notion of an ability—the organism will know when to deploy that ability to its best advantage and when to conserve it. This does not mean that psi will *only* be used in crisis circumstances or that any event which can plausibly be attributed to psi must have some vitally important need behind it. It does mean, however, that the rules for deployment which evolution has programmed into psi ability may be far more complex than we typically have been prepared to deal with in our experiments.

A third characteristic, related to the previous one, is that the manner in which psi is normally used will conform to what is known in evolutionary biology as an Evolutionarily Stable Strategy (ESS). An evolutionarily stable strategy is defined as a strategy, that is, a pattern of behavior, which, if adopted by most members of a population, cannot be bettered by any alternative strategy. Since a population is composed of many competing individuals, the strategy which persists, once evolved, will be the one which cannot be bettered by any deviant indivdidual. Based on a cost-benefit analysis for the individual the concept of an ESS can explain why, in a hypothetical population, it may be in one's best interest to be aggressive, say, 60% of the time and submissive 40% of the time. Obviously, we are not here talking about *conscious* strategies, but patterns of behavior which have, over time, proved to be the most effective in promoting an individual's ability to pass on its genes.

For the parapsychologist, an ESS may go some way to explaining a curious discrepancy that has often been noted, most recently by Braude (1986). If psi seems so unlimited in power, as evidenced by the so called "super stars," as well as by many spontaneous cases, why does it appear so circumscribed and ephemeral in the laboratory? Well, if psi ability is generally evolved among the population as part of a survival strategy, than we have to remember an important fact: if I have psi ability, then probably you have psi ability, and there is going to be a lot of competing psi out there, too. Some of it could well be more effective than mine. In other words, the psi we find in real life will probably be that which has evolved to deal with all the competing psi using a strategy which is *most likely* to benefit the individual in the long run: gain a little advantage here, give a little ground there. We are a long way from being able to do a cost-benefit analysis of various types of psi behaviors, but if psi exists as a human ability it probably fits into this model. Apart from the occasional deviant individual, the psi that we find in life is no doubt the result of a finely tuned evolutionarily stable strategy.

The idea of psi ability that is part of an evolutionarily stable strategy leads to some very interesting speculations about possible subsidiary aspects. I stress that these are speculations, or at least even more speculative thoughts than the foregoing, but I think they are worth bearing in mind as we seek to design ways to capture psi in the laboratory. The first of what I think would be some likely possibilities simply echo the points made by Stanford (1974a, 1974b) and Eisenbud (1983), namely that the operation of psi is ordinarily not subject to conscious control. Not only may we be largely unable to control our psi ability by deliberate conscious intent, but the goals and needs which psi serves may be very different from the ones which an individual consciously holds important. Indeed, it seems quite possible that what we might term "evolutionary wisdom" has determined that conscious control of psi is counter-productive, so "normal" psi is deliberately de-coupled from conscious intention. Of course, from the beginning J. B. Rhine was saying that psi is unconscious, as have many other parapsychologists and many of the psychics who have been studied. I have found it intriguing to think that perhaps psi ability is emerging in our species as a survival strategy designed to counter the advantage that consciousness confers on our competitors.

Related to this is a second quality that it is entirely reasonable to expect to find in an evolved psi. Psi may be elusive and obscure by design. Effective psi may need to be imperceptible psi and its elusiveness in the laboratory may be a by-product of its essential nature. Again, the trial-and-error methods of evolution may have determined that if psi abilities become too obvious, then the individual's chances of living long enough to reproduce and raise offspring may be seriously curtailed.

Psi may have evolved to be deliberately self-obscuring for its own purposes, that is, it works best when it is not noticed by the individual it is serving, and it may even be necessary for the individual's protection that the operation of psi remain secret. Batcheldor's (1984) concept of "ownership resistance" may have something to do with this aspect of psi. Certainly in shamanic practices, where we seem to have relatively controlled uses of psi by individuals, there are elaborate rituals and attributional characteristics to protect the practitioner from the harm that might otherwise befall someone displaying psi too ostentatiously. Likewise, the yogic tradition, which claims that one can achieve conscious control of psi at certain levels of development, assiduously warns practitioners against pursuing this tempting by-product of spiritual development.

One can go on speculating about the nature and the characteristics one might expect to find in an evolved psi ability—and I certainly think that we must continue to do this—but for the moment you are probably wondering what all this speculation has to do with experimental methodology, the theme of this conference. My answer is this: it has *everything* to do with experimental methodology. How can we design experiments to show us psi in action if we have no idea what psi is for? If psi is evolved to service fundamental needs related to an individual's health and well-being, are our experimental manipulations actually affecting anything that is related to the use of psi? At the very least, I think much of our methodology has been created in total obliviousness to any serious thoughts regarding the purpose of psi. Most probably, however, if we are to take the concept of an evolved psi ability to its logical end, it will force a radical re-examination of the methods that are used to test psi ability or solicit its appearance in the laboratory.

Some years ago there was a great hue and cry in psychology, particularly among the cognitive psychologists (e.g. Neisser, 1976), about the lack of what was called "ecological validity" in much of contemporary psychological research. Many psychologists rightly complained that researchers were trying to draw conclusions about real-life behavior or cognitive functioning based on highly artificial experimental circumstances that bore no relationship at all to the real-life situations they were trying to understand. These experiments lacked ecological validity—they did not accurately represent the circumstances which obtained when the behavior was observed in the real world. Generally parapsychology has been quick to adopt trends and techniques from orthodox psychology, but somehow the concern for ecological validity in experiments has completely passed us by. Granted, it is difficult to design ecological valid psi experiments when one has no good idea what the purpose of psi ability is. But, if we want to emerge from the malaise engendered by weak and contradictory findings, ephemeral and frequently unrepeatable results, then we will have to start paying attention to the ecological validity of our experiments. For too long we have been doing the equivalent of saying that finger-tapping or leg-lifting is a test of an individual's ability to run fast.

The key to making a start towards ecologically valid psi experiments, I think, lies in a serious consideration of the basic purpose of psi ability and the human needs that psi serves. While we can more or less deduce that psi has evolved to serve important, survival-related needs, these need not be limited to countering or escaping immediate threats to life and limb. Probably most of the needs that psi serves involve *promoting* the individual's well-being, both physical and mental, which are fundamental factors in an individual's ability to survive, reproduce and rear offspring in human society. I suspect that "ordinary" psi in daily life will look a lot more like intuition and luck instead of telepathic dreams or metal bending which could well be extreme or even deviant examples of psi ability. Indeed, I think we must realize that if evolution has been doing its job then our psi ability is likely to blend seamlessly with normal cognitive and motor function, and not stand out or call attention to itself as something radically different.

All this presents quite a challenge to the experimenter. How can one create a test situation that causes a subject to use psi without making it a death-defying contest for survival? How does one create ecologically valid psi experiments without running afoul of human subjects review boards? I do think it is possible to create psi tests that possess sufficient ecological validity for research purposes, but I do not think it will be easy. Ecological validity is, nonetheless, a goal that we shall all have to work towards if we wish to increase the stability and reliability of our experimental findings. We will have to create experiments that give our subjects *real* reason to use psi ability. Simply asking a subject to "use your psi" can no longer be considered sufficient.

I should like to suggest three interrelated areas in which we could start taking steps to deal with psi as a human ability. If we can at least keep the evolutionarily determined nature of psi ability in mind as we deal with these aspects of our research, we may put ourselves in a better position to tap that ability in our experiments. Of course, I do not wish to imply that no one has ever tried to do this before. Certainly many parapsychologists have made inventive and productive attempts to confront this issue, but what is required now are *sustained* efforts to treat psi as a need-serving ability.

### *The Nature of the Psi Test*

This first area that we can work on concerns the intrinsic nature of the psi test. Are there ways in which we can design the psi test so that it really does challenge a subject to use what psi ability he or she may possess? Can we increase the motivation to use psi, paying careful attention to the differences between intrinsic and extrinsic motivation as Robinson (1982) has advised? There is good reason to suspect that many of the needs that psi serves are psychological ones—such as desire for approval, feelings of competence and self-esteem—all of which may appear only obliquely related to survival and reproductive success, but all of which contribute to overall psychological health and physical health. Ultimately one's psychological well-being will have a lot to do with success in reproduction and the rearing of offspring. I suspect that many of our past experiments have accidentally tapped these needs. What we must begin doing is employing these needs more systematically in our experiments.

There have been a variety of attempts to make psi tests appear more like "real life." Among these has been the trend toward free-response picture tests as well as the specific techniques of remote viewing and dream ESP. For the most part, however, these techniques are addressing only the cosmetic aspects of psi ability. They are mimicking the way psi appears in life, but they are not touching the needs that drive psi ability. Granted, each of these techniques has a plausible rationale behind it as a means to facilitate the appearance of psi, but of themselves they do not deal with the subject's need to use psi. They may, incidentally, tap certain intrinsic needs, but of themselves they do not seem designed to trigger any real need to use psi. It is as if these techniques are equivalent to cleaning our microscopes and slides and even boosting the optical resolution, but the techniques themselves do not put anything on the slide to observe.

Examples of experiments that do attempt to tap subject needs can be found in the literature and often they provide tantalizing hints that have not been adequately followed up. Usually these come under the heading of non-intentional psi tests because the subject is unaware that it is a psi task and thus has no deliberate intention to use psi. Johnson (1973) cleverly embedded a psi test in an academic examination with successful results that were replicated by Braud (1975) and Schechter (1977). Stanford and his colleagues used a clever "work release" approach in which suitable responses (presumably involving psi) in one part of an experiment enabled the subject to avoid a long and tedious task and replace it with a short, reasonably pleasant task. The series of experiments by Stanford and his colleagues (Stanford & Associates, 1976; Stanford & Castello, 1977; Stanford & Rust, 1977; Stanford, Zenhausern, Taylor, & Dwyer, 1975) were not uniformly successful in demonstrating psi effects, but they did represent an important first step.

An approach which I have been using for over a decade has been to embed psi tasks in computer games. In these tests the experimenter is to some extent trying to second guess psi ability by creating a situation in which he hopes the subject's intrinsic motivation to win, feel competent, succeed and receive praise—whatever—will be brought to a sufficient level that the subject's psi ability comes into play. Of course, the technique of embedding psi tasks in games has numerous precursors in the pre-computer era, but there is some question whether the simulated baseball games of Ratte (1960) and other efforts really tapped the necessary motivators.

Although the strong appeal of computer games is obvious, it is equally clear it is not universal. Probably only a small percentage of the population find them appealing and that will largely depend on the type of game involved. Some are intellectually challenging while others require mastery of no small amount of skill—such as piloting a high-speed aircraft. For largely technical reasons (principally the lack of programming resources), parapsychologists have been limited to relatively rudimentary games in which to imbed psi tests. Although psi-games have been available at a number of labs for some years, there has been very little systematic research exploring their efficacy in creating the conditions under which a subject's psi ability might be brought to bear.

Lately, at the Institute for Parapsychology, we have had some modest success with a fairly simple computer game, but one which contains some of the elements that fill gambling resorts and casinos around the world—the lure of Lady Luck, of beating the odds and winning in a game of chance. This is, of course, our computerized dice game called P-OINK (Broughton & Perlstrom, 1985, 1986). By placing this simple game of chance in a competitive setting, i.e., by leading our Duke student volunteers to think they were competing against UNC players, we found that we had a very powerful motivator, at least as far as we could judge from external indicators. What we found in that experiment, however, was something different from what we were expecting. We naively thought that the competitive element would produce simple higher scoring by our subjects, but reality proved to be somewhat more complex. We measured the subject's anxiety level prior to the tests and what emerged was that the competitive game made some of our subjects more anxious than others—not at all a surprising result. But what did surprise us, although it probably should not have, was that the scores in this game of chance followed the subject's anxiety scores in a negative relationship. The more anxious subjects did poorly and let their simulated opponent win while those who were at ease and relaxed were more likely to get a high score and defeat their opponent. When this effect was neatly replicated in a second experiment it caused us to think a bit. Perhaps some of our subjects were simply uncomfortable with the idea of competing against an unseen opponent so they used their psi ability to "opt out" by throwing the game, while their compatriots who were more at ease with competition used their psi ability to win. Of course, I am just speculating, but it is this kind of speculation that we may have forced upon us if we are to take psi ability seriously.

Among the attempts to design methodologies that give subjects reason to use psi ability, one of the most exciting is currently being used by Braud and his colleagues at the Mind Science Foundation (Braud & Schlitz, 1983). In this series of experiments subjects have attempted to influence the electrodermal activity (skin resistance) of another person. Since this is commonly taken to be a measure of psychological tension, the test can be portrayed as one of "healing" or relaxing someone who may need it. This "psychic healing" approach is deliberately intended to increase subject motivation to use psi and it certainly seems to be effective judging from the reports.

I should not wish to leave the impression that these experimental approaches are exemplars of ecologically valid psi experiments. Far from it; they are merely tentative first steps in a direction that more of our research must take. If present and future psi researchers keep ecological validity in mind as they conceive and design their experiments at least we shall have made a start.

## *Demand Characteristics*

A second area which will require our attention if we are successfully to cope with a true psi ability is what can loosely be described as the "demand characteristics" of the test environment. This aspect of the test will most probably interact with the short-term needs of the subject. Are the laboratory personnel and their manner such that the subject will be made to feel needed and appreciated for his or her efforts? Will the subject's need for approval or desire to feel competent be met by success in the psi test? Is the overall ambiance warm and inviting so that the subject *enjoys* participating, or does the experiment come across as an obligation to be discharged with as little bother as possible?

Clearly this is a matter in which experience has taught parapsychologists a thing or two. Some experimenters and some laboratories take great care with the overall ambiance and treatment of participants—and their results speak for themselves. I need not give examples here, but we are all aware of the disproportionate success that some labs seem to have. Simply throwing our hands up and labeling this "experimenter effect" may obscure a very important fact: these experimenters are probably doing something right! If psi ability is connected with the servicing of psychological needs, then the psychological characteristics of the experimental setting assume a greater importance than we have been giving them lately.

The dynamics of the experimenter-subject interaction and how it effects psi results has long been a subject of discussion and study in parapsychology. Much has been written about the personalities of our successful experimenters. Generally, these discussions and studies tended to focus on how pleasant, friendly, warm or outgoing the experimenter was, all of which are undoubtedly relevant aspects of a successful experimenter's personality. More to the point, however, but somewhat overlooked, would be the degree the which the experimenter can instill a *need* in the subject to use his or her psi ability to accomplish whatever it is that the experimenter wants done. It may be the experimenter's ability to trigger the subject's internal need to use psi that may be the most relevant dimension of the experimenter-subject interaction.

There is an oft-quoted passage from *Extra-Sensory Perception after Sixty Years* (Rhine, Pratt, Stuart, Smith, & Greenwood, 1940) which usually has been given as evidence that the early Duke researchers were aware of the power of experimenter differences. I think parapsychologists have often looked upon this quote for its evidential value, but missed the essential point that Rhine and his colleagues were trying to get across:

> The methodology at this important point may consist in great part of the art of handling people successfully. All of the skills and methods that can be devised by the experimenter for conveying encouragement, inspiring confidence, implanting a realization of the importance of the tests, and arousing and maintaining an ambition to perform well will be decidedly to the point. (Rhine, et. al., 1940, p. 341)

The real message of this quote, I think, is the importance of motivating subjects—giving them reason to use psi in the experiment. Rhine himself remains one of our best examples of an experimenter who could motivate subjects. Many of those who watched him in action could testify to this, seconding, no doubt, Gardner Murphy's observations:

> My mind goes back to the year 1934, in which I first visited Rhine at Duke University and saw the rugged force of the demands he made upon his co-workers and subjects. In the light of his glowing intensity it became possible to understand the accounts given in his book of the way in which he had driven some of his subjects in the demand to get extrasensory phenomena. (Murphy, 1949, p.13)

It would be a rare individual who could recreate Rhine's manner, but I doubt that we need to. Simply examining the demand characteristics and the whole psychological milieu of our experiments in the context of dealing with need-serving psi ability could go a long way to suggesting improvements. Not too long ago Robert Van de Castle was telling students at FRNM's Summer Study Program what it was like to be a subject at the Maimonides Dream Lab where some of our most dramatically successful experiments have taken place. When Van de Castle walked into the lab everything stopped for him and, in his words, "they made me feel like the most important person in the world." Clearly this is more than being nice to subjects and it undoubtedly feeds whatever intrinsic motivation to use psi may be present.

While to a certain degree we can manipulate the demand characteristics of our experiments, the qualities that most impressed Van de Castle are not the kind that we can easily simulate for experimental purposes. They are going to have to be genuine and palpable qualities of the experimenter and the laboratory. As I mentioned above, some labs have paid attention to these factors and have achieved impressive track records with psi results. Oddly enough, though, these labs with their impressive track records tend to earn not our praise and our interest, but our suspicions. It must be experimenter psi, we are inclined to say (if we are being charitable). Must it, though? Have we really studied in depth the alternative psychological explanations?

Our consideration of the demand characteristics of the experimental situation leads to another matter that deserves the experimenter's attention: how can we best match our subjects to the test situation? As I found out with our competitive dice game, the motivator, i.e., the competition, did apparently induce our participants to use psi ability, but in very different ways according to how they reacted to the competitive element. I must admit that we came close to missing this detail, but, to me at least, this proved to be one of the most revealing aspects of those experiments: different subjects reacted differently to the demand characteristics of the experiment. In orthodox psychology this would not be in the least bit surprising, but in parapsychology, I think, we are not always prepared to deal with individual differences effectively.

It is probably time for parapsychologists to start taking what we know of individual differences in psi results seriously at a practical level when designing experiments. Perhaps extroverts respond best in public situations where success in a psi test brings a good deal of attention and social approval, whereas introverts may show psi best when it results in quiet, one-on-one social reinforcement. Perhaps people who thrive on competition will excel in competition will excel in competitive psi tests, but those who dislike competition may perform well in helping or cooperative style psi tests. The truth is that parapsychologists know a lot about individual differences in psi performance, but we seem shy about incorporating this knowledge into our methodology—into the way we design experiments. Most certainly there have been many experiments that have studied or tried to capitalize on individual difference, but the insights gained in these experiments do not seem to cumulate in the next generation's methodology. For example, how often do we routinely screen for extroverted subjects to participate in an experiment designed to test an hypothesis unrelated to extroversion, but in a setting that may be more likely to motivate extroverts to use psi? Granted, it is more work, but it will be a lot more work wasted if there proves to be insufficient evidence of psi to test the hypothesis of interest.

Increasing the yield of our psi experiments is a goal that we are all working toward so it seems obvious that matching our subjects to experimental demand characteristics is a bit of fine-tuning that would repay our efforts handsomely. In this respect I find extremely exciting the work of the Psychophysical Research Laboratories showing that it is possible to develop a profile of the type of person who is likely to succeed in the ganzfield experiments (Honorton, Barker, Varvoglis, Berger, & Schechter, 1986; Honorton & Schechter, 1987). Whether it is the case that these successful subjects represent a class of people who respond best to the treatment, or that several researchers fuss over them and make them the center of attention for two hours, or perhaps simply possess sufficient intrinsic motivation to help out these hard working researchers, is not known yet. But at least it focuses our attention on the fact that different individuals are going to respond differently to our experiments.

### *Selecting our Subjects*

The question of matching our subjects to the test situation leads into the third issue that confronts us if we take psi to be a human ability. That is the matter of how we go about selecting our subjects. One of the important implications of psi being a human ability is that it is likely to be normally distributed among the population. Of course, the evidence concerning whether psi conforms to a normal distribution or a skewed one comprising a relatively few gifted individuals is a matter of some debate (cf., Millar, 1979), but if psi is like all our other abilities then the distribution would be fairly normal.

Could it be that what we have been taking to be evidence of psi ability is actually wrong evidence? If psi is primarily designed to promote an individual's survival, well-being and reproducibility what should we expect it to look like in life—guessing ESP cards? Bending spoons? Of course not. Properly functioning psi ability should result in well-adjusted, successful, happy individuals. What we have been taking as evidence for the possession of psi abilities may be representative of the extreme reaches of a normal curve. Psi ability that is better than average will probably look like the intuitions and hunches of Dean and Mihalasky's executives (Dean, Mihalasky, Ostrander, & Schroeder, 1974) or like the luck of many of Tanagras' examples (Tanagras, 1967).

This point was driven home to me a couple of years ago when Ed May was sitting in my office and I commented "You're so lucky, Ed, the way you manage to get good subjects." Ed explained that he was just following some advice which Russell Targ gave him some years earlier: "If you want good psi subjects, look for successful people." Ed May's subjects are drawn from amongst other SRI employees, the vast majority of whom were very successful individuals at the peaks of their careers. Needless to say Ed went on to contrast his subject pool with ours: harried college undergraduates in a demanding university trying to cope with everything from academic pressure to finding a career and quite possibly a mate. Whatever psi ability our subjects have, it is likely to be directed towards serving more important needs than those which our ganzfeld or computer games are likely to raise.

Traditionally parapsychologists have been caught upon the horns of a methodological dilemma. On one hand we want our results to be generalizable, so we try to sample randomly from the population. On the other hand, sampling randomly may be yielding random results around a mean that is not far removed from chance. What we really want to be doing, at least at this stage of our research, is sampling from the upper half of the distribution. Researchers studying athletic excellence do not draw their subjects randomly from the population at large so, if we were looking for psychic excellence, we should be targeting selected populations. Just how to do that I am not entirely sure, but I think the first strategy should be to give serious thought to how we would expect an evolved psi ability to appear in the world, and then hazard some reasonable guesses as to what our population should be.

*Conclusion*

In conclusion, I do not wish to imply that there have not already been attempts to deal with the three issues I have discussed. There have been and there are today very creative methodological treatments of all these issues. What I am arguing now is that a view of psi as a human ability suggests that we should be addressing all of these issues all of the time if we want to maximize the yield of psi in our experiments. We must approach each experiment with a broad consideration of all these issues. It would make little sense to select a population of highly successful business professionals and present them with a trivial and boring psi test. It would make sense, if we must use a trivial and boring psi test, to "sell" that test as being very important to succeed in, as I believe J. B. Rhine was good at, or embed it in a situation that fosters intrinsic motivation to succeed. It would not make sense to embed a psi task in an aggressively competitive computer game—as I did—and then expect all subjects to welcome the competition. If we truly want to see reliability and stability come to our experimental data we must a) begin making informed guesses as to whom we should be testing b) take care that the demand characteristics of the psi test and its surroundings mesh with the subject's presumed needs, or at least do not conflict with them and c) above all, we must test psi ability with tests that have consequences for the subject—tests that give our subjects reason to use their ability.

In the end we may find that psi ability is not terribly different from the ability to run. If a person is crossing a field and sees a snarling dog entering from the other side, that person may reveal a running ability he never thought he had. Similarly a person may derive enormous satisfaction in demonstrating great prowess in running ability which, when coupled with a competitive drive, will enable that person to exhibit running ability of Olympic proportions. But, if a person of any degree of running ability were walking down the street and passed someone on a corner who suddenly said, "Run down five blocks and back again," I doubt the person walking would be inclined to do anything but ignore the one on the corner. We parapsychologists have to make sure that we do not spend our careers standing on the corner.

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## *DISCUSSION*

SCHOUTEN: You said so much that I really have to restrain myself. First, you were talking about psi as an ability. To me an ability is a capacity a person has that he can apply whenever he wishes. Now I think that research in this field that we carried out with psychics has indicated that psychics certainly do not have an ability which they can apply on demand. Psychics can certainly have paranormal impressions, but if you ask them to have them at a specific time I am afraid they are not able to do it. So I think that psi is not an ability in the sense that you can apply it whenever you wish.

The second point that I want to make is that it often happens that parapsychologists have unbelievable, wide-ranging theories and concepts. Fortunately, they sometimes make also very sensible suggestions and I find the same with your contribution. I think that you make very sensible suggestions especially with regards to motivation and how to select subjects. I am not experienced in finding good subjects, so I am really curious to know how others in their room feel about it who have been more successful. As regards your idea that psi would have evolutionary aspects and serve for survival of mankind, I have two problems with it. First, I think it is really not a viable sort of idea. It might be, it might not be, but I am afraid you will never be able to prove it. It is at best a suggestion. Second, there is your idea that psi is need-serving in the sense that with a special individual it would promote individual mental well-being. Perhaps I am a bit cynical, but if I look around in this world I think psi is doing a really bad job if you are right. Third, is that, as far as I can see, it does not fit in with what we know about spontaneous experiences. Spontaneous experiences most often relate to what happens to other people. I think it serves a need in that respect, you are right, but it is more a concern about other people than a concern for the individual himself. To me that seems a bit contradictory to what you are saying.

BROUGHTON: In spontaneous cases it is true, a lot of them relate to other people in the sense that they are announcing something that is happening to another person, but there is an enormous number that actually directly affect the person, helping to avoid danger. I think if one looks at too many of the apparitional cases these are almost by nature announcement cases: something is happening to somebody. I have just been looking at some of Louisa Rhine's cases. There are an awful lot that are really, directly survival related because they helped somebody avoid a catastrophe.

SCHOUTEN: But statistically they are still in the minority.

BROUGHTON: Even the announcement cases, the ones that tell you about somebody else, can, in fact, be very helpful and very satisfying in ways that are perhaps very important psychologically. I would agree with you to some extent. My use of the word ability is rather fuzzy and rather non-specific in this paper and deliberately so. When I talked about this in my PA Presidential Address, I made it clear at the beginning that I am using the word ability here rather loosely because we could be talking about something that we would also call a faculty. Admittedly, an ability connotes in many ways the application aspect that you spoke of; we should be able to apply it somewhat consciously. It also connotes the idea that we should perhaps be able to train it and improve it. Some areas have seen rather meager results in parapsychology partly because if it is an ability that is trainable or learnable perhaps we do not know how to exercise it yet, we do not know what muscles we should be using. You are perfectly right in pointing out that I am using the word "ability" rather loosely. I would be willing to say that we could label it "faculty" but I just could not quite find the words that fit. As far as the theory goes, I certainly would not designate what I have been talking about as a theory. I like to think of it as a common sense approach. If psi is the product of evolution then all the rest of that follows, that it is need-serving, that it does help us to pass on our genes. Why else would we have it if it did not come that way. I think psi is a product of evolution as opposed to a product of a dualistic philosophy of something like that. Given that it is a product of evolution, then everything else follows. It has got to be functional or we would not have it.

STANFORD: First of all I really appreciated your paper. I am very excited about the prospects that it suggests for research. I do not think that what you suggest in your discussion, Richard, at all indicates that there are not boundary conditions. The PMIR model spells out boundary conditions in some detail. This is why everything is happening lawfully despite the fact that science would deny it. With regards to the matter of the many spontaneous cases where we receive information about other people, the vast majority of these concern people whom we love, who are close to us. This is not outside the framework of the kind of thing that you are talking about at all. Evolutionary theory, psychobiology emphasizes kinship selection, for example, and if we knew that people whom we love or who were kin to us were in trouble protecting them is a way of potentially perpetuating our own genes. So I think this fits in very well. In your paper you speak of the distribution of psi ability. I would like to suggest that that may be oversimplifying, that there may really be several distributions of personal attributes that are relevant to the performance of psi tasks. A psi task can occur outside the lab, in the real world. Now the number and kinds of variables and personal characteristics involved might depend on the nature of the psi task, so there might not be a gene for some specific psi ability as much as a lot of aspects of psi performance.

BROUGHTON: A lot of ideas just leap into mind as you talk, Rex, as they usually do whenever I am listening to you. I think the matter of boundary conditions raises one of the points that I did want to mention in answer to Dr. Schouten's question of how we might deploy psi ability. One of the things the Dr. Schouten mentioned was that obviously not everybody is using psi to the best advantage. Well, it could be a function of psi being normally distributed. There may be some people who are not deploying psi very effectively. They may be at the lower end of the distribution and employing psi in a kind of perverse way. Your point about the distribution of psi being perhaps subject to overlapping distributions of other abilities is I think, an extremely important one. I think it relates directly to what Dr. Rao has been saying and, indeed, it seems to be a theme at this conference that psi is probably integrated with all of our other abilities. The assumption that Dr. Rao mentioned that we have too often held—that psi is completely independent of everything else that we do—is probably wrong. Psi is probably integrated with every other aspect of our personality and hard as it may be we are going to have to deal with this.

MAY: First off, Richard, I do agree with you in the sense that I too suspect that psi is a normally distributed ability and I favor your approach. I came to this field as a newcomer in the early '70s principally because I saw robust phenomena. I felt that, yes, the statistics were weak, we were not as well trained in those days to understand a meta-analytical perspective. I was fascinated. Something does not have to be real to be a large effect. I am as equally interested in a shaman who can just slightly change his weight on a scale as opposed to someone who has levitated all the way to the ceiling. So I am a little disappointed at the pessimism that I hear about why we have such a small psi effect. I do not believe that we have a small effect. Another comment is that I was surprised to hear you say something that should not have surprised me, namely that anxiety does not produce good performance. I know in my own case when I am the slightest bit anxious I really screw up, so I was surprised to hear that.

BROUGHTON: I fully admit it. I should have known it best myself because whenever I am under pressure I do abysmally badly, particularly in psi tests, but even in games which I am playing with my daughter. I am just a very highly anxious person, so it should not have surprised me, but it did. I think your disappointment echoes what Chuck was saying, that we should not be beating ourselves over the head with our weak results. In fact our results are a bit more robust than we give them credit for. I did try to make a slight distinction between effect size, in a sense of strong results, and stability of results. This is a personal issue, but I thought about it a lot. In the various labs that I have worked at there is a lot of instability in psi results even though certain labs do come up with very good, very strong psi effects. I trained in Edinburgh, you know, and we could not do much there in terms of psi results. Indeed Edinburgh has a reputation for lack of success. From Edinburgh I went to Utrecht. So in terms of strong effect sizes I have seen both sides of the coin. There are certainly strong effect sizes. I find the meta-analysis work of the last few years some of the most exciting stuff in the field. It makes me proud to be a parapsychologist. At the same time there are labs and there are experimenters who are somehow not included in this stability or reliability and it is to them that this advice is given. I think that ones who are somehow not finding these same robust effects may be simply messing up in some rather obvious ways that we should be thinking about.

HONORTON: Well, I really resonated to much of what you had to say, Richard. Ecological validity certainly is a very important consideration. The whole free-response work and the dream work represents very good examples of an attempt to take psi as it is frequently reported in real life situations and translate that into the laboratory context. PMIR does also. I wonder, though, whether psi is an evolving faculty, something that is emerging, or whether perhaps psi is the victim of evolution in the sense that we know that our physiological senses are designed in large part to eliminate information. Now, if the more interesting theories of a dualistic nature have any validity, the Eccles, Thouless, Wiesner type of formulations, then perhaps psi really does not have anything to do with evolution and the manner in which we attempt to study psi, as communication, as direct influence, as a substitution for senses and muscles, may be an attempt to produce what is really not the primary function of the phenomena. That may involve something like mind/brain liaison rather than communication, for which our senses and muscles are much more appropriately developed. It may very well be that many of the spontaneous cases that we look at that suggest need relevance are artifacts due to the way parapsychologists and psychic researchers over the last century have tended to be most interested in cases that are evidential rather than the more trivial sorts of things that Rex has collected to illustrate PMIR, for example. If you look for psi like that it frequently seems to operate in a way that may be need related, but may also involve rather trivial aspects that are not terribly survival oriented.

BROUGHTON: I tend myself to be somewhat of a pluralist regarding theoretical interpretations of psi. The evolutionary view that psi is evolving is only one of several views. It could be wrong. But from a practical point of view, for the methodological perspective of this conference, I think an awful lot of our experiments assume that psi ability is something that we have. Therefore we ought to follow it completely through, play the whole scenario out and take it seriously as an ability. If in the end we find that this does not work, that in fact psi, in all the ways we test it, is still ephemeral and that there are reasons to think the Eccles, Thouless, Wiesner sort of approach is in fact a better fit, a better model, then we would be prepared to deal with that. At the moment it is rather difficult to deal with that approach methodologically. Let us just make sure we have covered our experimental bases thoroughly before we start abandoning them.

UTTS: I agree with most of what you said and I really enjoyed your paper. I certainly agree that psi is probably used in everyday life and I think it is a good idea to add tasks to experiments that would encourage people to use psi to get a favorable outcome. But I think we should also keep in mind that one of the most negative experiences you can have is to have your worldview shaken. We saw this in the sheep and goat effect, I think. So in building these tasks into experiments we need to be careful that the outcome is not going to have that negative consequence for the person doing the task. I see this for myself when I go to casinos. I have often wondered why I seem to be so lucky in qualitative tasks and not in quantitative tasks. Then I realize that I spent eight years studying probability and that if probability gets thrown out I am out of a job. So there are more negative consequences. Several people have brought up the point that perhaps psi is a combination of other abilities. People are also talking about it as being normally distributed. I want to point out that the central limit theorem would say that if indeed psi is a sum of several abilities then you would expect it to be normally distributed. Loosely speaking, if you combine a series of abilities and add them together you get a normal distribution, so that may be what is going on there.

BROUGHTON: I agree with your comments concerning the negative aspects of having one's worldview shattered, and I think Charley Tart has addressed that issue quite a lot. That is what I would consider the demand characteristics of the experimental situation. We cannot willy-nilly bring in subjects and say, "Hey, you are going to do miracles here." Even though the subject does not protest and does not even show much concern, we are dealing with some very strong potential challenges to worldviews. I think all too often we overlook this aspect. I would consider that as one of the demand characteristics of our experimental settings.

# CONFRONTING THE EXPERIMENTER EFFECT

JOHN PALMER

## *The Experimenter Effect and Replicability*

The experimenter effect (EE) in parapsychology is closely linked to the broader problem of replicability. If the experimenter's identity or behavior is a crucial factor in determining whether a psi experiment succeeds or fails, then replication by a wide range of different investigators will be difficult to achieve unless whatever causal factors the EE represents can be identified and controlled.

Although formal survey data are lacking, it is widely acknowledged by both parapsychologists and their critics that some investigators and laboratories have been more successful than others in eliciting psi effects. Such consistency is often evident across a variety of test procedures and with selected as well as unselected subjects. This is not to deny that some cross-laboratory replicability does exist in parapsychology, but successful results are not distributed evenly enough within the community of psi researchers to inspire a great deal of confidence that comparable rates of success would occur outside this community.

This more broadly based replicability is essential if scientists outside of parapsychology are to be persuaded that psi anomalies are worthy of their attention, let alone that they are paranormal. Replicability within the fairly circumscribed parapsychological community is unlikely to have much impact. Such recalcitrance by our scientific brethren may be unjustified and even unfair, but that is beside the point. If parapsychology is to compete in the larger scientific arena, we must have procedures that we can export to outside scientists with reasonable assurance that they will succeed at the same rate they do within parapsychology. This does not mean that we must accede to the demand that hostile conventionalists be able to replicate our results, but it does mean that a healthy cross-section of benevolent conventionalists and neutrals be able to do so.

Two more specific factors can be mentioned that suggest to me the prospects of exporting psi success may not be as great as is sometimes assumed. First, some of the statistical methods currently being used in our meta-analyses and other literature reviews may be giving us inflated estimates of the degree of replicability that exists *within* parapsychology, at least in the sense that is relevant to the export issue. A case in point is the Stouffer Z statistic (Rosenthal, 1978). This test gives a valid and powerful estimate of the degree of statistical significance within a data base. Along with the complementary file-drawer statistic (Rosenthal, 1979), it can obviate objections that unreported nonsignificant studies in the parent population can wash out the results of the significant ones. However, it does not give a very good indication of how significance is dispersed within a population. Like the traditional CR method, it provides no empirical estimate of the variability of outcomes among experimental units. It is quite similar to the sum-of-squared-CR method introduced by Rhine and Pratt (1957), except of course that with Stouffer's Z the component Zs are not squared and the sign of the deviation is thus taken into account. However, in both cases one very large component can result in a significant summed value even when balanced by several components near zero. Indeed, it is this very characteristic that makes file drawer analyses so powerful.

To illustrate that statistics like Stouffer's Z can produce inflated estimates of replicability, consider the 28 direct-hit ganzfeld studies recently reviewed by Honorton (1985). Using Stouffer's Z, the aggregate result of these studies is highly significant ($Z = 6.60$, $p < 10^{-9}$), 1-tailed, indicating (quite properly) strong evidence for psi. The result is essentially the same when component Zs are computed for all the studies conducted by a given investigator and summed over investigators ($Z = 6.16$, $p < 10^{-9}$).

Compare the above significance levels to those obtained using the single-mean $t$ test, a method that does provide an empirical variance estimate. Using the *experiment* as the unit, the outcome is still very significant, although not quite as significant as with Stouffer's Z ($t(27) = 4.22$, $p < .0005$). However, when one takes the *experimenter* as the unit, the significance is much less than what is obtained with the comparable Stouffer Z and drops into the marginal range ($t(9) = 2.58$, $p < .02$). Moreover, the mean component Zs of three of the 10 investigators are negative.

As noted by Stanford and Palmer (1972), it is this empirical variance reflected in the error term of the test statistic that allows the statistic to serve an inferential function, i.e., to support generalization beyond the sample. Of course, it is this generalization that is at the heart of the export issue. While even the $t$-test analysis offers encouragement in this regard, it is much more equivocal than the encouragement one might draw from looking at the Stouffer Z.

Again, let me stress that the above discussion is not meant to deny the obvious statistical significance of the ganzfeld database, nor does it deny that within the sample the results cannot be wholly attributed to one or two investigators. Nonetheless, one should be cautious in promoting the ganzfeld as a widely replicable technique for eliciting psi effects until success is demonstrated on a broader scale. Although Honorton (1985) and other researchers have not made this claim, terms like "replicable" could be interpreted as implying it unless they are qualified.

A second, and more fundamental, reason for caution about the exportability of psi success is that the population of parapsychologists is probably not representative of the broader population of scientists (even excluding hostile conventionalists) on variables that many parapsychologists believe are relevant to psi outcomes. Parapsychologists, for example, are more likely to have positive attitudes toward psi, to be absorbed in the subject matter, and to be more highly motivated to achieve positive results than even benevolent outsiders. Thus even if one views the replication rate within parapsychology as providing a sound basis for generalizing to other parapsychologists, it is not really a sound basis for generalizing beyond that.

I do not wish to imply that parapsychologists are unaware of or insensitive to the replicability problem in parapsychology. Indeed, a recent Parapsychology Foundation conference was devoted entirely to this issue (Shapin & Coly, 1985). Likewise, attention has been paid to the EE and some progress has been made in coming to grips with it. Three major parapsychological explanatory frameworks have been proposed: differences in subject populations, differences in the way experimenters interact with their subjects ("experimenter psychology"), and experimenter psi.[1]

### *Subject Populations*

One possibility, suggested by Honorton (personal communication), is that the EE, at least in part, might be an artifact of the tendency of different experimenters to work with different subject populations. There has been some suspicion for years that ordinary college students

[1] There, of course, is also the conventionalist interpretation of the EE in terms of error and fraud by the experimenter. I have chosen not to address this possibility in the present paper, although I have done so elsewhere (Edge, Morris, Palmer, & Rush, 1986).

are, generally speaking, not a very good population for psi tests, and that better results are more likely to be obtained by concentrating one's efforts on volunteers who are especially interested in psi, practice mind-development techniques, are "successful" in life or their profession, or are engaged in occupations like art or clinical psychology that capitalize upon intuitive skills.

An important asset of this hypothesis is its easy testability. It can be confirmed by showing (a) that subject populations at successful and unsuccessful laboratories differ on certain operationally defined characteristics and (b) that psi scores at each of these laboratories are correlated with these characteristics.

A test of this hypothesis is currently underway involving Psychophysical Research Laboratories (PRL) (a successful laboratory with the ganzfeld) and Foundation for Research on the Nature of Man (FRNM) (a not-so-successful laboratory with this technique). Honorton and Schechter (1986) have shown that first-time subjects in the ganzfeld at PRL who have participated in other psi tests, practice a mental discipline, had many different types of psi experiences, and have a certain profile on the Myers-Briggs Type Indicator (MBTI) are more likely to score positively than other subjects. (Schmidt & Schlitz, 1988, who work at a laboratory noted for success in REG PK experiments, have recently replicated the MBTI pattern for this latter type of experiment). Kanthamani is currently in the process of analyzing comparable data from ganzfeld subjects tested at FRNM. It is already evident from preliminary inspection of the data that the PRL and FRNM samples differ on relevant demographic characteristics. Should the significant mean differences and correlational effects found at PRL be confirmed by formal analyses of the FRNM sample, a major piece of supporting evidence for this interpretation of the EE will be uncovered, at least insofar as the ganzfeld is concerned.

### *Experimenter Psychology*

There is a good deal of experimental evidence suggesting that the ability of experimenters to make their subjects motivated to succeed, confident, and at ease in the test situation facilitates psi scoring (White, 1977). However, exactly what is crucial about the subject-experimenter interaction is not so well understood. Research in psychology on experimenter bias effects, such as the work of Rosenthal and his colleagues (Rosenthal, 1966), suggests that such factors might be quite subtle and include nonverbal behavior.

These contributing factors are likely to be mediated to some degree by the experimenter's own belief in psi and its likelihood of appearing in the experiment. This could be a stumbling block in getting other scientists to successfully replicate psi experiments, as noted previously. One solution, suggested to me by Truzzi, would be to have such persons (including, in particular, unsympathetic conventionalists) farm out the actual testing of subjects to successful psi experimenters while maintaining in their own hands crucial control features such as the target order. A prototype for such an approach has already been developed (Schmidt, Morris, & Rudolph, 1986). Care would need to be taken, however, to be sure that the unsympathetic investigator does not create a negative climate that could adversely affect the mood or confidence of the experimenter and/or subjects. One case where this may have happened in the past was the unsuccessful attempt by Foulkes and colleagues to replicate the Maimonides dream experiments (Van de Castle, 1977). Such "climatological" factors might also help explain why different experimenters at the same laboratory often seem to get the same kinds of results; they absorb the attitudes and confidence level of the principal investigator or lab director.

A frequently overlooked aspect of this problem is the effect of experimenters' past track records on the attitudes with which they approach subsequent experiments. Experimenters who have gotten significant results in the past, especially if they have done so frequently, will likely approach their next experiment with an air of confidence that is bound to be communicated to their subjects. By the same token, unsuccessful experimenters are likely to find it difficult to avoid communicating a corresponding lack of confidence, however hard they may try. In other words, success breeds success and failure breeds failure. This factor might help explain why the divergence of results at different laboratories seems to be so stable over time.

One sanguine observation that can be made about all this is that experimenters' beliefs and attitudes are not communicated to subjects directly but indirectly by means of verbal and nonverbal behavior. If we knew more precisely what those behaviors are, we might be able to train experimenters to act accordingly, whatever their underlying attitudes happen to be. Alternatively, we might be able to find experimenters with the desired attitudes who act this way naturally. This obviously is easier said than done, but the idea is worth exploring. Specifically, we might consider creative use of videotaping. A good first step was taken several years ago in experiments where students rated videotaped excerpts of psi-conducive and psi-inhibitory experimenters speaking at a Parapsychological Association convention (Schmeidler & Maher, 1981; Edge & Farkash, 1982). It would be more useful, however, to have videotapes of such experimenters actually conducting a test session, including the all-important orientation or get-acquainted period. Analysis of such videotapes, informed by what social psychologists have already learned about factors influencing attribution of traits, attitudes, etc. might provide hypotheses that could be tested by systematically manipulating the experimenter behavior variable (either between or within experimenters) and observing its effects not only on psi scores, but also on various behavioral, cognitive, affective, and even physiological variables. Such a research program would be difficult and time-consuming, but it might also be very important in helping us to conquer the EE. Other useful suggestions for tackling this problem have been offered by Stanford (1985).

*Experimenter Psi*

The other major alternative interpretation of the EE is psi input by the experimenter. Traditionally, it has been assumed (implicitly, at least) that conscious intention and effort are necessary to produce psi in the laboratory. In an REG experiment, for example, where a subject and experimenter are seated side-by-side and the experimenter instructs the subject to attempt to bias the output of the machine paranormally, it was assumed that the subject is the only potential psi source, the reason being that only the subject is making a conscious effort to influence the machine. This linkage of psi to conscious effort was never very plausible, for a number of reasons. First, it was never supported by a theoretical rationale and, in fact, much emphasis has traditionally been placed on the unconscious nature of psi (Rhine & Pratt, 1957). Second, psi in the real world (e.g., poltergeists) does not seem to involve conscious effort or even conscious intent on the part of the agent or percipient.

However, what should have destroyed once and for all the linkage between psi and conscious effort or intent is the solid body of evidence in support of covert psi effects (Schechter, 1977; Stanford, 1977). In what is perhaps the cleanest demonstration of covert psi, Stanford, Zenhausern, Taylor, and Dwyer (1975) showed that an REG can be biased when *no one* is trying to influence it consciously, but the REG output has positive or negative consequences for the subject. What makes such findings relevant to the EE is that the output also has positive or negative consequences for other people involved in the experiment, most notably the experimenter. In the experiment of Stanford et al. (1975), it is at least intriguing in this connection that the more successful of the two experimenters was an extrovert, and there is considerable evidence from other lines of research that extroverts score better on ESP tests, at least, than introverts (Sargent, 1981). The main point, however, is simply that there is as much reason to consider the experimenter the psi source as any of the designated subjects. I am certainly not the first person to point this out (e.g., Kennedy & Taddonio, 1976; Stanford et al., 1975; White, 1976), but I still do not think its importance has fully sunk in among parapsychologists. If one thinks in terms of burden of proof, the burden should fall on those who maintain that the experimenter is not making some paranormal contribution to significant psi outcomes that are otherwise deemed of paranormal origin.

It is not as easy to conceptualize how the experimenter could be a psi source in ESP experiments as in PK experiments, because in the former the subject's conscious experience is by definition an essential ingredient in the exercise. Recently, the favored solution to this problem has been the observational theories (Millar, 1978), which treat all psi, in effect, as PK. In other words, observation of the data by the experimenter, which must occur at one level or another at some point in any psi experiment, retroactively causes the brain of the subject to create a mental image related to the target. However, the OTs are quite controversial (Braude, 1979; May, Radin, Hubbard, Humphrey, & Utts, 1985; Varvoglis, 1986; Walker, 1984, 1987). Indeed, the retro-PK evidence that serves as the main empirical foundation of the OTs can be accounted for (in my opinion, more parsimoniously) as real-time PK on the part of the experimenter or some other interested party.

However, one need not resort to the OTs to account for experimenter psi in ESP experiments. One alternative is Stanford's (1978) conformance behavior model; one would simply make the reasonable assumption that the experimenter is a "disposed system." Those of a more traditional bent can fall back on simple, albeit unconscious, ESP and PK. The experimenter, for example, might acquire knowledge of a target by clairvoyance and communicate it to the subject by active telepathy, or Stanford's (1974) MOBIA. In a personality-ESP study, he or she might also learn by clairvoyance who, say, the extroverts are and send the target information exclusively to them. More exotic mechanisms are also possible. To give just one example, an experimenter might unconsciously bias the timing of a ganzfeld trial so that the subject's (nonparanormal) stream of consciousness is intersected at a point when it happens to coincide reasonably well with the target. (This particular mechanism would not be plausible if the hit were striking, however.)

*The distribution of psi.* I find a tendency among parapsychologists to view experimenter psi as an all-or-none affair: either all the psi in a given experiment comes from the subject(s) or it all comes from the experimenter. Although this could ultimately prove to be the case, I think our best guess at this point is that the subject(s) and the experimenter contribute jointly to most outcomes.

The notion that psi ability is restricted to a small percentage of the population (Millar, 1979) has never appealed to me. Although there are isolated exceptions (e.g., idiot savants), most human abilities are distributed in the population in a manner roughly approximating the normal curve, whether or not they correspond to it exactly. Therefore, I think the odds strongly favor psi ability falling into this category. This does not mean, however, that in a given experiment the results may not be attributable largely if not entirely to one individual. First, even the normal-curve model allows for great individual differences in psi ability. Second, situational factors may dictate whether, or to what degree, this ability manifests in a particular experimental session.

Is there any way to tell what the relative strength of the experimenter's input is likely to be? One approach would be to examine the research literature to glean what variables seem to influence psi by subjects. The problem, of course, is how to be sure that the results of *these* experiments are not due to experimenter psi. For example, earlier in this paper I alluded to the review of extroversion-ESP research by Sargent (1981) to support the conjecture of experimenter psi in the Stanford et al. (1975) REG experiment. It is not entirely unreasonable to speculate that many of the studies Sargent reviewed themselves involved experimenter psi; certainly many of them were conducted by experimenters who had excellent track records in other types of experiments, most notably Sargent himself.

This, of course, illustrates the quintessential Catch-22 that the experimenter psi hypothesis confronts us with. There is no sure way out, but if we are willing to grant that subjects do contribute something, at least, to psi outcomes (which I think is reasonable), then it is also reasonable to suppose that some of the more replicable findings of process-oriented psi research are telling us something real about the factors that mediate psi success.

What, then, should we (tentatively) conclude from this research? The early observations of Rhine and Pratt (1957) and the covert psi research strongly suggest that needs and motivations are important. This factor clearly implies relatively strong psi input by the experimenter, because (at least with unselected subjects) the need of the experimenter to get positive results (or to confirm a hypothesis) is on the average likely to be greater than whatever needs the subjects bring to the task.

However, the experimenter may have other needs as well that may or may not support a positive result. If the psi process is really unconscious, some of the most important needs may themselves be of an unconscious, psychodynamic nature. If psi ability is fear-evoking at a deep level as some have suggested (e.g., Tart, 1984), then the experimenter's chronic or episodic level of ego-strength may be relevant. For example, experimenters may be prone to unconsciously inhibit their own psi at times in their lives when their self-concept is vulnerable to some personal crisis, even if consciously they may feel highly motivated to succeed. This same argument, of course, applies to subjects. The point is that assessment of needs may be more complicated and require more in-depth analysis than is often supposed. In any case, needs must be more precisely defined for experimental purposes (Weiner & Geller, 1984).

As far as the personality literature is concerned, we might expect experimenter psi to be more likely if the experimenter is an extrovert and accepts the reality of psi. As for situational variables, we might expect experimenter psi to occur most strongly when the experimenter is in a spontaneous, labile mental state, especially if this follows attention or concentration on the task (Stanford, 1974). I would argue further that this need not involve a conscious intent to use psi to influence the task outcome. It is not unlikely that such conditions are frequently met by experimenters in psi research (Kennedy & Taddonio, 1976).

*The chaos model.* If one carries the above thesis to its logical conclusion, the result can be both mind-boggling and depressing. This fact was vividly brought to our attention in an article written 25 years ago by Eisenbud (1963):

> Experiments are conducted on the curious assumption that the subjects in them will not use the very faculties they are being tested for (and over which, presumably, they have as little control as they have over the weather) until they step across the threshold of the laboratory and hear the starting gong, and that then they will use those faculties only within the confines of their designated roles in the particular design employed. . . .
>
> By the same token it seems implicitly to be taken for granted that experimenters (or "independent" judges or checkers or raters, for their part) will not, for whatever obscure reason, use any psi faculties *they* may have to muddy the field . . . Everyone behaves, in short, as if there were some sort of gentleman's agreement committing subjects, experimenters, judges and other participating personnel to

stick faithfully to their assigned roles in the experiment as scripted and to neither take any notice of nor infringe upon what any of the others are doing. (p. 258)

Eisenbud's remarks bear directly on the source-of-psi problem and reveal that it is even more far-reaching than I have implied so far. Up to this point I have been writing as if the only people we had to contend with were the subject(s) and (one) experimenter. In reality, there are other persons who may be as likely if not more likely psi sources than the experimenter; one prominent candidate is a principal investigator who is not actually testing the subjects, but is psychologically involved in the experiment.

Two other factors also add to the chaos:

1. *Space-time independence.* Although this matter remains controversial (Vassy, 1988), there is still little good evidence that the psi process is constrained by physical space or time. Even if there are such constraints, it is unlikely that they are so severe as to preclude a wide range of opportunities for various individuals to inject psi into an experiment. It is certainly unrealistic to suppose that the input would have to come at the time of a given experimental trial.

2. *Goal-directedness.* Several parapsychologists have made the point that psi seems to be goal-directed, in the sense that the complexity of the task seems to have little bearing on the likelihood of its accomplishment (e.g., Kennedy & Taddonio, 1976; Schmidt, 1975; Stanford, 1978). An important implication of this conclusion for the experimenter psi hypothesis is that single "bursts" of psi could have far-reaching effects on an entire experiment.

An example is the possibility that psi could determine an entry point in a random number table that produces a target sequence corresponding well enough with the response biases of a group of ESP subjects to yield a significant result. If one were to consider the amount of information one would have to process to achieve such an outcome by logical methods, the task would be staggering. But the assumption of goal-directedness precludes such considerations. Using complicated methods to determine the entry point helps not at all in light of this assumption. Although in the classic experiment on this problem, Morris (1968) found that the dice-throwing method obviated the bias, one could argue reasonably that Morris and his experimenter simply got the outcome they wanted or expected. It does not even stretch the principle too far to assume that the bias could be effected to some degree at least when a single entry point is selected for a series of experiments, as recommended by Stanford (1981), at least when effect sizes are small. This is not to suggest that we should not do all we can to frustrate such biases when the costs are not otherwise debilitating, but let's not be too sanguine about the success of such efforts.

The above considerations, if applied in full, lead one to predict total chaos in psi research. Countless individuals with all kinds of conscious and unconscious needs and motives would be competing to affect the outcome of an experiment, either positively or negatively. Subjects could use their psi ability not only to influence their own results, but also the results of other subjects, for example, by telepathically beaming incorrect target information (which they would not necessarily need to do at the time of the trial); in other words, the inputs of individual participants could be interactive instead of additive. The experimenter, principal investigator, lab assistants, secretaries, outsiders with interest in the outcome, all would add their input to the mix. Whatever psychological or physical factors mediate psi could not be anywhere near effectively controlled on such a broad scale. Chaos, indeed.

This scenario, however, points to the problem with the chaos model. Our experimental results in parapsychology, elusive as they often seem to be, are just not *that* chaotic. Various meta-analyses and more traditional literature reviews on topics such as the ganzfeld (Honorton, 1985), REG research (Nelson & Radin, 1988), and personality correlates (Sargent, 1981) reveal at least a rudimentary lawfulness in psi data. Ironically, perhaps the best single line of evidence contradicting the chaos model is the EE itself! In other words, the chaos model simply does not square with the data, which means, to put it bluntly, that the chaos model is wrong.

This implies that we must put some constraints on the scope of the various causal factors—number of psi sources, space-time independence and/or goal directedness—that uphold the chaos model. Unfortunately, we have no sound theoretical or empirical basis for deciding what those constraints should be. I would therefore suggest that we make our choice on pragmatic grounds. In other words, we should postulate the minimum constraints that would realistically allow empirical research on the source-of-psi problem, including tests of the experimenter psi hypothesis.

In this spirit, I would propose the following two constraints: (a) potential psi sources are restricted to those who are psychologically involved in the experiment, i.e., persons who are aware of it, think about it, and consider it meaningful or important; (b) psi sources can only affect an outcome (trial, target generation, etc.) within one hour before or during this outcome. These proposed constraints are obviously somewhat arbitrary and perhaps could be improved upon. Observational theorists, for example, would surely want to add a post-outcome time period for persons who observe the data in some form. However, we must agree on some set of such constraints if we hope to tackle this problem empirically.

*Testing the experimenter psi hypothesis.* The imposition of pragmatic constraints on the chaos model renders the experimenter psi hypothesis testable. Either a correlational or experimental approach could be adopted. The correlational approach in its broadest sense would require that experimenters observe and record during the appropriate time frame aspects of their mental states that could be relevant to the production of paranormal effects, including their levels of need or desire for a successful outcome, moods, general level of arousal, and activities that might reflect cognitive lability and opportunity for release-of-effort effects. Manipulating these variables, while desirable in principle, could sometimes prove disruptive and interfere with the experimenter's other duties. Also, some of these variables, like moods, would be hard to manipulate. Thus a correlational approach might generally make the most sense at the early stages of inquiry.

On the other hand, experimenters, particularly successful ones, might profitably consider manipulating their own mental states systematically to see if this affects the results of their experiments. Schlitz (1987) has noted that some successful psi experimenters intentionally put themselves in mental states that could be considered psi-conducive prior to or during their experimental sessions. It would be interesting to see what would happen if these experimenters treated such states as an experimental variable. Other predictor variables associated with psi success could also serve as the basis for experimental manipulations and testable hypotheses.

A related strategy is more holistic: to simply treat the experimenter as another subject. In a PK experiment, for example, the experimenter might actually join the subject in attempting to influence the REG, perhaps even acknowledging this participation openly. The experimenter would also fill out the same psychological tests as the subject and undergo the same experimental treatments. I am not suggesting here that conscious intent by the experimenter is necessary after all; what this approach accomplishes is to facilitate comparison between the experimenter and subjects by making their experiences and activities more uniform. This admittedly radical strategy has the added advantage from the point of view of the experimenter psychology hypothesis of helping to break down the barrier between the roles of "subject" and "experimenter."

But is such an approach desirable? Stanford (1981), for example, has warned us about the need to make up our minds whether we want to be "shamans" or "scientists". Indeed, should successful experimenters whose interests lie elsewhere worry about experimenter psi at all? There is something to be said for the recently overused adage: "If it ain't broke, don't fix it." If experimenter psi is a factor in the success of some experimenters, preoccupation with it might even create a psi-inhibitory mental set or experimental atmosphere that could undermine that success.

This attitude might be short-sighted, however. If experimenter psi is a factor in psi outcomes—and I believe there are good reasons to presume that it is (as I argued above) —failure to identify and deal with it could adversely affect the rate of replication at other laboratories, which (as I also argued above) is what ultimately will vindicate the experimenter's own success.

### *Interactions and Confounds*

Although I have devoted more space to experimenter psi than to the other interpretations of the EE, they all deserve serious and equal attention. My guess is that all three have some validity. They also interact with each other: for instance, if subjects are selected who have relatively great psi potential and are treated in such a way as to maximize their motivation, confidence, and ease, the relative psi input of the subjects vis-à-vis the experimenter or other interested parties is likely to be enhanced, as is the overall level of psi.

Second, it is not always easy to tease these interpretations apart experimentally. As an example, consider once again the experiment of Stanford et al. (1975) for which I had raised the possibility of experimenter psi by the extroverted experimenter. The authors, while acknowledging the possible role of experimenter psi in their study, interpreted this particular effect according to the experimenter psychology hypothesis: the extroverted experimenter may have facilitated a more psi-conducive social interaction with the subject. We have no way of knowing which interpretation is best. As a second example, consider my suggestion that experimenters might manipulate or control their own mental states to influence their own psi input. The problem here is that such manipulations carry the risk of affecting the experimenter's social interaction with the subject, thus providing a potential confounding of the experimenter psi and experimenter psychology interpretations of the EE. Any research program that seeks to deal incisively with the EE must keep these potential confounds and interactions in mind.

## *Conclusion*

As I look back over this paper, I question if I have said much that is really new. My main purpose in writing it, however, was not so much to break new ground as to sensitize my colleagues to what I see as one of the most important problems facing modern parapsychology. Although parapsychologists are certainly aware of the problem, and almost every discussion section of an experimental report nowadays seems to allude to the EE at least in passing, I have seen little evidence that most of us are yet ready to confront it head on. My main thesis is that until this attitude changes, we are not likely to see the breakthrough that many of us have devoted the better part of our professional lives to achieving.

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## *DISCUSSION*

HONORTON: John, I have a couple of comments. My own position has been that the absolutely terrible way in which we have tended to characterize our subjects in reports could alone be the source of experimenter differences. I doubt that that is the entire source of experimenter differences. I am inclined to believe that conventional experimenter effects account for a great deal of the variability. But I think we certainly need to enforce a requirement that experimental studies characterize their subject populations to a much greater, more precise degree than has been done so far. The experimenter psi hypothesis is a hypothesis that I believe has very little direct evidence to support it. It is a hypothesis that, as you yourself mentioned, has been attractive because it is plausible given the lack of known boundary conditions. It is also the lazy way out. You say, "I did not get results so it must be your psi rather than your subject's psi." And I think it has had generally a stultifying effect, one that has inhibited research for many years. I would like to see a more focused approach as you yourself do. Finally, on the Stouffer Z *t*-test difference that you raised in the beginning of your paper, certainly it is true that the *t* test is a more appropriate way of looking at replicability across investigators rather than just summing the Z scores by the Stouffer method. But I would argue that you should not be comparing how significant it is. You should be looking at effect size once again. Your $t$ of 2.58 with 9 degrees of freedom which is significant with a $p$ of .015 is equivalent to an average effect per experimenter of .82 standard deviations above the expected mean. Cohen's D and an effect size of .82 in psychology is considered a very strong effect. Furthermore, you say that the mean component Zs of three of the ten investigators are negative. That is true. That is, however, like saying that the water glass in front of you is a third empty rather than that it is two-thirds full. Seventy percent of the investigators have positive Zs and I think that is the point to be drawn from that particular assessment. The main thing that I want to communicate here is that you should not compare these things on the basis of $p$ values, but convert them into effect sizes because that gives you much more information about what is going on.

PALMER: As far as the descriptions of subjects are concerned, I certainly agree that we need better ones. I do happen to think that part of the experimenter effect may well be attributable to different characteristics among the subjects tested by different experimenters. I think I made that point in my paper.

As for there being very little direct evidence for the experimenter psi effect, there are certainly some supportive studies, such as the West and Fisk experiment, but there are not an overwhelming number of them. However, I think it is more than simply an *ad hoc* suggestion, as I attempted to point out in my paper. That is particularly true when you look at the results from the covert psi experiments, which I think provide indirect empirical support for the experimenter psi effect. But, again, I think that needs to be tested directly. I certainly agree that experimenter psi has been used in the past as a kind of a *post hoc* excuse, but that is not the way we have to use it. I would rather see us treat experimenter psi as an experimental hypothesis and test it directly, again, because I do believe there is a strong *a priori* likelihood that it is partly responsible for the experimenter effect.

With regard to the Stouffer Z, I think it is important to make a distinction between the magnitude of an effect, or an effect size, and its distribution. The issue in terms of replicability is not the magnitude of the average effect size. That could either be due, say to either four effect sizes of one, or two effect sizes of zero plus two effect sizes of two. These two outcomes have much different implications for the replicability issue. What is crucial in terms of replicability is not magnitude, but consistency. It is the estimate of consistency that I do not think Stouffer Z gives us very effectively. In terms of the three out of ten or seven out of ten, I agree that it's an example of the glass half empty or the glass half full, but I think part of the problem is that you have ten investigators. Unless you have very strong consistency, it is really hard to conclude anything from that analysis. A seven-three split with a sample size of ten is really not that much different from a fifty-fifty split in terms of statistical significance. I do think, by the way, that within the ganzfeld database, in particular, even when you do use what I consider the more appropriate statistics, there is still, good evidence of consistency. My point was not so much to question the conclusions about the ganzfeld, but to question the appropriateness of the Stouffer Z in drawing those conclusions.

SCHOUTEN: John, your talk focused on two important issues, that is the experimenter factor and its possible relevance for research and what you so nicely called the chaos model. Now as regards the second, you say, that everything points to chaos, but fortunately we see some order in that data and therefore the situation is not as bad as it seems. But it sounds a bit contradictory to me, because if you take the experimenter effect seriously, then in fact you are saying you can explain the order in chaos because some experimenters have some prejudice-isms but then indeed chaos reigns. Once experimenters get other ideas they will find other things, so I think that is not the reason to say the chaos model does not apply. I would say the chaos model does not apply because generally in science when you start, you start with a mess. In physics it took centuries before some really smart scientists found out what the regularities were. If you look at so called simple physical laws you can understand why. If there is a third power involved or four variables what an unbelievably difficult problem it must have been to sort it all out, considering also the noise. So I would suggest that you drop the chaos model for that reason. You just cannot say at the moment that there is chaos. We still have to look for regularities behind it. Your other point is the experimenter effect. I really think in the first place that there is weak evidence for it and, secondly, it is the easy way out. I think there are two other relevant points here. People tend to paint black and white pictures; some labs are successful, some labs are not. But I think the reality is much more complicated. In the first place, I think the labs who do have success have undoubtedly also run experiments which failed. I do not think there are 100% success rates. I know by experience that labs that are failing in this respect occasionally have experimenters or studies who turn out to be successful. Secondly, what bothers me a bit about the whole thing is that in practice most experiments are not carried out by the few experimenters we have in this field, but in reality are run by assistants, students and so on, who have most often different opinions than the experimenter who supervises the study. Unless you have these data you can not make a fair comparison. A third point is that I find it a bit overdone. If a subject is watching a screen in the RNG study or if you have a ganzfeld study where an agent is studying a target picture, we hope to see a little bit of psi in this situation. But then to assume that the experimenter who is outside that room will be equally able to influence the situation and the results is again assuming that a doubly difficult task will be as easy as a simple task. If the study is properly done the experimenter should be blind as to all the targets. To assume that he still has the same influence in the trial is to my taste a bit overdone. So I really think there are many reasons to be careful about the experimenter psi hypothesis.

PALMER: I concluded my paper denying that the chaos model applies. I do think that we need to put some constraints on it, and indeed it is those constraints that allow not only the experimenter effect but other parapsychological hypotheses to be tested. Ironically, as I pointed out in my paper, I think the best evidence against the chaos model is the experimenter effect, because it is an excellent example of order within parapsychology. It suggests, that the experimenter is having some kind of influence on the outcome, whether it be through his own psi or the way he treats subjects or the way he selects subjects or whatever. As far as the easy way out is concerned, I have to reiterate what I said to Chuck. I am not saying that we should use experimenter psi as a *post hoc* catch-all for anything that we cannot explain. I am simply saying that there is enough reason for us to take it seriously that we should investigate it more directly. The point was made that there is very little experimental evidence for the experimenter psi effect. To the extent that is true, it is not because, at least in my reading of the literature, that there has been a high failure rate among experimenters who have attempted to test the experimenter psi effect; rather it is because very few such experiments have been done, and that is my complaint. I am not trying to say that we should accept experimenter psi without evidence; I am simply saying that we should try to collect the evidence that will allow us to assess it, and that we haven't been doing enough of that.

As far as lab assistants are concerned, I think there is what might be called a lab ambiance that is different in some laboratories than in others. I am not sure who is responsible for that; perhaps if you had to assign responsibility to someone it would be to the principle investigator. But I think that ambiance does filter down to the experimental assistants or to other people who run the studies. I think there may be more uniformity there than perhaps you are assuming, even if the experimenters have somewhat different beliefs about psi. As for the implausibility of influences by experimenters who are blind or outside the room, if you are right, then the assumption of the goal-directed nature of psi for which there is some empirical support, is wrong. I see little reason, from what we know either from theory in parapsychology or from the experimental literature, to preclude someone in the next room who did not know the target, or even the experimental condition, from influencing the data. This is the kind of question that we need to investigate and not simply assume, as I think we have too often in the past, that if there is an effect, either it has to be the subject or, if it is the experimenter, then he has to be in the same room as the subject. Those are all assumptions that we have very little basis for making.

MORRIS: I was very glad to see you note that it is more that just other experimenters who may need to be taken into account. In fact, what you may have here is an entire experimental system all of which needs to be described much more thoroughly if we are going to do any kind of systematic hypothesis testing. It may be that what we are really talking about is the success or failure rate of an entire experimental system. We could try to erect as psi-conducive a system as we can, working with experimenters, trying to learn as much as we can about having them contribute positively to the system's overall results. The experimenter may well have contributed quite a bit in ways that we will gradually learn to be able to describe, so that when someone else tries to replicate, their experimenters will be in similar circumstances. An alternative would be to try to get the experimenters quite uninvolved, perhaps to train them to "keep their psychic hands off the data."

PALMER: I like very much your general point of looking at this with a systems approach. I did not get to it in my talk, but in my paper I put forth the radical suggestion that experimenters might actually participate in their own experiments as additional subjects. So instead of having an experimenter tell the subject to influence the RNG, both experimenter and subject attempt to influence it together. This might make the experimenter's behavior in the experiment more like that of the subject and make it easier to draw comparisons. The other approach is, of course, to try to isolate the experimenter from the situation as much as possible. I think this would be a very good thing to manipulate systematically and see what happens. It comes back to the point I was making before, that we need to take these possibilities seriously and start testing them.

ADAMENKO: Maybe the problem is how to control for the experimenter effect, how to decrease it. This is my first point. The second is that the replicability of psi is not important. I think it is very important to incorporate psi into mainstream science. We have a good example in nuclear physics. At the end of the last century Charles Richet suggested the use of statistical methods to investigate psi. Nuclear physicists took this idea because it is impossible to replicate "behavior" of elementary particles and they succeeded. Dr. Rhine used statistical methods to prove that ESP and PK exist. So my second question is, why do you believe that replicability is important to incorporate psi into mainstream science?

PALMER: I am not really acquainted with the situation in physics. I think parapsychology tends to be judged more by the standards that are applied to psychology, at least in the United States, in which field I think replicability is perhaps more of a factor. But my main reasons are two-fold. One is that when you have effects that are not extremely robust nor very closely linked to already existing theory, replicability is important simply to assure that there are no errors in the methodology. In other words, when you have different people getting the same effect it is somewhat less likely that any one of those outcomes is due to some kind of an artifact. You can always argue that maybe if different people replicate a finding they are all making the same mistake, but I think the problem of artifacts does get minimized to some degree with replication. My second reason is sociological. Given the particular situation we face in parapsychology, I think that other scientists are going to expect that at least some of them should be able to get these results themselves. I am not necessarily saying that is this fair or justified; but I think it is a reality that we simply have to live with.

With respect to your first point, controlling the experimenter effect is like controlling psi in general. Can we turn psi on or off? I think the evidence indicates that we can. Even if you assume that the subject is the source of the effect, we seem to have certain manipulations that facilitate psi missing as opposed to psi hitting. There are certain personality correlates that seem to suggest that some types of people score above chance, while other types of people score below chance. I think these same factors apply to the experimenter, simply as another psi source.

# GENERAL DISCUSSION
## DAY ONE

MAY: I am glad you like chaos because I am about to instill some. I have a number of comments that span all the talks this morning, principally addressing the issue of the conference in general. It seems to me that we have a direct problem to face methodologically in that we do not have a definition of our phenomena. ESP is what happens when nothing else could. One of the direct consequences is that we are confused as to who our target population is that we wish to convince. First off we have a problem with ourselves. We are arguing here whether something is real or it isn't. The next population that we seem to be in my view over-committed to are the skeptics at the other end of the distribution. What seems to be falling through the cracks in all of this is the vast middle group of very competent scientists who ask very difficult questions of our own work. I think our methodology would be better off if it aimed at answering their questions. That is certainly the approach that we are looking at at SRI. So for me that is a general problem. And a second comment that I want to make is if (and it is a big if) psi really is considered to be an ability and considering Jessica's comment that therefore it will be normally distributed in the population for a variety of reasons, then it just seems to me, when I look at some of the methodologies used by our colleagues, that summing across individuals to search for a psi effect when you have no indication at all that you are selecting your population from the right-hand half of that distribution is absolutely crazy. To give an example of that, if you locked myself and Itzhak Perlman in a room each with a violin and asked us to sum our results, you would conclude that violin playing is absolutely and utterly impossible. That is not the way we should look at exceptional behavior. It is just wrong to look at exceptional behavior by summing across people. So there is some chaos.

RAO: I think what Dr. May is saying is that if psi is normally distributed, then you cannot really test unselected subjects, because pooling their results would only confirm the normality of the distribution. This argument, I think, misses the central point of much of process oriented research in parapsychology which by attempting to separate hitting and missing scores in psi tests is really aimed at discovering the discriminators that would show whether a subject belongs to this side of the bell or that side of the bell. In working with unselected subjects under various testing conditions we are trying to identify those variables that would throw light on hitting and missing and what circumstance would enable, would highlight, would enhance the ability and what would inhibit it. So in that sense I do not think it is so crazy to work with unselected populations as long as you have ideas of how to discriminate them.

WICKRAM: I would like to comment on two papers—the one on ability and the one on the experimenter effect. The conceptualization of psi as an ability seems quite heuristic to me. I want to take objection to two concepts—one that all abilities have to be voluntary and two that they have to be adaptive. For example, human intelligence is generally regarded as an ability but there are certain conditions under which IQ may be used to induce pathophysiology and, in fact, psychopathology. In other words, people may use their intelligence for self-destructive purposes. So though an ability may generally be used for the purpose of survival, there may be special conditions under which it is used to produce pathophysiology and psychopathology. The other notion is that all abilities are voluntary. For example, there are conditions under which the ability to have erections will in fact induce detumescence rather than tumescence. Attempts to control certain abilities will in fact make them non-adaptive. The next comment is about the experimenter effect. My mentor was Hobart Mowrer. As those of you who are psychologists know, he was an experimental psychologist and a learning theorist. One of the things that he would always tell me is, "Ian, if you want to know which experimenters get positive results in running their rats, you have to ask only one question—do they have to use gloves when they handle their animals?" Those who have to use gloves when they handled their animals usually get more negative effects. So love may be one of the important conditions for producing positive experimental outcomes. Now, of course, the experimental psychology of love is a different issue.

BROUGHTON: I would just like to reply as a little of that was addressed to me. I love that analogy with gloves. I think that Charley Tart really picked up that idea too, on whether we approach psi with gloves on or not. As to your comments about the psi ability, I thank you for your support of my own vague notion of ability. It certainly may not be voluntary and indeed does not have to be voluntary. That is what I have been trying to stress in my own approach to it. As for the adaptive and maladaptive nature of psi, I think that is something which we really have to take into account. It relates to what Dr. Schouten mentioned in his question to me. It could be maladaptive. Keeping that in mind, however, just from a practical point of view why don't we worry about the adaptive uses first and take care of our maladaptive uses later.

STANFORD: With regard to something that Ed May said, I am not at all sure that psi is exceptional. I certainly think there are vast individual differences. I firmly am convinced that in almost any situation there are going to be individual differences in the ability to manifest psi. But I think that maybe one of the reasons we think psi is exceptional is because we are asking our subjects to do things in our experiments that, if you will, are ecologically rather ridiculous. They are not in the kind of context in which psi normally operates. I might make an analogy here to asking a poet, "Give me a poem; give me a poem right now!" This kind of thing is not going to happen with a good poet. I think we had better consider the kind of tasks that we put before people when we ask them to use, for utilitarian purposes, an ability that does not work in that kind of conscious or volitional way normally. The second point I want to make, before we get too far from John Palmer's paper, is that when we talk about experimenter psi we have got to recognize that one of the chief things that we need to worry about is experimenter concerns. Sometimes the most important, fundamental and obvious things are left out of a discussion. And I really can hardly think of anything that is of more concern to a scientist doing research—one who is worthy of the name scientist—than the desire for truth. What role does that play in experimenter psi?

HONORTON: It seems to me that the real importance of replicability is not in convincing critics of the reality of psi. Replicability is important for one reason only. As scientists we can only build on what we can reproduce. Now for 30 years the Rhine school promoted a series of key experiments that were done in the 1930s. These were like great works of art that were hung on the wall and admired for 30 years. Well those experiments long since ceased to be of scientific interest. They were works of art, of historical interest only. To the extent that we are interested in learning what is going on with psi or learning how to apply it, we have to be able to replicate our results. We can not build on quicksand. The other point that I want to make—and this will probably introduce further chaos into the proceedings—is that in talking with a number of people who regard themselves as having been less successful than they would like to be in this area my impression is that they simply have been unwilling to modify their behavior as experimenters in a way that, at least to me, would seem to be more productive in terms of producing results. It is all good and well to work with unselected subjects because they are convenient, because it is easy to do, but that is not where the action is. I do not see, on the part of any of the people who have consulted me about doing ganzfeld experiments, any serious effort to take my advice into consideration. So I am somewhat frustrated as someone who is labeled as a successful experimenter and who feels that more is needed than simply to consult with somebody who has been successful. You have to take what is said seriously and modify your procedures and see whether in fact they are right.

SCHOUTEN: A few comments on the points Dr. May raised. I would be inclined to say indeed we are not so much talking about phenomena, but about paranormal experiences of people. We should try to explain them. That is quite a different thing. What bothers me often in parapsychology is that it is a bit turned around. People have paranormal experiences, therefore we assume there is psi and therefore we study psi. I always found that a bit peculiar, but that is apparently the way it is. Another comment I would like to make is about your suggestion to take the subjects from that part of the distribution where you really find successful subjects. Well, of course, who would not agree with that? But there are two "ifs" in it: one is if it is an ability and the other is if it is normally distributed. I have seen many attempts to select subjects based on that model. We have tried it at Utrecht. I guess there are many experiments where we have tried to select subjects somehow and as far as I know most have failed. The next point I want to make is about Chuck's comment about the unwillingness to modify experimental procedures. Here again I think it is not so much a black and white situation. Most experiments which have been carried out in Utrecht which were not successful have been carried out by enthusiastic students and those students were always encouraged to bring their friends. It is not true that you have this on the one side and that on the other side. It is much more complicated in reality. So I think before we really go into this sort of thing we should know better how exactly successful experimenters did it. I am curious to learn, I am certainly willing to modify my behavior, but you see I can not find it in the literature how exactly to do it.

WALKER: There seems to be a great desire to communicate with other scientists, to convince other scientists, as John Palmer said, to let parapsychology be judged on the standard of psychology by psychologists, by critics. And this goes back to what Adamenko said with regard to replicability, about the great passion over replicability. I certainly understand that as scientists we should try to improve all the aspects of our science, but that is not what is going on with regard to replicability. Instead we want to convince other scientists that we are legitimate. Our preoccupation with this has become almost a pandering to other scientists, other sciences. This is not how other sciences work. A few months ago I was asked to write a commentary for BBS. I made the statement in my reply that if psychologists were to come to the community of physicists, to ask us whether psychology is a science, physicists would say thumbs down. If psychologists went to physicists for their justification, the result would be absolute zero. Yet it is a science that even a lot of physicists are interested in. If psychologists had tried to build their science by continually going to physics and saying, "Are we there yet, are we there yet?" they would not be there. They would never get there. My feeling is that you have to build your own science without any reference to most of the other sciences. They do their work, come to their conclusions and speak *ex cathedra*.

MAY: Harris, I agree with what you said. One aspect that I heard here today was that we have excessive concern for tight experiments or tight methodology. That to me is an oxymoron. You can not have excessive concern for that. I have grooves an inch deep in my back made by my fellow physicists when some of our work fails to take into account an excessive concern for methodology.

WALKER: I understand that.

MAY: You are not going to get the somewhat skeptical mainstream scientist to pay attention to us if we have even the slightest flaw in our methodology. I would not be convinced myself. As many of you know, I am a very strong skeptic about the whole field of psychokinesis, simply because I am aware that the methodologies, in my view, have not been as tight as they could be.

RAO: I think that there is not excessive concern about methodology, but there probably is excessive concern to shield psi from other modalities. The idea is to have adequate, sophisticated, sustainable methods that would give you a larger effect when psi functions in unison with other abilities. Nobody is talking about loose conditions. Nobody is talking about drawing conclusions that do not follow from the data. Good methodology is collecting data and interpreting them for what they are.

PALMER: I want to make a couple of clarifications about my answers to Dr. Adamenko's question. First of all I entirely agree with what Chuck Honorton said about the need for replicability for the purpose of having stable findings that we can build upon. If it had occurred to me, that would have been one of the points I would have made as well. Secondly, I want to go back and defend what I said about the importance of replicability in convincing mainstream scientists. I do not think it is appropriate to call this pandering. Science is a community activity and truth is defined by the consensus of that community. Science is also an integrated body of knowledge. Even though I have argued elsewhere that it does not need to be as integrated as some people think it does I still think much integration is required. So I think it is very important that we get the support of mainstream scientists and not try and go off on our own. There is also a very practical reason for this. We need the logistical support of the scientific community. For example, if we had the support of mainstream science we might be able to have more people teaching parapsychology courses in universities and when they are in universities, have them actually be accepted and not simply tolerated. There are all kinds of interpersonal interactions and resources that would be available to us if we had that support. So I think it is very important; it is not pandering.

WALKER: I think Adamenko is the only person who spoke today who made reference to J. B. Rhine's doing experiments to show psi is real and PK is real. There is such pandering in parapsychology that we are willing to almost chuck the whole episode of Rhine's work because of the critics. We want to appeal to these critics, so when they raise some question about Rhine's work we say, "Well, we are doing better experiments now." Chuck in his article with Hyman [*Journal of Parapsychology, 50,* 1986, pp. 351–364] almost shoved a lighted match up his rear end with his comment that we do not have any experiments that are fool-proof or adequate. He essentially handed Hyman just what he wanted on a platter. Our moderator here made the statement this morning that we have no theories. This is stated in order to appear to our critics to be as incredulous about the facts of parapsychology as they are. This is pandering to them.

HONORTON: I think that the main thing that we have to focus on in the future is convincing ourselves. If we convince ourselves then we will not have any difficulty convincing other people.

WALKER: That is my point.

EDGE: And I suppose I can say very briefly that I *never* stated this morning that we had no theories in psi. I am not going to give you a chance to respond, either!

# ANALYZING FREE-RESPONSE DATA: A PROGRESS REPORT

JESSICA M. UTTS

*Introduction*

Free-response experiments are often preferable to forced-choice experiments, partly because they allow for the possibility of observing striking correspondences between the target and the response. Unfortunately, the statistical methods used to evaluate these experiments are not generally sensitive enough to allow full credit for such correspondences. These analysis methods are primarily adapted from methods used for forced-choice experiments. Thus, the degree of correspondence between the target and the response is often reduced to a very conservative approximation of true correspondence. One aim of this paper is to show that the same analysis ideas can be used in a new way to allow more credit for such correspondence.

Another problem with free-response experiments is that their complexity often leads to incorrect application of statistical methods (e.g., see Kennedy, 1979). This paper reviews some common analysis methods from the perspective of the assumptions necessary for their use. Also reviewed are sources of randomness that allow these assumptions to be met.

The paper is divided into two major sections. The first section shows how some common approaches to analyzing free-response data can be categorized according to the source of randomness built into the experiment or the analysis. The basic requirements given in the first section must be followed in order to apply these methods or extensions of them. That section also describes some pitfalls that must be avoided.

The second section presents some advances in the analysis of free-response (remote viewing) experiments at SRI International. These advances allow for more refined estimates of the degree of correspondence between the target and the response. As shown in this paper, these methods fit into the context of the sources of randomness discussed in the first section and can thus be viewed as extensions of the existing analysis techniques.

*Sources of Randomness*

It is well known that responses in psi experiments, both forced-choice and free-response, cannot be considered to be random in any sense. Yet, all statistical analyses are based on the assumption that the experiment or analysis contains some source of randomness. By categorizing methods for analyzing free-response experiments according to where the randomness enters the procedure, we may avoid incorrect analyses such as those discussed by Kennedy (1979). Focusing on the source of randomness may also help in the design of free-response experiments.

Past discussions of free-response analyses methods have distinguished between holistic and atomistic approaches (Burdick & Kelly, 1977, p. 110). In holistic approaches, a judge assigns rankings or ratings to responses matched with their corresponding targets and other potential targets. Atomistic approaches are those for which specific features are compared in the target and the response. As shown below, both types of analysis can be categorized by the nature of the underlying assumptions of randomness.

*Sum-of-ranks method.* A common procedure for analyzing free-response experiments is to ask a judge to assign a rank to each target/response pair, and then use the sum of ranks across trials as a summary measure. Stuart (1942), Morris (1972), and Solfvin, Kelly, and Burdick (1978) all discuss this method. Solfvin et al. list the assumptions needed for the application of this method as: "a) there is only one judge per trial; b) all targets are equally likely to be selected; and c) successive trials can properly be treated as independent" (p. 94).

For completeness and future reference, we present the formulas used to find significance levels for this approach. Let $R$ = number of ranks possible for each trial, $n$ = number of trials, and $M$ = sum of ranks. Then the exact significance level for a given sum of ranks is

$$P = \frac{1}{R^n} \sum_{j=n}^{M} \sum_{k=0}^{\frac{j-n}{R}} (-1)^k \binom{n}{k} \binom{j - kR - 1}{n - 1}.$$

For large $n$, the sum is approximately normal with $\mu_M = n(R + 1)/2$ and $\sigma_M^2 = n(R^2 - 1)/12$. Thus, a z-score can be formed as $z = (M - \mu_M \pm .5)/\sigma_M$, and the significance level can be found in the normal table.

The assumptions (a through c) listed above are sufficient, but not always necessary to ensure that these formulas are valid. The key to understanding when they apply is understanding the basic assumption built into the computation of the formulas (under the null hypothesis):

> ASSUMPTION 1. *The summary statistic is a sum of integers. Each number in the sum is an integer from 1 to* R. *The* $R^n$ *possible sets of integers that could make up the terms of the sum are all equally likely.*

In the situation where this technique is usually applied, the ranks are assigned on each trial by presenting a judge with the response, and with the correct target embedded with $R$-1 decoys. The key source of randomness in this application is that the target and each decoy must have been equally likely to have been the actual target at the start of the experiment. Furthermore, the rank assigned on any given trial must not influence or be influenced by the rank assigned on any other trial. It was this latter condition that was violated in some experiments discussed by Kennedy (1979); in those experiments the judges ranked each target against each response. Thus, assigning a target a rank of one for a particular response might have precluded that same target from being assigned a rank of one for another response.

Notice that it is crucial that the target and decoys were all equally likely to be chosen as the target at the beginning of the experiment, since the target selection was the only random source in the experiment. Thus, for example, if a target was selected without randomizing being involved and decoys were selected later, even if they were the same type as the target, the approach was invalid. In fact, since no randomization is involved in such an experiment, no statistical technique can be recommended without flaw.

We now describe another use for these formulas, which seems to have been unrecognized. R. G. Jahn, Dunne, & E. G. Jahn (1980) describe an atomistic approach to remote viewing analysis that uses a 30-bit descriptor list. They outline several normalization and scoring methods that can be used to access the quality of a particular remote viewing. The final analysis, however, is done by converting the quality measure to a rank. This rank is determined by computing the quality measure for a given response as matched against each possible target. If there are $R$ possible targets, then $R$ quality measures are computed for the given response. The rank assigned for the trial is simply the number of targets in the pool that match the response as well as or better than the actual target used in the trial. In other words, the target/response pair is assigned a rank, but it is assigned by a formula instead of by a human judge.

Jahn et al. then use what they call "the common $z$ method for a discrete distribution" (p. 223) to obtain significance levels. But this is simply the normal approximation given above for the sum-of-ranks method. (They apply this to the average rank instead of the sum of ranks.) Their summary statistic is exactly equivalent to a sum-of-ranks statistic. Given certain assumptions about how the experiment is conducted, the above formulas are valid. Thus, for example, in their Table 12 (p. 227) they use the normal approximation even though there are only five trials. The exact formula (1) given above could be used instead.

The crucial feature of the sum-of-ranks method, and thus the method used by Jahn et al., is that assumption 1 must hold. Under the null hypothesis, each rank must be equally likely to be any integer from 1 to $R$, independent of the ranks assigned during other trials. These conditions would hold for the following experiment, analyzed using an atomistic approach. A pool of $N$ targets is selected and coded according to the bit list. A series of $n$ trials is conducted by choosing targets from the pool with replacement. Each response is coded according to the bit list. Ranks are assigned by choosing a quality measure, computing it for the response compared to each of the $N$ targets, and then counting how many targets match as well as or better than the actual target. (If ties are present, a slight modification is necessary. This is common if the quality measure can only assume a few values.) The sum of the ranks is computed, and the significance level is evaluated using formula (1) or the normal approximation.

This method is not valid if the targets are chosen without replacement, because assumption 1 no longer holds. To see this, suppose an experiment is conducted with only two targets, $T_1$ and $T_2$, in the pool and two trials. As an extreme case, suppose both responses yield the exact same bit-list configuration. Further, suppose this particular response configuration matches $T_1$ better than $T_2$, so that when $T_1$ is the correct target, a rank of 1 is assigned; and when $T_2$ is the correct target, a rank of 2 is assigned. If the targets are sampled without replacement, the only possible sets of ranks are (1,2) or (2,1). If sampled with replacement, all $R^n = 2^2 = 4$ possible sets are equally likely, and thus assumption 1 is met. Notice that the only source of randomness in the experiment is in the target selection; it is that source that allows or disallows the use of assumption 1.

*Forced one-to-one matching.* Forced-matching procedures for free-response experiments are discussed by Burdick and Kelly (1977), who attribute the first computation of the exact probability distribution to Chapman (1934). Scott (1972) gives tables that can be used to find significance levels; formulas for exact probabilities are given by Feller (1986, pp. 107–108).

The procedure is essentially equivalent to comparing two closed decks and counting the number of matches. For example, suppose $N$ free-response trials are conducted and a judge is given the $N$ targets and $N$ responses, and told to match them one-to-one. The statistic of interest is the number of correct matches. There are $N!$ possible configurations of matches. The assumption used to calculate the formulas and tables mentioned above is

> ASSUMPTION 2. *The summary statistic is the number of correct matches when matching* N *targets to* N *responses. Each of the* N! *possible configurations of matches is equally likely.*

This is generally the case (under the null hypothesis of no psi) if a closed set of $N$ targets is presented in random order, and then the targets and responses are sufficiently randomized before being presented to the judge. Hyman and others suggest that problems can arise if trial-by-trial feedback is given (see, for example, Druckman & Swets, 1988, p. 182). Under such conditions, a subject might avoid mentioning features that were prominent in previous targets. Those targets would then have a smaller than average chance of being matched with the new response, thus negating assumption 2. Notice that while the source of randomness in this kind of experiment is the random presentation order of the targets, the other details of the experiment must ensure that assumption 2 holds under the null hypothesis of no psi.

Forced matching can also be employed in experiments using an atomistic, bit-list approach. A set of $N$ targets can be coded according to the bit list, and then presented in random order over $N$ trials. The quality measure derived by comparing the bit lists for targets and responses can be computed for all $N^2$ target/response pairings. Matching can then be done by finding the one-to-one pairing that maximizes the sum of the quality measure over the $N$ pairs. The summary statistic is the number of correct matches in that pairing. This is essentially the method Scott (1972, pp. 86–87) recommends for evaluating verbal statements from mediums, except that the $N^2$ quality measures in that case are based on the $N$ subjects' assessments of the accuracy of the statements in the $N$ readings provided by the medium.

An interesting feature of the matching method is that the probability of exact $x$ matches, and, thus, the significance level (significance level = P[x or more matches]), is about the same for any $N$ of at least 10. Further, these probabilities are quite accurately approximated by the Poisson distribution with mean (and thus variance) of 1 (Feller, 1968, p. 108). The appropriate formula is

$$P(\text{exactly } x \text{ matches}) \approx e^{-1}/x! = .367879/x!\,,$$

regardless of $N$. Using this formula, P(4 or more matches) = .019, and P(3 or more matches) = .080. Thus, regardless of the number of trials, four or more matches lead to a significant result, while three or fewer do not!

*Unforced matching.* Many experiments criticized by Kennedy (1979) had the order of target presentation as the only source of randomness. Instead of being asked to do a one-to-one matching, however, judges were instructed to rank each response against all targets. The summary statistic and significance level were then based on either the sum-of-ranks method described above, or the number of hits as compared with a binomial distribution. Both methods assume trial-by-trial independence, a feature not present in these experiments.

The most conservative approach for reanalyzing these experiments correctly, and the one adopted by Kennedy, is to assume forced matching was used and then evaluate the significance level for the number of first-place matches. How conservative is this approach? It depends on the behavior actually adopted by the judge. The following discussion compares the two extremes when the summary measure is the number of direct hits.

Assume $N$ targets are compared to $N$ responses, and there are $M$ first-place matches. At one extreme, assume forced matching was used. At the other extreme, assume ranks were assigned independently for each trial. Table 1 shows a comparison of results for these methods.

Notice that the $p$-values for the two methods get closer as $N$ gets larger. It is an established fact that for large $N$ and small $p$, the binomial distribution is well approximated by the Poisson distribution with mean $Np$ (see Feller, 1968, p. 153). Thus, for large $N$ the two methods are essentially equivalent.

*Permutation methods.* Permutation methods were apparently first ap-

TABLE 1

Comparison of Methods for Evaluating First-Place Matches

| | Forced Matching | Independence |
|---|---|---|
| Mean, $\mu_M$ | 1 | 1 |
| Variance, $\sigma^2_M$ | 1 | $(N-1)/N$ |
| Distribution of $M$ | Approx. Poisson | Binomial, $p = 1/N$ |
| $p$-value, $N = 4$, $M = 4$ | .042 | .004 |
| $p$-value, $N = 10$, $M = 4$ | .019 | .013 |
| $p$-value, $N = 20$, $M = 4$ | .019 | .016 |

plied to free response data when Pratt and Birge (1948) recognized that Greville's (1944) forced-choice formulas were applicable to the assessment of verbal material from mediums. They restricted discussion, however, to methods using a normal approximation. Scott (1972, p. 87) seems to have been the first to recognize how to do an exact test.

Consider an experiment with $n$ trials, so there are $n$ targets and $n$ responses. Suppose that judging is done by creating an $n \times n$ matrix of scores comparing each target with each response. These scores could be based on, for example, ranking the $n$ targets for each response, using an atomistic quality measure for each target versus each response, or having a judge assign ratings to the degree of correspondence.

To apply the permutation method, arrange the matrix so that the scores for the correct matches are on the diagonal. The total score for the correct match is the sum of the diagonal elements (the trace) of the matrix. The summary statistic for the experiment is the proportion of all possible matches that have a total score as good as or better than the total score for the correct match. In other words, if the columns of the matrix are permuted in each of the $n!$ possible ways, and the trace of the matrix computed for each permutation, the summary statistic is the proportion of those traces that are as good as or better than the trace for the correct ordering. Note that in some cases, such as rankings, smaller traces are better; in other cases, such as ratings, larger traces are better.

To apply this procedure the order in which the targets are used must be randomized just as in the case of forced matching. The assumption under the null hypothesis can be summarized as follows:

> ASSUMPTION 3. *A series of* n *responses is given and compared to each of* n *targets to form an* $n \times n$ *matrix of scores. The summary measure is the proportion of permutations of targets for which the total sum of scores is as good as or better than for the correct ordering. At the start of the experiment* $n!$ *possible orders of use of targets were equally likely.*

Notice that this technique is not appropriate if the order of use of targets is not random, although it may be tempting to try to use it in such a case. Non-psi factors such as the day's headlines and weather can be too easily incorporated into both the choice of the target and the response.

### *Remote Viewing Methodology*

Humphrey, May, and Utts, (1988) discuss a methodology being developed at SRI International for the analysis of remote viewing experiments. In this section, we first summarize the methodology, then show how it can be applied under the different assumptions in the previous section. Finally, we show how this methodology can be used to pick decoys for free-response experiments.

*Quantitative definitions of targets and responses.* The main goal in the analysis of remote viewing data is to assess how well the responses match their intended targets. To make that assessment, three elements are needed: a definition of the target, a definition of the response, and a measure of comparison.

Recent experiments in remote viewing at SRI have used an established pool of 200 photographs from *National Geographic.* Responses have been limited to a few pages of drawings and words. The purpose of the present analysis has been to develop a method of quantifying the targets and responses that is refined enough to incorporate both concrete and abstract features and that is flexible enough to allow the definition to be changed according to the purpose of the experiment, the level of experience of the subjects, and so on. In an experiment with novice subjects, for example, the goal might be to see if they can identify major features; in an experiment with more experienced subjects the goal might be to measure identification of more specific features.

To accomplish these goals, a list of 130 features was developed. These were categorized into ten levels, ranging from specific structures (e.g., churches, forts) in level ten, to abstract one-dimensional geometry (e.g., parallel lines, spirals) in level one. The complete list is given by Humphrey et al. (1988).

The 200 targets in the pool were coded according to the visual importance of each of the 130 features on the list. For each feature a value between 0 and 1 was assigned, with 1 meaning that the feature virtually dominated the entire picture, and 0 meaning that the feature was absent. Thus, the quantitative definition of a target consisted of a list of 130 numbers, each between 0 and 1, describing the degree of visual importance of each feature on the list.

After an experiment was conducted, the responses were coded similarly, except that the number assigned to each figure represented by the analyst's degree of belief that the feature was present in the response. For example, if the response contained the word *river*, then the river feature was assigned a value of 1. On the other hand, if the response contained a drawing of parallel snaking lines without a label, the analyst might have assigned a value of .3 to the river feature.

To compare the targets with the responses, the values assigned to the features should have the same meaning in both. Thus, for this phase of the analysis, the target values were set to 1 for each feature for which the visual importance was rated at .2 or higher, since those features were definitely present in the target. The others were set to 0.

*Comparison of targets and responses.* May, Humphrey, and Mathews, (1985) describe a method of comparing targets and responses based on a *figure of merit* (FM). This measure is essentially a product of the proportion of the target material that was in the response (the *accuracy*) times the proportion of the target material that was correct (the *reliability*). The accuracy, reliability, and FM are easily adapted for comparing targets and responses as defined using the list of 130 features. The general versions of the formulas for the $j$th target/response pair are

$$\text{Accuracy}_j = a_j = \frac{\sum_k W_k(R_j \cap T_j)_k}{\sum_k W_k T_{j,k}},$$

$$\text{Reliability}_j = r_j = \frac{\sum_k W_k(R_j \cap T_j)_k}{\sum_k W_k R_{j,k}},$$

and $FM_j = a_j \times r_j$, where $R_{j,k}$ and $T_{j,k}$ are the values for feature $k$ in response $j$ and target $j$ respectively, and $(R_j \cap T_j)_k$ is the intersection between the target and response for feature $k$, defined in this application to be $\min(R_{j,k}, T_{j,k})$. The sums are taken over all 130 features in the list. In this version of the figure-of-merit definition, we allow for the possibility of adding weights $W_k$, in order to change the contribution of various features to the FM.

*Assessment of a single remote viewing.* The quality of a single remote viewing can be assessed by computing FMs for the response compared to each of the 200 possible targets. Assuming that the target was elected randomly from the set of 200, with each target equally likely to be chosen, the proportion of FMs as large as or larger than the one for the correct target can be thought of as a $p$-value. It represents the probability—under the null hypothesis of no psi—of obtaining a match as good as or better than the one obtained.

Note that the crucial source of randomness here is the equal probability of selection for each target. Certain targets, particularly those with more detail, produce higher FMs on the average than others. The quality of a remote viewing, therefore, cannot be assessed by the magnitude of the FM alone.

*Assessment of the entire experiment.* An entire experiment based on $n$ trials can be evaluated using one of the methods in the previous section, with the choice of method depending on how the experiment was conducted. Suppose a series of $n$ trials is conducted and the targets are selected with replacement. The sum-of-ranks method can be used by computing the rank of the FM for the correct target when embedded in the ordered list of all 200 possible FMs. Under the null hypothesis, and assuming no ties, this rank is equally likely to be any integer from 1 to 200. To see this, suppose the response is generated before the target is selected. The 200 FMs can then be computed and put into an ordered list. The corresponding ranks from 1 to 200 can be assigned. Randomly selecting a target is equivalent to randomly selecting one of those ranks, with equal likelihood for each one. The argument does not change if the target is selected before the response is generated.

After conducting $n$ such trials and finding the corresponding ranks, the significance of the sum of the ranks can be evaluated using equation (1), with $R = 200$, or the corresponding normal approximation. Note that the legitimacy of using the normal approximation is based on the magnitude of $n$, not $R$.

The other analysis methods discussed in the previous section can be used similarly. For example, if an experiment is conducted by selecting $N$ targets from the pool of 200 and presenting them in randomized order without replacement, then the forced-matching method described for atomistic bit-list approaches can be used. Or, a matrix of FMs can be created and used in the permutation methods.

One problem with these approaches is that statistical power may be low because the FM depends on the target complexity. More complex targets are more likely to be matched to responses because of this dependency. This does not affect the significance level, but it may give unnecessarily discouraging results. Work is underway to try to normalize the FM to avoid this problem. Meanwhile, the feature list and cluster analysis have been used to help choose decoys for human judging.

*Using the feature list to choose decoys.* In addition to the problems already mentioned with the feature-list approach, certain elements in both the targets and the responses are not contained in the list of 130 features. Furthermore, no mechanism in the FM approach gives credit to responses that look similar to the target in various ways but are possibly mislabeled. So far, the best approach for evaluating such matches seems to be to use a human judge, presented with $R$-1 decoys embedded in a set with the correct target.

One issue of concern with the judging approach is how to choose decoys that are dissimilar enough to not be confused with the correct target. For example, in the pool of *National Geographic* photographs there are several waterfalls, several snow-capped mountains, and so on. If decoys were selected from this pool randomly, there would be a relatively high probability that a decoy would look similar to the actual target. The following discussion presents a method of selecting decoys such that they are as dissimilar as possible.

The original assignments of visual importance of the 130 features can be used to compare targets and separate them into groups from which decoys can be selected. Similarity between targets can be assessed by computing an FM for the pair of targets. Using the same notation as in formula (2), we define the similarity ($S_{j,k}$) between targets $j$ and $k$ to be

$$S_{j,k} = \frac{\left(\sum_i W_i(T_j \cap T_k)_i\right)^2}{\sum_i W_i T_{j,i} \sum_i W_i T_{k,i}}.$$

Using these measures, we can create clusters of targets that are similar within clusters and different between clusters. For $N$ targets there are $N(N-1)/2$ unique values (19,900 for $N = 200$) of $S_{j,k}$. The values $j$ and $k$ that correspond to the largest value of $S_{j,k}$ represent the two targets that look most similar. Suppose another target, $m$, is chosen and $S_{m,j}$ and $S_{m,k}$ are computed. If both values are larger than $S_{m,n}$ (for all $n$ not equal to $j$ or $k$), then target $m$ is assessed to be most similar to the pair $j$, $k$. The process of grouping targets based on these similarities is called *cluster analysis*. See Johnson and Wichern (1982, Chapter 11) for a discussion of various clustering algorithms. We used hierarchial clustering with the complete linkage method, with the S-Plus statistical software package. Statistical packages such as BMDP and S also have clustering routines; BMDP has a version for PCs.

This procedure was followed to create clusters of the 200 targets in the *National Geographic* pool. Table 2 provides an overview of the 19 clusters found in the analysis. Some names appear to be quite similar, but, in fact, these sets are visually quite distinctive. Figure 1 shows the graphic output of a single cluster in detail. A much more complex—and visually difficult—graph is generated for the full cluster analysis and is not included here; this smaller subset has been chosen to illustrate the analysis. (To make the graphic analysis more meaningful, we did the analysis with $1 - S_{j,k}$.) All targets in this particular sample cluster are islands. Except for one outlier (i.e., a hexagonal building covering

TABLE 2

Names of the 19 Clusters

| No. | Name | No. | Name |
|---|---|---|---|
| 1 | Flat Towns | 11 | Cities w/Prominent Geometries |
| 2 | Waterfalls | 12 | Snowy Mountains |
| 3 | Mountain Towns | 13 | Valleys with Rivers |
| 4 | Cities with Prominent Structure | 14 | Meandering Rivers |
| 5 | Cities on Water | 15 | Alpine Scenes |
| 6 | Desert/Water Interfaces | 16 | Outposts in Snowy Mountains |
| 7 | Deserts | 17 | Islands |
| 8 | Dry Ruins | 18 | Verdant Ruins |
| 9 | Towns on Water | 19 | Agricultural Scenes |
| 10 | Outposts on Water | | |

an island), the islands fall into two main groups: with and without man-made elements. The natural islands include three similar mountain islands, two sandbars, and two flat verdant islands.

Once these clusters have been created, decoys can be selected such that the $R$ choices for judging, i.e. the target and the $R$-1 decoys, are each from separate clusters. This ensures that no decoy is too similar to the target or to another decoy. Since clusters have varying numbers of photographs, one should select $R$ clusters with equal probability, and then select a photograph within each cluster.

*Using cluster analysis to create target packets.* The concepts of target similarity and cluster analysis can also be used to create sets of targets

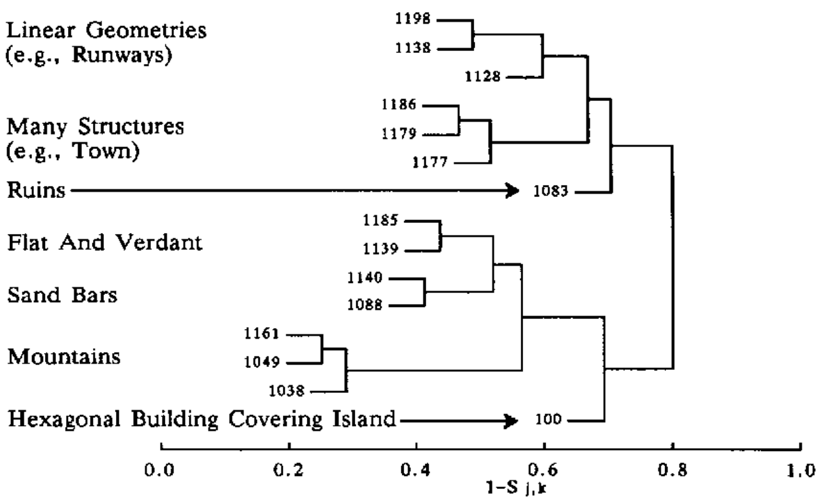


Figure 1. Detailed cluster analysis of the island cluster.

that are different within a set. Using this technique, we created 20 packets of 5 targets each from the *National Geographic* pool. To accomplish this, we used cluster analysis with *dissimilarity* between targets as the clustering criterion. Thus, the two most dissimilar targets were paired first, the next two most dissimilar next and so on, until a picture somewhat like Figure 1 emerged, but with all targets. Targets closest to each other were those most dissimilar. We used that information along with some visual shuffling to create packets of dissimilar targets.

These packets can be used as self-contained target/decoy units by randomly selecting a packet and then randomly selecting a target within the packet. Human judging can then be used by ranking the five targets in the packet against the response, repeating this for $n$ trials with replacement and using the sum-of-ranks method of analysis.

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## *DISCUSSION*

MORRIS: I have two kinds of comments. First of all I think it really is an interesting, innovative and exciting method. As far as the method itself is concerned, however, it sounds like an enormous amount of work and I wonder if you can comment on this aspect of it. What you have there, given all of the work that you put into it, is a very effective set of targets and sets of descriptors for them and there is now a lot that you can do with them. One question is: do you see this as something wherein there should be an attempt to have a standardized set of such targets that can then be used across labs? Do you see this as something wherein each lab that might define an idiosyncratic set of target materials really is going to have to do this all themselves right from scratch, including an enormous amount of playing around with the different levels of descriptors that they are going to try to deal with? If so, can you give some kind of feeling as to how easy this would be pragmatically for any lab to do given its own target interests? You have got geographical locations and sites and it seems to have worked really rather well with those. My second point is just simply more of a question. Do you see any restraints on the kinds of target pools that this might be used on just simply because it maybe harder to devise the different layers of meaning that you have been talking about?

UTTS: That is a good question. It certainly is a lot of work to put together a target pool and then to go ahead with the fuzzy set or with any bit list approach. Clearly it is a lot of effort. On the other hand, if a lab is in the situation where the experimenters know they are going to be in business for any length of time, which unfortunately is not always the case, then I think it is worth the effort. It is not valid to wait until the experiment is done and then fill out the bit list for the target, unfortunately. And so that is a problem. I think that you are right. It is a lot of work and it might be a useful idea to share target pools across labs. In fact that might add to the replicability issue. But you know it is like any endeavor; you have to decide how much the prep time is worth in the payoff at the end. I have to say frankly that I am not sure that the payoff for using the figure of merit approach was that high. I would say that there was strong payoff in using the method to choose decoys. When you have a huge target pool, such as 200 targets, it is hard to simply choose decoys and not get some repetition—you end up with a snowy mountain as the target and one of your decoys is also a snowy mountain and you are out of luck.

MORRIS: Can you share with us roughly how long it did take?

UTTS: I think that I will pass that on to Ed.

MAY: Well, it is really hard to say. We have been working out this problem for four or five years. I want to point out that it is really an iterative process. The pool started out at 400 and we did a clustering and then we noticed holes and some really junky targets that we could throw out. I would say with the target pool of 100 that we have now it would take maybe two years.

STANFORD: In terms of getting decoys that are "different" this seems great as a mechanical method. However, it seems to me that there may be a much more fundamental problem. This is derived from a set of categories that you developed from the bit list. But what does this really have to do with human cognition and perception? Similarly the way it works in the head is one thing and the way it works in terms of a system like this may be something altogether different. Do you have anything to say about that?

MAY: In fact, one of the real problems we had in putting this thing together was that Bev Humphrey, who was the primary mover on this for us, paid a great deal of attention to the particular bit list. Clearly the thing is sensitive as to what your bit list is. Under no circumstances would we recommend that this bit list be used in some other laboratory for two reasons. It was very highly tailored and I recommend that they all be tailored to match the target pool in question. Why have a purple giraffe in your bit list if there is not one in your targets. Also it was tailored to the general skill level of the subjects who were to be used in analyzing their data. That is a fair thing to do as long as you do it up front and a priori. So when you do that it is not such a large problem as you suggest. Also as to the fine-tuning of the bit, the target pool—which was frankly a surprise to me when we laid them all out on the floor—just looks visually different and that was our criteria, visual difference. And there was some minor fine-tuning on top of the technical part.

STANFORD: With regard to that, do you have ancillary evidence that that is true? Did you actually have people rating similarities so that you could show that?

MAY: Yes, we did. Certainly we did not do that over all 100, but we took samples of it and did it among ourselves around the laboratory. Our PA paper last year described in some detail how we gathered ground truth and did the comparison.

SCHOUTEN: I must say that I was very impressed by the paper. It is one of the things which has always interested me, because I think analyzing free-response data is really difficult. I have a couple of comments. One is that I think your approach to the bit list assumes that the psi information would cover all of the details. A bit list means that you split up the target into details. If I understand you correctly, the scoring you use is, in effect, a combination: the more response items are correct, the more target items are correct, the higher the psi score. It is my impression from the analyses of spontaneous cases we carried out, that actually what often happens is that the basic concept, the idea behind an event is what is transmitted and not the details. But I must say it is an excellent way to establish differences between targets. A second comment is that I am a bit surprised because there are already various scaling methods which are used to establish distances between items. You might save yourself a lot of work using one of these if you only want to establish dimensions and different sets. A third point is with regard to sensitivity. In Utrecht we have been using a method which would give us a somewhat more correct test for evaluating free-response data. I grant immediately that yours are much better than ours. But then we took the data of an actual experiment and wrote a program to simulate outcomes of experiments in such a way that we introduced different levels of psi. So we increased the probability that the outcome was influenced by psi and then applied our evaluation method. We found, to our great disappointment, that the nice method that we had developed was still less sensitive in demonstrating the psi we had introduced into the data than the simple binomial. Did you ever try a simulation like that to find out whether your method is indeed more sensitive than the binomial?

UTTS: First, we did indeed find that the method of having human judges do rankings was more sensitive than using the figure of merit with the bit list approach as the actual assessment method. That is why we went back to just choosing the decoys using the fuzzy set approach. We used sum of ranks instead of a binomial, but it is the same idea to use that. Secondly, about other methods for making paired comparisons, we looked at some other methods and none of them seemed to do as well as this method. In fact, for that reason we were thinking of writing this method up and putting it out into literature in other fields where they are trying to solve the same problem. And finally, your first point was that just because you have done well by comparing things for this particular bit list does not mean that you have more psi. In response to that, you do have to tailor your list of things according to your definition of what you are looking for as evidence of psi. Now at SRI the decision was made that it should be visual correspondence. So this bit list is specifically designed to find visual correspondence.

HONORTON: In doing a meta-analysis of the ganzfeld work it was absolutely impossible to code anything concerning targets and composition of target pools. Just as it is theoretically possible, that a lot of the experimenter effect is due to different subject populations, it is quite possible that a lot of the variability in free-response studies is due to differences in free-response target pools. There is also the degree to which different investigators are successful in creating interesting and yet relatively orthogonal target pools. The situation is, I think, analogous to what would happen in psychology if, in Rosenthal's person perception test of experimenter expectancy effects, every investigator used a different set of photographs without describing anything about their characteristics or if clinicians using projective techniques such as the Thematic Apperception Test or the Rorschach Test each created his own ink blots or ambiguous figures. That is bound to greatly increase the variability. Certainly some degree of standardization is really very important.

PALMER: It seems to me that the more holistic methods, such as the matching procedures, and the more atomistic methods such as the one that you have developed are tapping very different things. Have you looked at the correlation between the results with those two methods? If the correlation is high, what would you think about possibly combining the two outcomes to get something that takes advantage of what is going on with both procedures?

UTTS: That is a good question. I would say the correlation is not that high because they are tapping different parts of what is going on. In fact, we have been looking at methods of trying to combine them and have not yet come up with one that we feel is satisfactory. But that is what we actually are ultimately trying to do.

MAY: One of the problems with this kind of atomistic approach and the holistic methods and rank order procedure is that if the response is of bad quality, but good enough that a judge can just squeak it into a first place match that is one circumstance. The second circumstance would be if you have a fantastically high correspondence, an agreement with the first place, and the judge had no trouble making a first place match, the statistic does not differentiate between that really great hit and that just squeaking-by hit. I feel that it is not fair to a good response, so we are looking at a way of merging the two procedures to take advantage of that.

STANFORD: When you talk about standardization as Chuck was—and I agree with those remarks—before we standardize let us be sure we have all the elements together. For instance, if I were myself going out after targets, I don't know whether I would get into some of these 19 that are listed. My own experience suggests—and this is a very clinical type of thing—that you can't frame scientifically, but that there are some kinds of targets that might be a lot better than this. Some investigators, Chuck in his lab and some others historically, have been looking at what types of material make better targets. So when we standardize, if our aim is really high psi yield and not some specific kind of target that we are interested in or something of that sort, it seems to me that we really do need a lot more research on what types of targets individuals are likely to be sensitive to.

UTTS: I absolutely agree with you.

HONORTON: I also have been thinking a lot lately that what we maybe ought to do, at least in our experiments with novices, with people who have not done these free-response procedures before, is to have a single target pool that is used consistently across all of the screening or first-timers' sessions. One of the real problems in doing any kind of process-oriented research with free-response methods is that, to the extent that there are target effects and given the amount of time it takes to do an individual's free-response session, you can very well mask either good subjects or some correlate of subject performance, and because of the luck of the draw you get a psi-missing target. But if you have a standardized single pool that is used consistently for at least the initial stage, then all subjects are being assessed on an equal footing.

CARPENTER: I am new myself at free-response work, so my comment may be a bit naive, but I am wondering about limitation of the "bit" approach to analysis. What we have been doing is using group psychotherapy sessions as the mode of ESP response and then relating that to one of four pictures taken from magazines. The relationship between those two things reminds me of the dream work in that the relationships are very allegorical and metaphorical. For example, a session might have a certain mood and there might be no literal reference to anything that happened in the session in any element in the picture, but there is something about the mood that members of the group take as alluding to a kind of similarity. Now it seems to me that a bit approach would not have any way of catching that. I am wondering if those of you who have been doing this feel that anything significant is lost with the bit approach, the more metaphorical kind of relationship.

UTTS: I would say that you need to construct the bit list if you can to somehow incorporate those elements that you think are likely to arise that show evidence of psi. In the SRI case we were mainly focusing on visual correspondence. I do not know if there is a way to capture that sort of thing in a bit list, but that is something that you would want to think about. Then you need an experienced judge who can pick those out of the response.

MAY: I am not so sure that standardization of targets is the great way to go, other than as Chuck just suggested, for the very first level novice activity. What is important with this particular procedure, at least from our point of view, is that one can tailor it to match whatever one is looking for. If you are really interested, Jim, in the allegorical nature of what you are doing, you can design a bit list that focuses upon that and down-plays the visual or maybe the literal interpretation, so you can explore that. In fact, the method is powerful enough to allow you to put different weighing factors in so you can explore specific imagery. The neat thing about this procedure, at least from my point of view, is that it is infinitely flexible and each group can tailor it to their own specific needs.

HONORTON: In fact, with a computer you get rid of visual targets altogether and tailor your bit descriptive list to the particular subject population, the kind of problem that you are working with. You can give the agent whatever the bit categories are that are selected for the session and let him or her create out of a playroom full of materials some representation of that. That is another possibility that would greatly increase the freedom of expression while still providing an objective scoring structure.

# SUMMARIZING RESEARCH FINDINGS: META-ANALYTIC METHODS AND THEIR USE IN PARAPSYCHOLOGY

Charles Honorton

What is the value of a scientific research literature? We know how to evaluate the outcomes of individual studies, but what can we conclude regarding an entire research domain?

This question is important from a variety of perspectives. Policy-makers, funding agencies and research administrators all need the most complete information possible in order to make informed decisions regarding allocation of limited resources. For the scientific community, informed evaluation of new knowledge claims requires integration of all the available evidence. Investigators pursuing basic research, as well as those seeking reliable applications, need to realistically assess previous findings, suggestions regarding particularly successful approaches, the degree to which specific research practices may lead to unreliable outcomes, and so on. The caveat "Further research is necessary," is nearly always true, but what can we learn from all of the research that already exists?

Meta-analysis applies the methods of data analysis to the assessment of findings across all available studies in a given research domain. Regardless of the specific intent of any meta-analysis, there are two general activities common to all meta-analytic investigations, *cumulation* and *blocking*. Cumulation addresses the question "Is there an effect and, if so, how strong is it?" It involves assessing the statistical significance and magnitude of the effect under study, and the extent to which the cumulative effect is vulnerable to the selective reporting of "significant" results. Blocking subdivides the research domain on the basis of differences across studies that might account for their variability. By coding variations in procedures, subject populations, stimulus conditions, etc., the meta-analyst can address a variety of important questions such as: "Is the effect systematically related to study quality?" "How robust is the effect?" "Can we identify variations in procedures, subject populations, stimulus conditions, etc., that are particularly successful, associated with especially strong or reliable outcomes, or which have been consistently unproductive?"

Meta-analytic procedures are described in a number of excellent volumes (e.g., Cooper, 1984; Glass, McGaw & Smith, 1981; Hunter, Schmidt, & Jackson, 1982; Light & Pillemer, 1984; Rosenthal, 1984). Investigators wishing to embark on meta-analytic investigations should consult these sources, especially the Rosenthal and Cooper texts. Light and Pellemer provide a particularly readable discussion of meta-analysis that will appeal especially to those interested in the value of research integration for policy-making.

### *Meta-analysis of Experimental Precognition*

A meta-analysis of forced-choice precognition experiments will serve to illustrate some of the major characteristics of meta-analytic investigations. This meta-analysis was performed by Diane Bailey, George Hansen, and myself at PRL, under a subcontract from SRI International. In the limited time available for this presentation, my discussion of this study will be limited to an overview of its essential features. As such, it represents somewhat of an oversimplification, and I will avoid discussion of some of the complexities inherent in any study of this type. A more detailed report is in preparation for publication elsewhere. My purpose at this time is simply to concretize the meta-analytic process for those of you who are not already familiar with it and to convey, through this example, some appreciation of its power.

We addressed four major questions through our meta-analysis of the precognition literature:

1. Is there overall evidence for accurate target identification (i.e., above chance scoring) in experimental precognition studies?
2. What is the magnitude of the overall (directional and predicted) precognition effect?
3. Is the observed precognition effect related to variations in methodological quality that could pose serious threats to validity?
4. Does precognition performance vary systematically with potential moderating variables, such as differences in subject populations, stimulus conditions, experimental setting, knowledge of results, and temporal distance?

### *Delineating the Domain*

*Source of Studies and Criteria for Inclusion.* The source of studies was restricted to forced-choice precognition studies published in the peerreviewed English-language parapsychology journals: *Journal of Parapsychology Journal (and Proceedings) of the SPR, Journal of the ASPR, European Journal of Parapsychology* (including the *Research Letter* of the Utrecht University Parapsychology Laboratory) and *Research in Parapsychology*. We restricted our review to studies in which significance levels and effect sizes based on direct hitting could be calculated. Reports using outcome variables other than direct hitting, such as run-score variance, displacement, etc., were included only if they provided relevant direct hits information (i.e., number of trials, hits, and probability of a hit). We also excluded studies by two investigators, S. G. Soal and Walter J. Levy, whose work has proven to be unreliable. Many published reports contained more than one experiment or experimental unit. Experiments involving multiple conditions were treated as separate study units.

*General Characteristics of the Domain.* We located 309 studies in 113 separate publications. These studies were contributed by 62 different senior authors and were published over a 52-year period, between 1935 and 1987. Considering the half-century time-span over which the precognition studies have been conducted, it is not surprising that the studies are quite diverse. The data base comprises nearly 2 million individual trials and more than 50,000 subjects. Study sample sizes range from 25 to 297,060 trials with a median of 1194 trials. The number of subjects ranges from 1 to 29,706 with a median of 16 subjects. The precognition domain encompasses a diverse range of subject populations. Student populations comprise the largest grouping (approximately 40%), while studies with the experimenter as subject and animal studies comprise the smallest groupings (each representing about 5% of the studies).

*Outcome Measures.* Significance Levels: We calculated two significance estimates for each study. The *directional z-score* (Zdir) measures the subjects' success in scoring in the direction of their intention.

Effect Sizes: Significance levels are a function of sample size and comparisons based on raw significance levels can be very misleading. Consider a hypothetical example. Investigator A reports a ganzfeld study with 100 trials and a hit-probability of .25. She obtains 33 hits, a conventionally-significant result ($z = 1.7$, $p = .045$). An attempted replication by investigator B yields 11 hits in 33 trials. Since B's result is not significant ($z = 0.91$, $p = .18$), he concludes that he has failed to replicate A's results. B's conclusion is incorrect; his scoring rate (33% hits) is identical to A's. Even the significance levels of the two studies are not that different: ($z = 1.70.91\sqrt{2} = 0.56$, $p = .288$).

Thus, it is useful to have a basis for comparing study outcomes that is independent of the study sample sizes. Most parapsychological experiments, particularly those in the older literature, use the trial rather than the subject as the sampling unit. It is necessary to use a trial-based effect size estimator in such cases. In the precognition meta-analysis, for example, we use an effect size for each study that is the $z$-score divided by the square root of the number of trials in the study.

*Overall Cumulation.* As shown in the top part of Table 1, the overall results are highly significant. There is strong evidence for overall directional hitting. Thirty percent of the studies show overall significant hitting at the 5% level.

Lower bound confidence estimates of the mean $z$-score displayed in the bottom portion of Table 1 indicate that the mean $z$-score is well above zero at the 95% confidence level.

As indicated earlier, significance levels are related to sample size and it is therefore not surprising that $z$'s correlate positively with sample size. The correlation ($r$) is 0.156 $z$'s (307 $df$, $p$ =.003).

The effect size analysis is presented in Table 2. The directional outcome is significantly above zero.

*Replicability across Investigators.* Virtually the same picture emerges when the cumulation is by investigator rather than study. The combined $z$ is 12.31. Twenty-three investigators (37%) had directional outcomes significant at the 5% level. The mean (investigator) effect size is .028 ± .091.

These results indicate a substantial level of cross-investigator replicability and directly contradict the claim of critics such as Akers (1988) that successful parapsychological results are achieved by only a small handful of investigators.

*The Filedrawer Problem.* There is a well-known reporting bias throughout the behavioral sciences, favoring publication of "significant" studies (e.g., Sterling, 1959). The exteme view of this "filedrawer problem," as Robert Rosenthal describes it, "is that the journals are

TABLE 1

Precognition Significance Levels

| | $Z$ |
|---|---|
| Mean | 0.65 |
| Standard Deviation | 2.68 |
| Combined (Stouffer) $z$ | 11.41 |
| $p$, | $6.3 \times 10^{-25}$ |
| Filedrawer Estimate | 14,268 |
| Lower 95% Confidence Estimate of Mean | .40 |

TABLE 2

Precognition Effect Sizes

| | ES |
|---|---|
| Mean | .020 |
| Standard Deviation | .100 |
| $t(308)$ | 3.51 |
| $p_t$ | .00025 |
| Lower 95% Confidence Limit | .011 |

filled with the 5% of the studies that show type I errors, while the filedrawers back at the lab are filled with the 95% of the studies that show nonsignificance . . ." (Rosenthal, 1984, p. 108). Recognizing the importance of this problem, the Parapsychological Association in 1975 adopted an official policy against selective reporting of "positive" results. Even the most cursory examination of the parapsychological literature will show that nonsignificant results are frequently published and in the precognition database, 60% to 70% of the studies reported nonsignificant results. Nevertheless, 75% of the precognition studies were published prior to 1975, when the Parapsychological Association formulated its policy, and it is necessary to ask to what extent selective publication bias could account for the cumulative effects we observe.

The central section of Table 1 uses Rosenthal's (1984) filedrawer statistic to estimate the number of unreported studies with $z$-scores averaging zero that would be necessary to reduce the known database to nonsignificance. The filedrawer estimate suggests that there would need to be over 46 unreported studies for each reported study in order to reduce the cumulative hitting (directional) outcomes to a nonsignificant level.

Based on this analysis, we conclude that it is implausible that the cumulative significance of the precognition studies is due to selective reporting.

*Study Quality.* While precognition experiments are not usually vulnerable to sensory leakage problems, there are a number of other potential threats to validity that must be taken into account. Statistical and methodological variables are defined and coded in terms of procedural descriptions (or their absence) in the research reports. One point is given (or withheld) for each of the following criteria:

Specification of Sample Size. Did the investigator preplan the number of trials to be included in the study or was the study vulnerable to the possibility of optional stopping? Credit was given to reports which explicitly specified the sample size. Studies involving group testing, in which it was not feasible to precisely specify the sample size, were also given credit. No credit was given to studies in which the sample size was either not preplanned or not addressed in the experimental report.

Preplanned Analysis. Was the method of statistical analysis, including the outcome (dependent variable) measure, preplanned? Credit was given to studies explicitly specifying the form of analysis (and the outcome measure). No credit was given to those not explicitly stating the form of the analysis or those in which the analysis was clearly post-hoc.

Randomization Method. Credit was given for use of random number tables, random number generators, or mechanical shufflers, but not for hand shuffling, die casting, or drawing lots.

Controls. Credit was given to studies reporting randomness control checks, such as RNG control series and empirical cross-check controls.

Recording. One point was allotted for use of automated recording of targets and responses and another for duplicate recording.

Checking. One point was allotted for use of automated checking of matches between target and response and another for duplicate checking of hits.

Each study received a quality weight between zero and eight. We found no overall relationship between study quality and effect size ($r307 = -.062$, $p = .279$). Of the eight quality measures, controls and duplicate recording correlated signficantly positively with effect size and randomization correlated significantly negatively. Eighty percent of the studies (N = 247) used adequate methods of randomization and the Stouffer $z$'s for these studies alone remain highly significant ($z = 5.49$).

It has long been believed by critics of parapsychology that psi disappears as methodological rigor increases. The precognition database provides no support whatsoever for this belief. Precognition effect sizes have remained relatively constant over a half-century of research, even though the methodological quality of the research has improved significantly. The correlation between effect size and date of publication is $-.064$. Study quality and date of publication are, however, positively and significantly correlated ($r307 = .266$, $p < 10^{-5}$).

*Moderating Variables.*[2] The stability of precognition study outcomes over a 50-year period is also of course bad news. It indicates that we have not yet developed sufficient understanding of the conditions un-

[2] Throughout this report, $t$-test comparisons involving unequal variances are computed using the separate within groups variance for the error (Wilkinson, 1988) and degrees of freedom following Brownlee (1965).

derlying the occurrence (or detection) of these effects to reliably increase their magnitude. Can meta-analysis help? I believe the answer is "Yes." Our precognition meta-analysis has identified a number of variables that appear to covary systematically with magnitude of precognitive performance. I will briefly discuss three:

1. Selected versus unselected subjects;
2. Individual versus group testing;
3. Feedback Level.

*Selected vs. Unselected Subjects.* Precognition studies using subjects selected on the basis of prior performance show larger effects than studies with unselected subjects. Two-thirds of the studies with selected subjects are significant at the 5% level. Indeed, the *mean* directional *z*-score for these studies is 2.37 ($sd$ = 3.42). The basis of selecting subjects was the subject's performance in a previous experiment or in pilot tests. As shown in Table 3, the magnitude of effect size is significantly higher for selected subjects studies than for studies with unselected subjects. The *t* test of the difference in mean effect size is equivalent to a point-biserial correlation of .48.

Is this difference due to less stringent controls in studies with selected subjects? The answer appears to be "No." The average quality of studies with selected subjects is in fact significantly higher than studies using unselected subjects ($t52 = 2.57$, $p = .013$).

*Individual versus Group Testing.* Studies in which subjects are tested individually by an experimenter have a significantly larger mean effect size than studies involving group testing (Table 4). The *t* test of the difference is equivalent to a point-biserial correlation of .234, favoring individual testing. Forty-one percent of the studies with subjects tested individually are significant at the 5% level. The methodological quality of studies with subjects tested individually is significantly higher than in studies involving group testing ($t201 = 3.57$, $p = .00022$).

*Feedback.* There is a significant positive relationship between the degree of feedback subjects receive concerning their performance and precognitive effect size (Table 5).

TABLE 3

Selected vs. Unselected Subjects

| Subjects | N Studies | Mean ES | SD |
|---|---|---|---|
| Selected | 44 | 0.096 | 0.147 |
| Unselected | 265 | 0.010 | 0.082 |
| $t_{47} = 3.76$, $p = .00023$ | | | |

TABLE 4

Individual Versus Group Testing

| Test Setting | N Studies | Mean ES | SD |
|---|---|---|---|
| Individual | 134 | 0.045 | 0.111 |
| Group | 123 | −0.001 | 0.077 |
| $t_{255} = 3.85, p = .000075$ | | | |

Subject feedback information is available for 139 of the studies. These studies fall into four feedback categories: No feedback, delayed feedback (usually via notification by mail), run-score feedback, and trial-by-trial feedback. For analysis purposes, these categories were given numerical values between 0 and 3. Directional precognition effect size correlates .228 with feedback level (137 *df*, $p = .0035$). Of the 67 studies involving trial-by-trial feedback, 49% were significant at the 5% level, while only 1 of the 18 studies with no subject feedback (5.6%) was significant. Degree of feedback correlates positively, though not significantly, with research quality ($r137 = .124$, $p = .145$).

*Precognitive Time Span.* We have examined a number of other factors that vary across studies including differences among subject populations, target variations. etc. Space (and time) limitations require that a full presentation of our findings in this area be postponed for another occasion. However, there is one other question that I will discuss now, because of its intrinsic interest, and because it illustrates an important limitation of meta-analysis.

What, if anything, can we say regarding the temporal range of precognitive functioning? We attempted to address this question through analysis of the 190 studies which provide information concerning the interval between the subject's response and the determination of the target. Since the information provided was usually not very precise, our analysis was limited to seven broad temporal categories: milliseconds, seconds, minutes, hours, days, weeks, months.

TABLE 5

Subject Feedback of Results

| Feedback Level | N Studies | Stouffer $z$ | Mean ES | $SD_{ES}$ | SIG.5% |
|---|---|---|---|---|---|
| No Feedback | 18 | −1.41 | −0.002 | 0.044 | 5.6% |
| Delayed | 24 | 1.83 | 0.010 | 0.038 | 25.0% |
| Run score | 30 | 13.25 | 0.039 | 0.084 | 46.7% |
| Trial-by-trial | 67 | 12.87 | 0.063 | 0.136 | 49.3% |

The reported intervals range from a few milliseconds to one year and we did find a marginally significant decline in precognitive effect size across these seven temporal intervals ($r188 = -.131$, $p = .036$). To complicate matters, the relationship interacts with the selected/unselected subjects difference. The negative relationship between performance and temporal distance, to the extent that it exists at all, appears to be restricted to studies involving unselected subjects. These studies show a much larger negative relationship than the database as a whole ($r149 = -.211$, $p = .009$), and studies with selected subjects show a nonsignificant *positive* correlation between performance and time interval ($r37 = 0.84$). The difference between these correlations approaches significance ($z = 1.92$, $p = .054$).

Unfortunately these findings cannot be taken very seriously because the precognitive interval is systematically related to both study quality and degree of feedback. Studies using automated testing methods, for example, generally received higher quality ratings and were much more likely to be associated with trial-by-trial feedback than studies involving longer precognitive time spans.

Such confounds are inevitable in meta-analytic investigations. Meta-analysis provides an important and valuable method of summarizing an existing research literature, but it is not a substitute for new experiments.

*What the Data Tell Us.* Returning to the five basic questions we asked at the beginning of this exercise, what has the meta-analysis told us about experimental precognition effects?

*Is there overall evidence for accurate target identification (i. e., above chance scoring) in experimental precognition studies?* Yes. The cumulative results cannot reasonably be attributed to chance fluctuation. Independently significant outcomes are shown in 30% of the studies and by 39% of the investigators and these outcomes cannot plausibly be attributed to selective reporting of positive results.

*What is the magnitude of the overall precognition effect?* The effects are small. While we knew that before the meta-analysis, we now have some indication of their actual magnitude. Our confidence estimates indicate, for example, that the average magnitude of significance level is at least one-third of a standard deviation.

*Is the observed precognition effect related to variations in methodological quality that could pose serious threats to validity?* There is no overall relationship between study quality and effect size. Of the individual quality criteria, two correlate significantly positively with outcome and only one (method of randomization) correlates significantly negatively with outcome. Even if we discard studies with nonoptimal randomization, the results remain highly significant.

*Does precognition performance vary systematically with potential moderating variables?* Yes. We have identified three correlates of precognitive performance. Subjects selected on the basis of prior achievement show significantly larger effect sizes than unselected subjects, individually tested subjects perform at significantly higher levels than those tested in group settings, and precognitive achievement covaries with the degree of feedback provided to the subjects.

Should these meta-analytic findings be regarded as conclusive? Probably not, but they provide a much richer and better-informed foundation upon which to base future research than we had before. And that, in my opinion, is the value of meta-analysis.

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## *DISCUSSION*

RAO: Chuck, I think you have a very interesting paper here and we feel very reassured that the research effort of over half a century has produced some solid robust effect attesting the reality of precognition. I would like to make a couple of comments. First of all, it is very interesting to see that in spite of the variation in test techniques and different subject groups, experimenters and laboratories, we seem to have a consistent effect. It is reassuring indeed that the past effort is greatly reinforced by the fact that much improved methodology and greater control seen in current research have essentially given the same results as the original studies. The second point is with regard to the blocking analysis that you have done. It is possible that there may be some kind of a confounding here. Now the selected subjects did better than the unselected subjects. The individual testing was better than group testing. It is all very likely that the selected subjects were tested individually.

HONORTON: That is right.

RAO: And the unselected subjects were tested in groups so this could be a confounding variable. I can think of a number of others.

HONORTON: There are and I mentioned some in the written version of the paper.

RAO: There are other variables which could also account for some of these differences.

HONORTON: However, Ram, if you look at it in terms of a multiple regression analysis it is clear that the strongest of those three predictors is the selected subjects feedback.

RAO: It is possible that the differences are genuine and you may be right about the selected subjects feedback. But I have a lingering worry that a consideration of effect size without regard to the sample size may mislead us sometimes. Higher effect sizes obtained with smaller samples may have other explanations than the size of the sample itself, such as experimenter expectations, his effort and involvement. In any case I think we need further analysis and evidence to say one way or the other.

HONORTON: That is not true in the precognition database. In fact if you are talking about effect sizes rather than significance levels there should not be a significant relationship between effect size and sample size. In fact I do not know of any evidence at all to support . . .

RAO: That, I think, is really the paradox that I am talking about.

HONORTON: But what I am saying is that I do not think there is any empirical support for what you are saying.

RAO: Well, I think that is something we should look into seriously. I do not know if you have any data to show that it is not the case.

HONORTON: The data from this meta-analysis shows that there is no relationship between effect size and sample size. There is a significant relationship between significance level and effect size and that is what you would expect. In fact, if you look at the number of trials in a study in relation simply to whether the study was significant or not, there are almost three times as many trials in the significant studies as in the non-significant studies.

RAO: Again these are all confounded because of unselected subjects working under no feedback conditions and vice versa. With selected subjects and individual testing it is more likely that the feedback is given right away. In group tests when you test 200 subjects in a classroom it is less likely the subjects are given immediate feedback. Again it is probable there are more subjects in group studies than in studies involving select subjects.

HONORTON: That is really irrelevant to the issue of relationship between effect size and sample size.

RAO: I do not think so. Let me explain. As you recognize, the significance of a result is a function of both effect size and the sample size. Hence both of them are important. For example, if you have an effect size of .1 with data on 200 subjects the result may be significant. But with just 10 subjects the same effect size may not be statistically significant. Therefore, the consistency of the effect size in these two samples does not lead us to have the same confidence about the genuineness of the effect in both the cases. So what I am saying then is that the reason why we would like to have probability estimates is to see whether this effect is not due to some kind of variability. Therefore, sticking only to effect size ignoring the sample size altogether is likely to leave room for making some errors. So I would suggest that we do not simply look at the effect size and throw away and disregard other factors.

HONORTON: Who has thrown away what? Who has thrown away something?

RAO: We have to keep in perspective the probability values as well. We just cannot speak about effect size alone.

HONORTON: Yes, but we can assess the probability values of the effect sizes. In other words, when you look at an effect size you are talking about the magnitude of the effect. RAO: Right.

HONORTON: Taking out, stripping out the effect of the sample size.

RAO: The magnitude of the effect is meaningful only when we have confidence in the reality and genuineness of the effect.

PALMER: I was going to make a somewhat similar point. Let me make it slightly differently.

HONORTON: I want to respond to another aspect of what Ram was saying first, if I may. Quite obviously, when you do a meta-analysis you are not doing an experiment. You take the data as they exist and you learn as much from them as you can. Obviously, there are confounding factors in any meta-analysis that can only be resolved through further new research. The whole purpose of the meta-analysis is to provide a more informed estimate of what the effect size is and what the conditions are that are most likely to be productive for your study.

PALMER: I have a bit of a problem about comparing effect sizes with different sample sizes, and it has to do with the stability of the effect size. As an example, say you are doing an ordinary card test with a run of 25 trials. Someone gets 10 hits. That is equivalent to an effect size of 10. That happens from time to time in the lab and does not get anybody too excited. However, if you were to maintain an effect size of 7 over, say, several thousand trials, as in some of the old card guessing experiments, by any standard of evidentiality you would have something of much greater consequence, even though the effect size is smaller. I think a solution to that—and I really have to credit Jessica for this—is to report confidence intervals around the effect sizes. This would give the kind of information that would keep one from being misled about the stability of an effect size. In fact, I would almost go so far as to suggest it as a reporting requirement in our journals. We have recently had a reporting requirement that authors need to give means and standard deviations, which in a sense is the same thing. But I think confidence intervals may be even a better way of conveying the essential information, along with the effect size. You certainly cannot get a good characterization of the data simply by reporting $p$ values.

My second point has to do with the sources of studies for meta-analyses of this type. From time to time I am amazed when I see reports of parapsychology experiments appearing in, particularly, psychology journals, places such as *Psychological Reports*. I don't think there are a large number of these, but, as you might expect, they are consistently negative, so they probably have a different mean as compared to the rest of the sample from the parapsychology journals. Particularly when you are dealing with something like precognition, which is a very broad and widely used procedure, it might be good to go into *Psychological Abstracts* and get some sources from there as well. The good news here, at least based on the studies in the psychology journals that I have seen, is that if you do a blocking in terms of the source of the study in relation to study quality, you can come up with results that I think would be rather flattering to us.

SCHOUTEN: What really amazed me was the results of the file drawer approach, where you find that on the average, you would need about 140 insignificant results for each study in your database which was significant to wipe out the overall significant effect of the meta-analysis. It might be unclear how I arrive at the number of 140. The explanation is: Chuck reports in his paper that ±14,000 non-significant studies would be needed to wipe out the significant overall results of his meta-analysis. His meta-analysis is based on the outcomes of over 300 actual experiments of which, however, only less than 100 were significant at the 5% level. So on the average it looks as if 14,000/100 = 140 nonsignificant studies are needed for each significant one to make the meta-analysis non significant. I still do not understand that.

HONORTON: Averaging?

SCHOUTEN: Averaging. Now that is an amazingly high number. How can that be? I just don't understand how that is possible. My second question is just curiosity. When you report some results from these analyses, for instance, selected versus unselected subjects, what sort of importance do I have to attach to that? Would you rate it as equal as, for instance, if you had run an experimental study and had found a significant difference to that effect? Or do you consider it to be merely suggestive? I have no idea.

HONORTON: I consider it somewhere in between. I think it is very strongly suggestive, but certainly not conclusive. It is not conclusive because, as Ram pointed out, there are other variables that might interact and confound with it. We found that there is also a tendency for subjects who were tested individually to do better than those who were tested in groups. Selected subjects are usually tested individually. To give you another example which is in my paper, we were of course very interested to see whether there was any declining precognitive effect size over time, over intervals ranging from a few milliseconds to a year. And there is in fact a significant negative correlation between effect size and precognitive interval. However, this is also very strongly related both to feedback and to quality. The quality relationship, which is very strong, is opposite to what it would have to be in order to be a problem. That is that the strong results are in the shorter time intervals. Those studies also have the highest quality, but as for feedback there is simply nothing that we can really do about that. We have evidence that relatively immediate feedback is associated with much stronger results than delayed or no feedback. If somebody is doing precognition over a year, he is not getting feedback for a very long time. So I consider it to be basically an exploratory technique in terms of the process-oriented aspect. In terms of being able to say that there is overall evidence that something non-chance is going on, I would say that that is as close to conclusive as we can get. The filedrawer estimates and the overall significance levels simply are not compatible unless your basic starting point is that the likelihood of precognition is less than one in a million or something like that, which with precognition may not be that uncommon.

BROUGHTON: I have the feeling that all too often we do not accumulate our knowledge over generations of experimenters. I hope we are indeed learning, as your moderating variables analyses show, that there are patterns, and there are regularities which have direct practical consequences for the way we design experiments. I hope these do start influencing the next generation of experimenters. I would like to take this opportunity to ask you what should the rest of us be doing to make this job easier? I mean as people who are going to be doing experiments and reporting them, what sort of things do you advise?

HONORTON: Let me say first that I agree with you. I have never liked the term parapsychology particularly, but by golly given the degree to which it has been unfairly attacked in recent years I wear the badge with great pleasure and honor. And another thing is that when you do these meta-analyses you begin to realize that we really are a community. There are half a dozen of you in this room whose research has contributed to the database. Now, the major source of frustration for anyone who does a meta-analysis in parapsychology is that, in spite of the fact that the results do not seem to correlate with methodological threats to validity in any of the meta-analyses that have been done so far, I am embarrassed by much of the literature in terms of the lack of sophistication in the way we report our findings. We call ourselves parapsychologists, but a Martian looking at our literature would think that the human aspect of this is very minimal—subjects 15 males and 14 females, college age, volunteers—the description of subjects alone is pathetic. Looking at the very vital, philosophically tremendously important issue of the reach of precognition over time, the fact that even in the more recent literature involving automated testing techniques we have not started to adopt some fairly precise way of talking about the interval between the subject's response and the selection of the target is embarrassing. So I think that the journals and the people who have been involved in meta-analyses should get together and have a conference sometime and try to come up with some generalized guidelines for reporting in different areas. When you do a meta-analysis in a particular area you find where a lot of the missing elements are. Now Ray Hyman and I in our joint communique I think did that for the ganzfeld domain. As far as I am concerned that is up to the editors of the journals now to make sure that the ganzfeld papers that are submitted include the level of descriptions that are dealt with there, for example. I would hope that Dean Radin and Roger Nelson would offer similar descriptions for the RNG area. I might add that the problem is not simply in the so-called proof-oriented research. Some of the most flagrant examples of inadequate reporting are in the more process-oriented research where the investigator has a pet theory or model which predominates with very little description of what the subjects were told to do, how were they recruited, these kinds of factors. I think we can do much better. I think one of the very totally uncontroversial claims that can be made about meta-analysis is that it will help us to improve our reporting of our research in such a way as to increase the ability of future replicators to actually replicate what we have done and not have to read between the lines and second guess us.

UTTS: I, too, would like to share in the congratulations to you for this work and for your other meta-analyses. This is actually a comment that I was going to make this morning after Rao's paper and we did not have time for me to do it. And that is that I think one of the things that has come out of this meta-analysis is the fact that you did not find a relationship between effect size and sample size. Is that right?

HONORTON: Yes.

UTTS: I think the belief has been around for a long time that there is that relationship. In fact, that is one of the things the critics have tried to push, that when you increase your sample size suddenly you decrease your effect size. In his meta-analysis paper in the *Journal of Parapsychology*, Ray Hyman gave a statistical argument that that was the case in the ganzfeld database. It turns out when I looked at his argument that it was a statistical fluke and that indeed he had not shown that relationship. So anyway I would just like to say that I hope that we will see meta-analysis shattering some other myths that we have had around for awhile.

HONORTON: I think that, as far as I know, this is the first meta-analysis that goes beyond overall accumulation and starts to look at moderating variables. That is where we are going to find the real limitations of meta-analysis in terms of not being able to parse out how the different variables might be confounded. But at least we know a lot more now about what to look for, we have a much more informed basis for future precognition studies now than we did before. I think that is asking enough of this relatively new approach to data integration.

MORRIS: First let me just add to the compliments. I think this is really good, especially getting at the process aspects of things. However, I would like to know how you really define what a precognition study is. As you know many of them have multiple interpretations.

HONORTON: A precognition study was a study in which the subjects' task was defined for them as being precognition, as predicting the future.

MORRIS: Did you find ambiguities where that was difficult to assess?

HONORTON: The very first experimental precognition study was by Carrington and it was a dice throwing study. A few years later it would have been called a PK study. We considered it a precognition study because that is how he conceptualized it.

MORRIS: I think that is important, too.

HONORTON: Now the other thing I should mention here—and I think, Bob, you might be particularly interested in this—that an analysis on this database that we have not done yet, but one of the things that we kept coded, is in the non-automated studies that used random number tables or various shuffling methods, who was the randomizer? And did he use one of these complex calculations to try to reduce the likelihood that it was a combination of contemporaneous psi effects on the part of the experimenter? I have not looked at that yet, but I suspect that that will be informative. If there is, there should not be significant variation across randomizers or randomizing conditions, if the randomizer is not implicated in the outcome. So I think we will, for the first time on a larger scale, at least be able to begin to address that issue as well.

MORRIS: I am glad to hear that you are doing that. You might also pull in some of the studies that might be interpreted within the IDS model as well to see whether or not that shakes loose anything.

# THE PROBLEM OF TIME AND PSI

VICTOR G. ADAMENKO

## *Introduction*

Psi phenomena have been denied in their totality at the end of the last century because they could not enter the fundamental laws of physics of that time, which were based on mechanistic materialism. However, some remarkable scientists like the physicist Sir William Crookes, Lord Rayleigh, and J. J. Thomson, as well as the Russian chemist V. Butlerov have tried to apply their knowledge to the investigation of psi.

The theories of relativity and quantum mechanics have changed the way of thinking of physicists, but it is difficult to use a new dialectical understanding of the physical world, not only in psychical research, but even in exploring the "recognized" psychological processes in general. To unify the deep psychology of unconscious and conscious states, the physicist Niels Bohr has proposed to transfer some new physical approaches from physics to psychology. He considered the conscious states of the psyche together with the unconscious ones to be similar to a centaur: the corpuscular-wave's dualism of elementary particles, which has been recognized by contemporary physics (Bohr, 1955). Bohr's proposal is valuable for parapsychology too because Freud believed that telepathy is transmitted through the unconscious and conscious functions that disrupt it (Freud, 1921). Apparently there are some corroborations of Freud's idea about telepathy. It has been reported in the case of the Russian scientist M. Lomonosov, who lived in the XVIII century and described his dream about his dying father. He saw him on a solitary island of the North Sea. Lomonosov described his dream to his brother, who really found the corpse of their father on the island, according to the telepathic dream's information (Vasiliev, 1964).

The discovery of REM was very important for dream telepathy studies because it makes it possible to carry out experiments in this field today using more or less controlled conditions. (Ullman, 1966; Ullman & Krippner, 1970). It is interesting to notice also, that on of the main methodological standards in carrying out experiments on remote viewing is "no criticism" of the subject who describes the target (Puthoff & Targ, 1976). It is easy to see that the psychological condition "no criticism" is nearer to unconscious functions while the conscious and logic components of the psyche are always connected with criticism.

Since radio communication was discovered many attempts have been made to use classical electrodynamics to the find the "material carrier" of telepathy. The most promising hypothesis was the one about transmission of telepathic information by means of superlong radiowaves (Kogan, 1966; Kogan, 1968). But ESP information was transmitted by remote viewing from a submarine to the coast and back while it was submerged under 200 meters of sea water (Puthoff & Targ, 1981). So agents and percipients were satisfactorily shielded from electromagnetic waves and the electromagnetic hypothesis of telepathy failed.

As for attempts to apply physical theories not widely known and not established to psychical research, it has been proposed to consider electromagnetic solitons (stable, secluded waves produced in strong electromagnetic fields) as "material carriers" of telepathic information (Fedorenko & Tchugaevsky, 1980). These "telepathic" solitons, or so-called intellectons, have not been discovered yet. Nevertheless there is a generalized soliton hypothesis. According to this life is connected with a soliton-like architecture and if solitons relate to the inanimate world, "living" solitons or lifons relate to the animate one (Chakravarty, 1987). No experiments to confirm or to refute this interesting soliton hypothesis are available yet.

Certainly it is possible that classical electrodynamics for 3-dimensional space misses some phenomena which may be connected to psi. Theoretically multidimensional electrodynamics is able to explain most of the psi phenomena (Egely, 1987), but to carry out some multi-dimensional space experiments is a "little" difficult. That is why to somewhat explain this hypothesis only methods of analogies known by psychic explorers as far back as the beginning of the century can be used.

The theory of physical vacuum can be applied to research in parapsychology also. It is known that the so-called physical vacuum is not really a void. There is a theory in modern physics that all elementary particles are constructed by quarks. Because it is impossible to observe quarks (considered as fundamental "small bricks" of matter) the term "vacuum" is used for the field of quarks and so physical vacuum is a quark field in an unexcited state. But if physical vacuum is supplied by some energy, real elementary particles spring up from it. It is known in physics that energy, which can be produced in physical processes, depends on the distances between the elementary particles. In physical vacuum these distances are much smaller than the ones in the atomic nucleus and so the energy of it must be stronger than those of the nucleus. This energy of physical vacuum (hidden in an unimaginable small distance) may be used even for interstellar journeys (Adamenko, Laptchinsky, & Malinov, 1976). Probably ESP information may be transmitted through a physical vacuum by insignificant excitation of it and some PK phenomena may be explained in this way. But no experiments have been carried out to check this hypothesis. In addition established science today bewares of generalizations of the theory of physical vacuum, because if it is possible to use "the universal vacuum" 's a universal system of coordinates, as did the concept of ether at the end of the last century the principle of relativity would be broken. But the special theory of relativity is based on two postulates fulfilled simultaneously:

1. material objects cannot move at a velocity higher than the velocity of light in vacuum;
2. the principle of relativity is universal.

If only one of the two postulates is proved wrong, the special theory of relativity falls.

As for the exploration of the deepest structures of matter, some physicists, for instance, the Vice President of the Academy of Sciences of USSR, A. Logunov believes that ". . . to solve the fundamental problems of the structure of matter it will be necessary to reevaluate the contemporary knowledge of space and time. Maybe a few physical processes in the microworld, which seem improbable now, really exist. Even today astrophysics demonstrates to scientists some physical processes, like radiation of quazars, which are improbable from the energetic viewpoint" (*Izvestiya*, 301, 1976).

Several well known, open-minded physicists in the USSR are ready to recognize that ESP and PK are real phenomena and it may be possible to find some explanation of them from the viewpoint of contemporary science. As for the phenomenon of precognition, they believe that, for the time being, it is absolutely impossible to find any valid explanation. The above mentioned established and not established physical theories really are unable to explain these phenomena. Meanwhile some experiments in precognition were carried out under well controlled conditions (Krippner, Ullman, & Honorton, 1971; Puthoff & Targ, 1976). Rao and Palmer mentioned it in their survey article about parapsychology and the criticism of it. One of the main "trump cards" of the opponents is the lack of replicability of parapsychological experiments (Rao & Palmer, 1988). Other critical arguments can be considered trivial. But no one of the opponents has said that precognition is the main obstacle to the acceptance of psi by contemporary physics. Replicability of the "behavior" of elementary particles is likewise impossible in principle, so nuclear scientists work using only probabilities.

To explain some of their precognitive experiments, Puthoff and Targ have tried to apply the so-called advanced potentials of classical electrodynamics (Puthoff & Targ, 1976). But it seems that they have undermined their own explanation by their submarine remote viewing experiments. The quantum mechanical interpretation of psi, including precognition, is valuable enough today (Schmidt, 1974), but quantum mechanics formally describes only random processes without analyzing the properties of time. Meanwhile, it is clear, in my opinion, that the phenomenon of precognition must be connected with some properties of time itself.

### *The Basic Ideas and Experiments of Kozyrev about Properties of Time*

The Russian astrophysicist Nikolai Kozyrev has elaborated a theory of time that is not established yet. Nevertheless it is in accordance with the conventional contemporary criteria required for a theory to be recognized as a scientific one, i.e., some predictions of the theory must be confirmed by experiment.

What are the basic theoretical premises of Kozyrev? There is a fundamental law of physics, the Second Law of Thermodynamics established by the mechanistic-materialistic approach to science in the last century. According to this law all physical processes in nature have a tendency to go from less probable states toward the more probable ones, i.e., all kind of energies transform into heat which dissipates over the universe (in a statistical sense all processes go from order to disorder) and in the future there must be a universal equilibrium of temperatures or a so-called thermal death of the world. Degradation of the chemical elements (radioactive processes) and tendency to equilibrium of electrical potentials takes place, too. This degradation of energy and matter formulated also as increasing entropy, it is possible to say increasing disorder, is an irreversible process in our world.

According to Kozyrev "in the universe, however, there are no signs of the degradation which is described in the Second Law. Stars die and are born again. The universe sparkles with inexhaustible variety, in it one finds no traces of an upcoming thermal and radioactive death. Apparently here is where the basic contradiction lies—a deep contradiction which may not be explained away through a reference to non-applicability of the Second Law to the infinity of the universe. The fact is that not only separate stellar bodies, but whole systems are isolated from each other to such a degree that they may be regarded as closed systems, for all practical purposes (usually the Second Law is applied only to closed systems). For them the thermal death could visibly draw nearer before any aid could come from outside. Such systems, in a state of degradation, should prevail in the universe, and yet they are almost non-existent" (Kozyrev, 1958).

The second enigma of contemporary astrophysics is the origin of the stellar energy. The traditional theory of inner structure of stars affirms that they produce energy by means of thermonuclear processes. But these processes must be accompanied by the radiation of neutrinos. But the sun does not radiate neutrinos according to the experimental data of astrophysicists. Consequently the origin of the sun and stellar energy cannot be explained by only thermonuclear processes. So what is the cause of the origin of stellar energy?

To solve both problems Kozyrev assumed that there are some constant forces in nature which hinder the spreading of entropy and at the same time supply the stars with energy. But the fact is that a change in the Second Law of Thermodynamics is hardly possible,while keeping intact the First Law (the First Law of Thermodynamics is the general law of conservation of energy). Therefore, one may suppose, that having solved the problem of the origin of stellar energy, it will be possible to discover the key to the most important phenomena of the stellar world.

It was Kozyrev's conviction that the basic characteristics of matter, space and time, must be principles of mechanics. That is why to explain the origin of stellar energy, he thoroughly analyzed its fundamental laws. Kozyrev concluded that what constitutes the incompleteness of these laws is the inability of mechanics to express the basic characteristics of causality, which consist of the distinction between cause and effect in principle.

Really, how is it possible to distinguish the difference between cause and effect objectively? He assumed that in the complicated cause-effect chain there must exist an elementary link and he postulated the basic principles of "causal mechanics":

1. Implying the impenetrability of matter (two bodies cannot occupy the same place at the same time) he claimed that cause and effect, which are associated with separate bodies, must be separated by a certain minimum space. It is only in atomic physics, where quantum considerations permit the overlapping of fields and no precise meaning can be given to the spatial separation of particles, that the distinction between cause and effect no longer applies. So $\delta x$ is the distance between two points representing cause and effect in the elements of this chain;

2. In Newton's mechanics there are no means to distinguish between past and future and time is reversible. But in causal mechanics the time interval between the two events, cause and effect, is non-zero, though infinitesimal. So $\delta t$ is one element of the chain sequence in the time-interval between two events representing cause and effect.

> The velocity of propagation of the cause-effect signal in the elementary link of a long chain of cause-effect transformations, is $C_2 = \delta x/\delta t$. For classical mechanics, $\delta t = 0$ and $C_2 = \infty$, while in quantum mechanics $\delta x = 0$ and $C_2 = 0$. In the macroscopic universe $C_2$ lies between these two extremes and has a finite value which can be calculated. Calculations based on the axioms of causal mechanics show that $C_2$ is less than the speed of light and so there exists no conflict with the theory of relativity. The chain of cause-effect sequences defines the unidirectional character of the time flow from past to future and it appears suitable to describe $C_2$ as the "course of time." It coincides with the direction of increasing entropy. This asymmetry or irreversibility of time is absent from classical mechanics . . . (Kozyrev, 1958, p. 172)

Kozyrev paid attention also to the probability that, if time is asymmetric, space must have the same property. Formerly it was held that it was impossible to differentiate between our universe and its mirror image, but the experiments of Lee and Yang establishing that parity is not necessarily always conserved, suggested that some basic differences may exist between left and right handedness. As time and space are closely interlinked it may be considered that spatial asymmetry is a consequence of time asymmetry and that, if time were to run backwards, one would find, for example, one's heart on the right side instead of the left, i.e., the universe would become its mirror image. Thus time flow is involved with spatial rotation. Astronomical data point to the fact that the above mentioned asymmetry is caused by the asymmetrical nature of time itself, i.e. it is caused by the objective distinction between the future and the past. It is this directional flow, its reversibility, which is characteristic of time and which establishes the differentiation between cause and effect (Kozyrev, 1958, 1963).

"The strangest and most unexpected conclusions arise from all of this, in relation to the creation of the stars. However, they do support the basic thesis that there exist constant forces in the universe which work towards preventing the achievement of a state of equilibrium . . . The conditions show that stars produce energy as the result of certain electrodynamic processes. But the principle according to which a closed system is capable of producing energy, must be sufficiently profound to include the simple laws of mechanics. Therefore the first task lies in formulating the following questions: how can a closed mechanical system produce energy and where will this surplus of energy come from? . . . If the common laws of mechanics are not symmetrical it becomes theoretically possible for a closed system to produce energy, according to causal mechanics. So we may say that this directional attribute of time is caused by certain astrophysical phenomena. Thus the stars only appear to possess perpetuum mobile: actually they obtain their energy from the flow of time" (Kozyrev, 1958, 1976).

The conclusion of causal mechanics that rotating bodies, including stars can produce energy by using some property of time, is an amusing one. However, what do we know about time? A physicist knows how to measure the duration of time, therefore for him time is an absolutely passive concept. Newtonian physics considered space and time as "empty" extension and "empty" continuance. It is proved by the theory of relativity that space has physical properties, i.e., it must be filled by physical fields. Conventionally, in this theory, time and space are even interchangeable. But physical properties of time never have been discussed by scientists. Kozyrev believed that time as well as space, has physical properties. What are those? He claimed that space is passive because it is only an "arena where the events take place." But, if time, beside the passive geometrical property of continuance measured by clocks, has some active properties, they must manifest as a force in material systems. So the existence of active or physical properties of time must lead to an interaction between time and the processes which take place in the world. Then time, as some physical medium can influence the substance of the course of processes and connect different phenomena that have no connection at first glance. Events not only go on in time, but time itself influences them through its "physical" components. One of the simplest properties of time may be the course of it. As mentioned above, this physical property of time when it influences the substance, can supply it with energy and sustains the life of stars as well as hinders thermal death of the universe (Kozyrev, 1968, 1976). The course of time is a universal constant which is connected with the evoking of forces directed along the axis of revolution in the rotating bodies. But there is another physical property of time which is of a variable quantity. It is called the "density of time." This physical "component" of time can be "absorbed" or "radiated" by substances and so density reflects the active property of it. The experiments carried out using special detectors, showed that near the systems in which entropy increases the density of time increases too. Consequently in this process the order, lost when entropy increases in one system, can be transmitted by changing the density of time, to the substance of the detector, increasing its order. So the elasticity, the conductivity, the work function of electrons (in photoelectronic processes) in the substances change. These phenomena were confirmed by experiments (Kozyrev & Nasonov, 1978). Also there are other properties of time such as the possibility of being reflected by mirrors and shielded by substances. A parabolic mirror of a telescope focuses not only the rays of light, but the physical component of time, too. That is why astronomical observations of the physical properties of time can be carried out only by telescope-reflector, not by telescope-refractor (Kozyrev & Nasonov 1978, 1980).

To check these theoretical conclusions about the physical properties of time Kozyrev carried out some experiments. For instance, he defined the value of the course of time ($C_2$ = 700 km/sec.) from his experiments with rotating gyros. He obtained the same value of $C_2$ from the analysis of the astronomical data. Also he made a successful prediction that a lunar volcanic eruption would take place. Before that the moon was considered by astrophysicists to be an absolutely "dead" satellite of the earth. But the most impressive of his experiments is the determination of trigonometrical parallaxes based on the measurement of a difference between the true and apparent star's positions. The results of this experiment were predicted by Kozyrev's theory. In the *Journal of Astrophysics of Armenia* he wrote, "Time does not propagate (for example, like electromagnetic waves) but appears at once all over the universe. That is why the connection through time must be an instantaneous one. So it is possible to observe some phenomena of very far astronomical bodies in real time, without delay. This perspective does not contradict the special theory of relativity because, when we have instantaneous connection through time, there are no movements of material objects" (Kozyrev, 1976).

What is the essence of this experiment, which was carried out approximately two years after its prediction? It is known that earth and many stars are separated by immense distances in the order of tens and hundreds of parsecs. The velocity of light $C_1$, is 300,000 km/sec. and so we can observe these stars only in their past positions, where they were a long time ago. However, if Kozyrev's theory is correct, it is possible to observe the true position of the stars in real time but not only in the past. There are technical possibilities for constructing special detectors, too. Because if entropy (and the density of time) changes in the star, this process must affect also the substance of the detector, changing its conductivity. The electrical scheme of one of the detectors for measuring the density of time is shown in Figure 1. The sensing element *6* of the detector is able to receive the "radiation" of time from stars and it is possible to measure quantitatively changes of the density of time by the readings of the galvanometer *4*.

The approximate scheme of the experimental device for the measurement of the true star positions is shown in Figure 2. The sensing element (resistor) of the detector for the measurement of the density of time was located in the back of the spectrometer at a distance of 0.5—lcm. The first observations of the true and apparent positions of the stars, by means of the device described in Figure 2, were carried out with the 50-inch telescope-reflector of the Crimean Astrophysical Observatory in October 1977. The data obtained i.e., the difference between the true and the apparent positions of the star were used to calculate its parallaxes. The other way around: it was done using known parallaxes to calculate the true positions of the stars theoretically. (Parallax of a star is the maximum angle subtended at the star by the mean radius of the earth's orbit around the sun.) Some results of the observations are given in Table 1, where the observed deviations for the planet Venus and true star positions are compared to the predetermined ones (Kozyrev & Nasonov, 1978). The data of Table 1 confirm the prediction of Kozyrev's theory about the possibility of observing the true positions of astronomical objects.

When these observations of stars were carried out, some interesting phenomena were discovered. A few stars "radiated" alternating density

TABLE 1

Experimental Data to Compare the True and Apparent Positions of Astronomical Bodies

| Astronomical Body | Π | $\Delta\alpha_1$ | $\Delta\alpha_2$ | Deflection of the Galvanometer's Needle: Apparent Star Point | Deflection of the Galvanometer's Needle: True Star Point | Date |
|---|---|---|---|---|---|---|
| Planet Venus | | +36″ | +37″ | 8 | 5 | 10/18/77 |
| Andromeda | 0.″031 | −41″ | −38″ | 5 | 6 | 10/21/77 |
| Cassiopeia | 0.″182 | +1″ | 0″ | 6 | 6 | 10/21/77 |
| Gemini | 0.″093 | −17″ | −20″ | 3 | 3 | 10/19/77 |

Twenty-seven observations/experiments were carried out at the Crimean Astrophysical Observatory in October 1977.

Π = Parallaxes.

$\Delta\alpha_1$ = Theoretically predetermined deviations of astronomical bodies using *The General Catalogue of Trigonometric Stellar Parallaxes* by L. Jenkins and the *Year Book of Tables of Special Astronomical Data.*

$\Delta\alpha_2$ = Observed deviations of astronomical bodies, experimental data.

of time when observed during a span of several days. Sometimes the density of time was so decreased, that it was impossible to receive its "radiation." Five stars, out of 23 observed, did not show this "radiation" noticeably because probably, the sensitivity of the detector was not enough. Also the star position in the past was discovered, i.e., its light was received by the detector. To exclude the influence of light upon the detector, the big mirror of the telescope was screened by a duralumin plate 2mm thick. Nevertheless the detector received the time's "radiation" from stars and its positions in the present moment and in the past.

These experiments were continued by Kozyrev and Nasonov at the same Crimean Astrophysical Observatory in 1978–1979 with improved results in the determination of the true and apparent star positions (Kozyrev & Nasonov, 1980). Simultaneously it was discovered that the electric conductivity of the resistor of the detector changed at three points in the sky:

1. position of the astronomical object at the present moment,
2. position of the astronomical object in the past,
3. position of the astronomical object in the future, which the object will have should it receive a signal from the earth with the velocity of light (see Figure 3a).

The last result of the experiments was unexpected because 14 astronomical objects were observed. To explain this phenomenon, Kozyrev supposed that the four-dimensional world of H. Minkowski really exists. As the conclusions of Newtonian physics about the independence of time and space are not correct, special geometry connecting time and space in a unified four-dimensional diversity, was elaborated by Minkowski. This geometry is completely in accordance with the consequences of the special (restricted) theory of relativity. Before, Minkowski's world was considered by the scientists as a mathematical abstraction. In this imaginary world, all the events which can take place in the future, exist already from the viewpoint of our world. They continue to exist in the past also and when we move along the axis of time, it is possible to meet them as well in the future.

In the theory of relativity instead of the intervals of space $dr$ and time $dt$ one uses the so called 4-dimensional interval $ds$:

$$ds^2 = c^2\, dt^2 - dr^2$$

where $c$ is the velocity of light.

There are four coordinates in the Minkowski geometry: x, y, z and ict (i is imaginary = $\sqrt{-1}$). Kozyrev believed that the connection through time can take place when interval ds = 0 (see Figure 3b). Changes of the physical properties of the interval are perceived exactly by the detectors of the density of time. As for "remote action" (instantaneous connection) Kozyrev claimed that the "special theory of relativity was elaborated on the base of physical considerations, but the strict grounds of this theory were given by Minkowski's geometry and so physical considerations may be incorrect" (Kozyrev, 1980).

According to the convictions of Kozyrev, there are astronomical proofs of the reality of Minkowski's world and that we live in this world. So, probably, it is possible to explain the phenomenon of precognition in the framework of Kozyrev's theory of time.

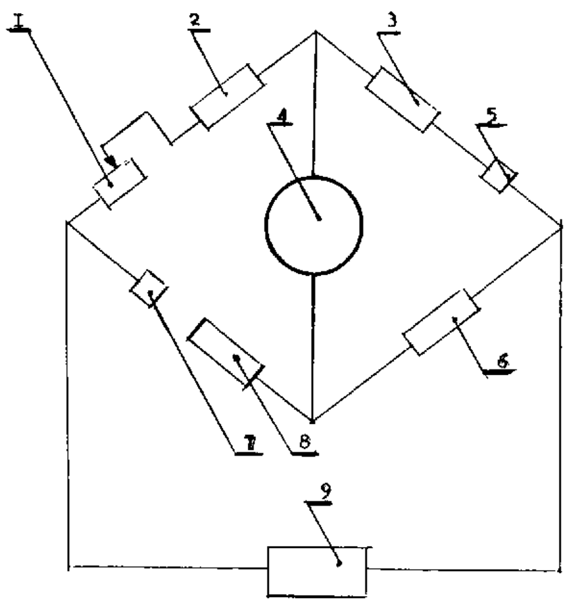


Figure 1. The electric scheme of a detector for the measurement of the density of time based on the Wheatstone bridge.

1. Variable resistor.
2, 3, 8. Constant resistor.
4. Galvanometer (M-95).
5, 7. Plates of aluminum (its resistance practically zero).
6. Constant resistor—sensing element.
9. Source of stabilized voltage 30V.

One division of galanometer is $TO^{-9}A$. The aluminum plates stabilize the work of the galvanometer when the measurement of the density of time takes place. All constant resistors have the positive temperature coefficient 1,5 $10^{-4}$, and its values are equal to the inner resistance of the galvanometer 5 kOm.

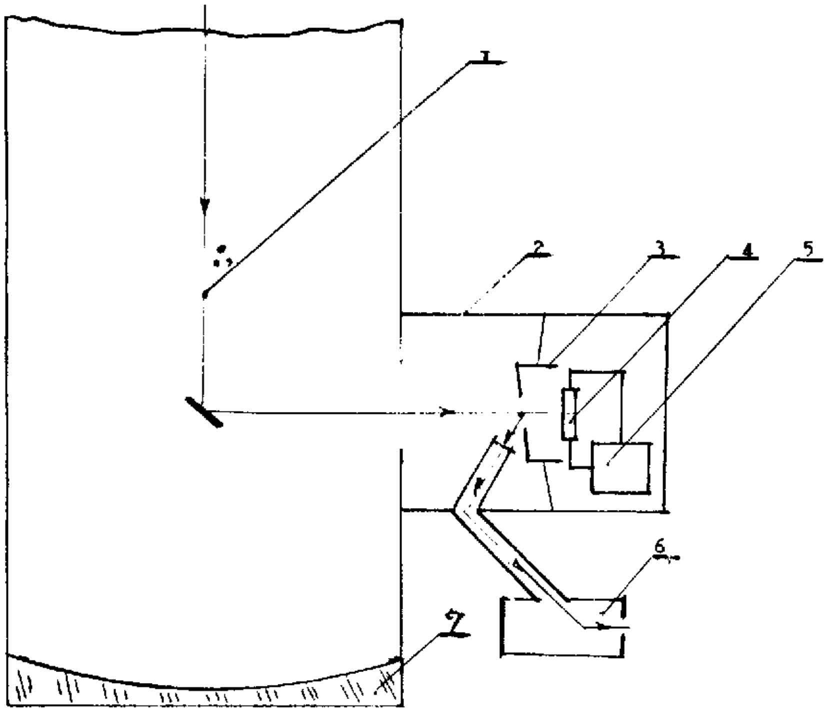


Figure 2. Approximate scheme of experimental device for the measurement of the true star position.

1. Direction of light from star.
2. Encasing of spectrometer.
3. Aperature of spectrometer.
4. Sensing element of the detector of density of time.
5. Describer in Figure 1 detector of density of time in place of the standard spectrometer.
6. Visual instrument for observation of stars.
7. Mirror of 50-inch telescope-reflector.

## *Under the Sign of Irreversibility?*

Certainly Kozyrev's theory of time looks paradoxical, but it was elaborated to explain some discrepancies between the generalized consequences of the Second Law of Thermodynamics and the real physical conditions in the universe. Is there any alternative?

In the last century Klausius raised the Second Law to the rank of universal law and it was he who introduced the conception of entropy. But even Lord Kelvin, who was one of the founders of the Second Law did not recognize entropy and used instead of it the concept of "capacity

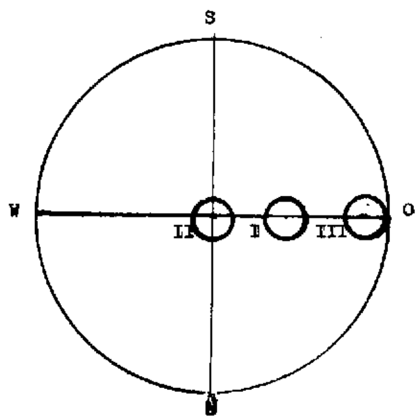


Figure 3A. Three positions of Nebula of Andromeda in the sky when it influences the detector of density of time.

I. Position at the present.
II. Position in the past.
III. Position in the future.

for work." Other famous scientists were not satisfied by the law of the inevitable increasing of entropy, too. For example, the Soviet physicist Sergei Vavilov spoke about the discrepancy between the Second Law and the real course of processes in nature, but the philosopher John

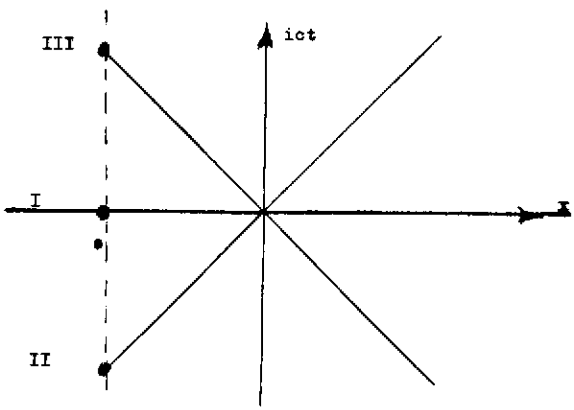


Figure 3B. Three cases of the possible connection through time in Minkowski's world.

I. At the present moment.
II. In the past.
III. In the future.

Bernal believed that the phenomenon of life itself is in contradiction to this law. The phenomena of consciousness and psi contradict the concept of increasing entropy.

The founder of Soviet Space Exploration, Konstantin Tsiolkovski, believed that the Second Law is incorrect and the circulation of energy takes place in nature like, for example, the circulation of water on the earth. This viewpoint means that in nature the processes of concentration of energy exist at the same level as the processes of its dissipation. So the Second Law is only one of the branches of the universal law of concentration-dissipation of energy in accordance with the natural processes of creation-destruction. The inventor of the first Soviet radar, Pavel Otchepkov, is developing the idea of concentration of energy to get new harmless sources of energy instead of nuclear ones (Otchepkov, 1967). One of the technical solutions of the problem is the use of thermal pumps, invented by Kelvin. Modern thermal pumps, including electronic ones, probably concentrate energy, but there are no calculations to check if they violate the Second Law or not (Adamenko, 1983).

The incessant generation of star groups was discovered by the Soviet astrophysicist, V. Ambartsumian. This fact is recognized by contemporary science and it is impossible to reduce the discovered concentration of matter-energy to the fluctuations in the universe. But if energy can be concentrated without the surplus energy, not only the Second Law, but even the First Law can be contested. There is no theoretical elaboration of the processes of dissipation-concentration of energy, as in Kozyrev's theory. However, if the mentioned natural concentration of energy really takes place, time must be reversible. So probably we have an alternative today: either to consider time reversible and not able to produce energy, or irreversible and able to produce energy, according to Kozyrev's ideas.

The controversies about the Second Law testify to insufficient understanding of the concept of energy which was introduced to science only in the beginning of the last century (before scientists used the concept of force). The above controversies have repercussions not only in astrophysics, where the origin of stellar energy is discussed, but in biology, too. One of the founders of quantum mechanics, Erwin Schrödinger, who believed that the Second Law is unshakeable, described the way living organisms avoid the transition to a state of thermodynamic equilibrium, i.e., death. It is known that metabolism is the complex of physical and chemical processes involved in the maintenance of life. But, translated from Greek, metabolism means exchange. The exchange of what? It is implied that an exchange of substance takes place. However, it looks like nonsense because any atom of nitrogen as well as oxygen is no worse than any other. What is the purpose of their exchange? Today it is believed that living organisms feed on energy and various kinds of foods have different energetic values. This is an absurdity, said Schrödinger. The organism of an adult contains a stable quantity of energy and matter and because any calorie is equal to any other, it is impossible to understand the purpose of an exchange between them. After such an introduction, Schrödinger puts a question: what is the precious "something" which is contained in food and protects the living organism against death? This is the only answer, he claims: in any point of the universe entropy increases and the living organism continuously produces positive entropy, too, and so everyone is drawn toward a state of maximum entropy, i.e., to death. To avoid this state and so to be alive, the living organism decreases his entropy, continuously extracting the negative entropy from the environment, including food. He concluded that the essence of metabolism is the removal of the entropy which is produced by the living organism (Schrödinger, 1972).

Kozyrev affirmed that the stars extract energy from the physical properties of time, hindering the spreading of entropy in the universe. Schrödinger was sure that living organisms hinder the increasing entropy in themselves, thus extracting negative entropy, i.e., order from the environment. It is possible to suppose that both are correspondent processes. Can the changes of entropy of living organisms be measured in the case of the stars? Up to now nobody was interested in this problem, from the viewpoint of connections between living processes and entropy. One of the methods of measuring the changes of entropy in the living organisms, in my opinion, is Kirlian photography, i.e., making pictures of different objects, including living ones, by means of high-frequency currents. This method has been used for diagnosis in medicine and psychophysiology (IKRA Communications, 1978, 1987). Some scientists were perplexed by the diagnostic multipurpose of Kirlian photography: the structure, the color, the brightness of Kirlian pictures changed similarly when different diseases were diagnosed (from diabetes to schizophrenia). Kirlian photography's researchers feel sure that a certain energy in living organisms is responsible for these changes in the pictures. However, in my experiments in this field I proved that these changes are connected with the processes of metabolism in the living organism. According to Schrödinger, the essence of metabolism is changes in entropy and Kirlian pictures reflect these changes. It is possible now to explain the diagnostic multipurpose of Kirlian photography: the devices for taking the Kirlian pictures are detectors of the changes in entropy and because these changes are similar in different diseases, the pictures are similar, too. In addition, it was proved that the Kirlian pictures are electron images. The electrons, "drawing" the Kirlian images, are due to autoelectronic emission (Adamenko, 1975). But the current of the autoelectronic emission depends on two variables: the strength of the electric field and the work function of electrons. So Kirlian devices work like Kozyrev's detectors of the density of time because "radiation" of time changes the work function of electrons as mentioned above. To study the work of healers objectively, high frequency photography was used by the Kirlians in 1969. Later this work was duplicated by scientists and amateurs in different countries. A hypothesis on the connection between entropy and the biological field, as well as healing, was proposed (Adamenko, 1973). Today it is possible to say that Kirlian pictures of healer's and patient's fingertips prove the exchange of entropy between them. Apparently the healers do not radiate any energy. To heal they must only take some entropy from the patient. Sometimes the connection between healer and patient looks like telepathic communication. From this viewpoint, some experimental work of the Italian doctor S. Guarino is very interesting. He has improved the results of his telepathic experiments changing the metabolism of the agent and of the percipient by manipulation of food intake (vitamins, amino acids, etc.). Guarino had no information about Kozyrev's theory, nevertheless he supposed that the transmission of telepathic information is connected to the Second Law of Thermodynamics and so introduced the new term "thermodynamic radiation." This radiation must be responsible for ESP phenomena. He did not work out any physical theory of "thermodynamic radiation" and had no detectors of changes in entropy, but his experimental data probably confirm the connection between telepathy and mutual changes of entropy in both agent and percipient (Guarino, 1975).

Kozyrev's first detectors of the course of time were the gyros. Their functioning was sometimes so unpredictable, that the opinion was expressed about the experimenter's interference with PK in Kozyrev's experiments (Tiller, 1972). The functioning of detectors of the density of time is no better than the one about gyros. These detectors "feel" even such indispositions of the experimenter as slight grippe. It was necessary to choose the suitable season to duplicate the experiments about the true positions of the stars. Kozyrev and Nasonov usually carried out this experiment in late autumn because the large number of plants and trees which bloom in the spring and partly die in the autumn, produce such a high hindrance to Kozyrev's detectors, that it was very difficult "to catch" the signals from the stars.

In the Institute of Psychology of the Academy of Sciences of Ukraine some verifying experiments were carried out, using the detector of the density of time for the measurement of psychophysiological conditions. It has been proved that the sensing element (resistor) of Kozyrev's detectors functions equally well when the endothermic reaction (absorbing heat) or the exothermic one (releasing heat) takes place. It means that the detector "feels" something else besides the heat. It was shown that Kozyrev's detectors can be used in psychology for the registration at a distance of transitions into states of deep relaxation or emotional excitement (Bahtiyarov, 1984).

Obviously Kozyrev's detectors "feel" the processes which are connected with the changes of entropy. It would not be so preposterous for established science, if it was not claimed that they can "feel" these changes at immense distances and instantaneously, as well as "foresee" star positions in the future. The last claim is the most amazing and it is like a deterministic view of nature. However, Kozyrev did not believe in the determinism of Laplace. According to his viewpoint, the possibility of observing the future is the consequence of the intrinsic phenomena of physical systems: mainly the influence of the future on present conditions. For instance, if it is possible to observe the position of the stars in the future, theoretically it is possible also instantaneously to influence the present position of a star and so to change its future.

Physical properties of time free the world from a strict determinism and so the image of the future is always "vague" (Kozyrev, 1980). Some Soviet philosophers believe that, if Kozyrev's theory is correct, it is possible to really guess the future, like the prophets do (Kunitsyn, 1984).

There are two extreme viewpoints about the events in the world: determinism and indeterminism. They must be integrated, because neither one separately reflects the truth. The theory of probabilities suitable in quantum mechanics, describes the random "behavior" of elementary particles in the microworld. However, the paths of stars are more determined than the trajectories of elementary particles. Therefore, we do not say "probably the sun will rise tomorrow," although there is the slightest probability of its not rising.

Mathematical statistics and the theory of probabilities are used to describe very complicated processes, when they depend on many variables. One of the examples is weather forecasting. Today it is almost impossible to change bad weather even when we have information by means of scientific calculations. But there are many examples of scientific predictions of probabilities of events in the future that can be averted. These predictions are made in a logical way and it is not necessary to use Kozyrev's theory to explain them. However, information about the future may be gathered in another way. The Soviet physiologist Peter Anokhin, who with Norbert Wiener introduced the principle of feedback in cybernetics, believed that, in order to survive, even individual cells must have the ability of precognition. It is impossible to connect this kind of precognition to logic. Apparently, there are other ways of the functioning of psyche. It was shown, for example, that a surprising similarity exists between contemporary theories in physics and the insights of ancient mystics who lived 2,000 years ago, (Capra, 1976). Precognitive dreams and the future events that psychics can see are very similar to the direct vision of the ready results of complicated arithmetical calculations obtained by "psychic-counters." This "penetration" into the future is a vision of events which have more or less a high probability to happen. Sometimes it is possible to change those events, sometimes it is impossible. However, to experience these visions, it is necessary to perceive some data from outside. That is why Kozyrev's theory about the properties of time today is the only one in physics which can explain the phenomenon of precognition.

### *Conclusion*

A working hypothesis can be proposed about the connection between some psi phenomena, like healing as well as GESP, and the changes of entropy in the living organisms. However, the question of how these changes can be transmitted at great distances remains unanswered. Physical properties of time may be responsible for this transmission. In addition, if time has energetic characteristics, it is possible to explain the finding of Tart, namely the appearance in the laboratory of PK with the same frequency as precognition (Tart, 1982).

"Remote action" is one of the main points of Kozyrev theory criticized by established science. But there are experimental data about "remote action" (instantaneous connection) in quantum mechanics, too (Aspect, Grangier, & Roger, 1982). The only criterion to judge whether the Kozyrev theory is correct or not must be the duplication of the experiment to determine the true position of stars by astrophysicists. And if we live really in the world of Minkowski (Kozyrev, 1982) the theory of the physical properties of time must change the way of thinking of scientists and so explain not only the phenomenon of precognition, but other psi phenomena, too.

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## *DISCUSSION*

WALKER: There is a generally discussed notion in physics about a connection between entropy and time. I believe what is being suggested here is a modification to this theory in which you would have two regions that are totally outside contact with one another. If in one region you have a deviation in entropy there has to be an overall conservation of entropy in a second region. This would suggest the experiment with the Wheatstone bridge in which you're looking at a distant star that is actually in a position right now that you do not see it to be in because it is moving. Right now you see it in this location and that is because the light you are seeing it with came a million years ago, but it is now over here. You are going to take your apparatus and look at that future position and see whether or not you detect something with the Wheatstone bridge. At least that is my understanding of the experiment.

ADAMENKO: What is your question?

WALKER: I am just trying to paraphrase this. Is the theory one in which it is being proposed that you have conservation of entropy in two different regions or is it a theory in which an excursion in deviation in entropy in one system is compensated for in the other? You can have an overall decrease in entropy allowed, but if you have an excursion in one region from what the usual theory would say, it is compensated in another system. So that is two different notions.

ADAMENKO: The consequence of Kozyrev's theory is if you consider two systems connected for a "physical properties of time," the order lost when entropy increases, for example, in the stars can be transmitted by changing the density of time, to the substance of the detector, increasing its order.

MORRIS: What do you think might be the impact upon Russian parapsychology of glasnost? One impact is that you are here. Might there perhaps be others?

ADAMENKO: I do not know. I think that now the situation in parapsychology changes, too, but it began before *glasnost*. In science it is possible of course to stop for a short time to develop the science slowly. In parapsychology if you have a good scientific result, *glasnost* must help to develop it.

# TECHNOLOGY:
# A MIXED BLESSING FOR MODERN PSI RESEARCH

EDWIN C. MAY

*Introduction*

Technology is sometimes blamed for society's ills. Water and air pollution, the threat of nuclear destruction, and the greenhouse effect (to name just a few) are considered to be the result of technological advances. Of course, technology itself is not inherently evil; rather, difficulties such as these result from our misuse of it.

Subtle difficulties, in fact, arise with the use of technology. As we become more dependant upon it, we risk losing basic knowledge about the world. For example, who can remember how to hand-compute the square root of a number, now that we have calculators? As we rely more on the expertise of others (in this case, the individual who programmed the square root function), we become dependant upon their view of reality and lose the ability to make independent judgements. It is all too easy to take as fact the answers our technology provides.

Having warned against some of the pitfalls of technology, we consider some of its benefits. When carefully applied, our technology has enabled us to make advancements across most of human experience. In the physical sciences, our rapid increase in understanding has resulted primarily from an accelerated growth of technology. In the behavioral sciences, the single most important technological contribution has been the invention of the computer. Fifty years ago, handling large databases and computing intricate analyses was nearly impossible: now our microcomputers do it with ease. Complex statistical analyses such as ANOVA and MANOVA can be performed with a simple push of a return key.

In this paper, while we provide a brief overview of two examples in psi research where the reliance upon experts and technology have led us momentarily astray, the primary focus is on a technologically sophisticated experiment to explore the effects of feedback in a remote viewing (RV) experiment.

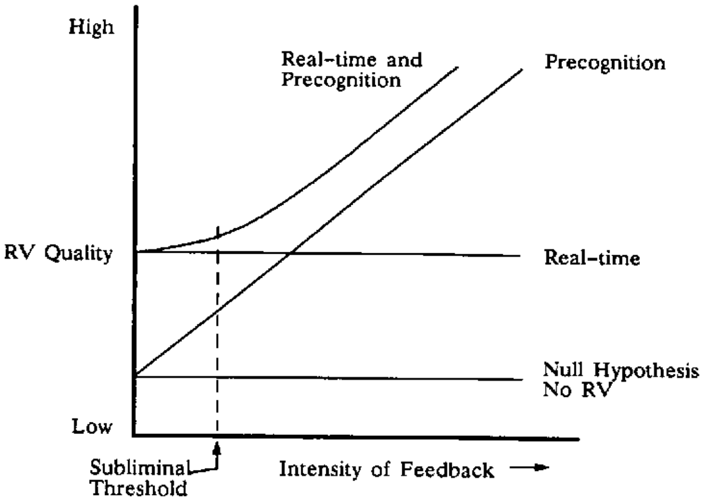


Figure 1. Idealized curves of potential relationships between RV quality and intensity of feedback

## *Geomagnetic Effects on Psi Performance*

In recent parapsychological literature, researchers have shown considerable interest in observed correlations between psi performance and the geomagnetic field (GMF) indices (Adams, 1985; Persinger, 1977, 1979, 1985a, 1985b, 1985c, 1985d, 1986). Providing a complete analysis of this literature is beyond the scope of this paper, but a description can be found in Hubbard and May's 1986 paper. They described the origin of the GMF measurements, the hardware involved and the reporting practices of the National Oceanic and Atmospheric Administration and showed the distributions for the *ap* and *aa* indices.

If one simply used the *ap* or the *aa* index and attempted to compute correlations with a psi performance measure by standard statistics (e.g. ANOVA), it would be possible to underestimate residual variances because of the important underlying structure in the GMF. In a test sample, Forbush et al. showed that *p*-value (computed with standard ANOVA) of $10^{-15}$ increased to 0.33 when the correct residual variance was used (Forbush, Pomerantz, Duggal, & Tsao, 1983). Hubbard and May called attention to the extremely low frequency (ELF), ultralow frequency (ULF), and GMF literature, which demonstrates that blind reliance upon the GMF indices ignores the contribution from local sources, and ignores the strong spatial dependences (coherence is less than 25% 600 km away from a measuring station).

We do not mean to imply that technically difficult problems should not be addressed. In a recent paper, Persinger specifically examined these potential problems and demonstrated that a significant correlation may indeed exist between the GMF and psi performance—a very important result, if true (M. A. Persinger, personal communication, August 1988). But even in his latest paper, a problem may remain with the statistics. While MANOVA can deal with the statistically dependant data points, it assumes that the covariance matrix of the data set is stationary (i.e., the variances and covariances do not depend upon when the data sample was measured). In brain wave data this assumption is completely false, but for GMF data over a few days, it *may* be valid. The point is that the vast literature and considerable expertise available to psi researchers must be utilized before we can begin to contribute to the general research literature. It is simply a mistake to find published GMF indices and calculate various quantities with ANOVA to search for correlations with psi performance—a very tempting thing considering how easy it is to accomplish.

*Observation Theories*

The observation theories assume that room-temperature macroscopic (i.e., $>10^{23}$ atoms) objects are governed by the formalism of quantum mechanics. In particular, these bodies can exist in indefinite states (i.e, not in any of their allowed states). In other words, macroscopic bodies, prior to observation, exist only as a set of possibilities rather than as unobserved actualities. As in the GMF case, it is beyond the scope of this paper to describe the variations on this theme that constitute the observation theories; a broad overview and references to specific papers can be found in Edge, Morris, Rush, and Palmer (1986).

Current quantum mechanical formalism does not prohibit macroscopic superposition, but there is substantial evidence against the idea. It is true that some quantum mechanical effects can seem macroscopically (e.g., the single-photon interferometer, tunneling) but they are, in fact, manifestations of single quantum events rather than a phase-related macroscopic phenomenon. Phase-related events are required before macroscopic indefinite states can be observed. Washburn and Webb (1986) and Chakravarty (1980) have demonstrated true macroscopic phenomena, but under exceptional circumstances. Cooled to 0.050 degrees above absolute zero, $10^5$ atoms maintained quantum coherence, and thus Washburn and Webb and Chakravarty were able to prepare systems in indefinite states. These states decay rapidly when the temperature is slightly increased. The implication is that quantum coherence is completely lost even at a few tenths of a degree above zero. Since coherence is required before a body can exhibit truly macroscopic quantum effects, saying that room-temperature devices can exist in indefinite states contradicts experimental results.

Why this is so, is well understood. The quantum mechanical mathematical description of a macroscopic body has on the order of $10^{23}$ terms and each term has it own coefficient. There are no observed indefinite states of macroscopic bodies because, at room temperature, the relative phases of the coefficients are random. Even so, quantum mechanics does not *prohibit* macroscopic bodies from being in indefinite states. But the experimental evidence, so far, does not support the idea.

The observation theories are based upon an incorrect assumption. Room-temperature macroscopic systems are not in indefinite states. They cannot "collapse" under observation by humans, fish or any other forms of consciousness. Random number generators, ROM chips, computers, and so forth are not indefinite states. They may be in *unknown* definite states, but they are definite, nonetheless. A ROM-chip bit is either 1 or 0, but not both even if no one looks.

In this example, misuse of technology, per se, was not responsible for the error. Rather it was the reliance upon a few experts in quantum theory. We are not suggesting that we should refrain from speculation using unsubstantiated theories. In fact, one might argue that we are obligated to speculate, given the nature of psi data. But we must understand the orthodoxy in detail before we can refute it.

### *Feedback Dependency Experiment*

Beginning in 1986, SRI conducted a 2-year investigation of the dependency of RV quality upon feedback.[3] The experiment was conceptually quite simple, but to address precognitive issues it became technologically complex. In addition to the feedback question, we were interested in determining from what time frame a viewer accessed a target.

*Conceptual Description.* During the feedback portion of a RV session, the viewer is usually presented with a complete description of the target material and participates in a complete debriefing of the RV experience. In our experiment we eliminated all discussion of the target material and presented the feedback tachistoscopically. The intensities varied

[3] We would like to thank Dr. T. Piantanida for his valuble assistance with the psychophysics and visual details in this experiment.

from zero to a level that just exceeded recognition threshold. Extreme care was taken in order to insure that the viewer was the only individual who was simultaneously aware of both the target and the response.

Figure 1 shows a number of potential feedback dependencies. If a viewer acquires information about the target from the future feedback experience, then one might expect the relationship labeled as "Precognition." Likewise, if the information is acquired in real-time, then there should not be a dependency upon feedback ("Real-time" curve).

One important implicit assumption must be true before the various models shown in Figure 1 can be valid. Namely, the feedback experience is assumed to be proportional to the *cognitive* awareness of the feedback material. Under this assumption, the amount of information available at feedback time constitutes the independent variable.

*Detailed Description-Calibration.* The crucial independent variable is the amount of feedback perceived by the viewer. We assume that the magnitude of the feedback is directly proportional to the duration of the viewer's exposure for a given level of luminance. In a calibration experiment, subjects were presented with slides and asked to say when they were aware of the presentation. We manipulated the magnitude of the feedback from zero to a value where the viewer could recognize the gestalt of a scene. Each feedback slide was presented for 50 microseconds (ms), and the magnitude of the feedback information was adjusted by attenuating the luminance of the feedback slides over a range of two logarithmic units. In adjusting the magnitude of the feedback, we relied upon Bloch's Law, which says that for presentation times shorter than about 100 ms, the product of time and intensity is constant (Marks, 1975). Thus, varying the luminance of the feedback slide is equivalent to varying its duration.

For luminance calibration, the tachistoscope was loaded with 80 photographic slides (5 opaque and 75 having various luminance contrasts) of natural and man-made scenes (photographs from *National Geographic*) randomly chosen from a larger pool of 400. We varied the luminance contrast of the slides by duplicating them at one of twelve f-stops (including 0) to provide a target pool having variations in intensity covering two logarithmic units. The contrast in luminance for each slide, which may be considered to be the ratio of the brightest to the darkest part of the slide, was further attenuated in pilot trials so that some of the slides were above and others below the observer's detection threshold.

The 75 feedback slides and five opaque slides were back-projected by a Gerbrands G1170 two-field projection tachistoscope onto a 14-inch-square frosted glass window. The tachistoscope was programmed to present each feedback slide in numerical order for 50 ms, followed by a 5-second pause during which the next slide was cycled into position. Slides were attenuated by projecting them through a pair of plane polarizers: one fixed and the other variable. The luminance of the projected image varied as the cosine of the angle between the two polarizers.

Two naive female subjects participated in the calibration. A complete data set was obtained from one subject, and data trends were confirmed by the second subject.

The calibration procedures were as follows. The subject was seated approximately three feet from the projection screen, which was positioned at eye level in the wall between the room in which the apparatus was housed and the room in which the subject sat. The subject was permitted to view the screen and the other contents of the room freely for several minutes to ensure that she adapted to the ambiant illumination level. To screen the sounds of the tachistoscope, the subject listened to white noise through earphones. The response was registered by a foot switch that the subject pressed to indicate detection of the feedback slide. In a typical session, the variable polarizer was set at a predetermined value and each of the 80 slides was presented 5 times. Two sessions were conducted at each polarizer setting, providing ten data points per slide per polarizer setting. An alternative procedure was used when the variable polarizer was set near one of the extremes of the experimental range. (Under the extreme conditions, the subject saw nearly all of the slides or very few of them.) To reduce the tedium, only those slides near the detection threshold were presented.

Each time a new slide was presented, the subject reported whether the presentation was detected. Counters recorded whether a particular slide was detected as well as the proportion of slides detected. From these records, a psychometric function was generated relating the proportion of time each slide was detected to the contrast in luminance for that slide. This function, which relates the contrast in luminance for the slide to its detection threshold, is an index of the detectability of the geographic scene depicted in the slide. By using this psychometric function, it is possible to specify not only which slides are subliminal (i.e., never detected), but also how far above or below the detection threshold each slide lies.

Figure 2 shows a series of six psychometric curves generated by plotting the probability of detecting a given feedback slide as a function of the variable polarizer setting. The magnitude of target slide information was estimated from a psychometric function relating target slide contrast as abscissae and target slide detectability as ordinates.

Normally, data would be collected from a larger sample of individuals in order to arrive at an average function, but in this experiment, data from two persons were sufficient for several reasons. First, pilot studies indicated that interperson variability of target slide detection was quite low. Second, to collapse interperson variability even further, we generated a steep psychometric curve by sampling the abscissae coarsely. For example, if we sampled target slide contrast at only two values—0 and 100 percent contrasts—all observers would respond identically, thus eliminating interperson variation. In this study, we sampled target contrast at intervals that were found in pilot studies to produce low interperson variability. Finally, for the purposes of this study, interperson variability was not significant because it only shifts the psychometric function along the abscissa by some unknown amount without changing the shape of the function. Thus, interperson variability could only result in an erroneous estimate of feedback magnitude. While these errors may influence the intercept of the function relating the dependent variable (RV performance) to feedback magnitude, the slope of RV performance versus magnitude of feedback is independent of these errors.

*Detailed Description—Protocol.* Forty targets (selected randomly from the pool of 200 *National Geographic* magazine photographs) were prepared into eight intensity groups of five targets each using the calibration data described above. Each intensity group represented the cognitive awareness that each viewer would experience (on the average) at feedback time. Of the eight intensities, one was zero (i.e., no feedback at all), one was below subliminal threshold (SL), one was low SL threshold ( 25% recognition), one was mid SL threshold ( 50% recognition), one was high SL threshold ( 75% recognition), and three were of in-

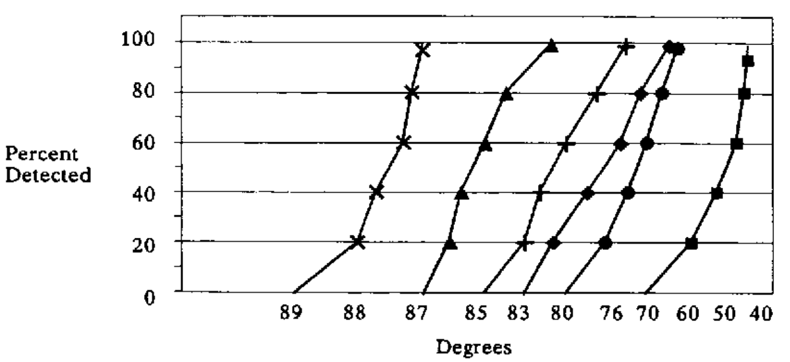


Figure 2. Degrees of polarizer rotation (scaled for equal luminance intervals)

creasing intensity above 100% recognition. The top two intensities were sufficient to experience nearly complete cognitive awareness of the feedback material. By definition, those below SL could not be cognitively sensed.

To attempt to maintain some control over precognitively available "answers," we arranged that at no future time would a response be cognitively compared to its intended target. Three pieces of information are needed to provide complete knowledge of a session: (1) the target, (2) the response and (3) the comparison between them. The target system was prepared by individuals who had no access to the responses. The RV monitor, the assistant and the viewers had no access to the targets. Finally, the analysts were never informed which were the correct results on a trial-by-trial basis.

The slide tray in the tachistoscope (the device to display the feedback material) was controlled by a computer (Sun Microsystem 3–160) in such a way that everyone was blind to target selection during a trial. For example, the tray always began and ended in the zero position. When the computer moved the tray, an independent electrical unit, which could be accessed by the computer, counted the tray steps to assure us that the intended target was displayed at the correct time.

Three experienced viewers (Viewers 009, 105, and 177) each contributed 40 trials (five at each of the eight intensity levels). A novice (Viewer 137) also contributed 40 trials.

A random order of intensities of feedback was determined (by computer) once (and differently) for each viewer prior to the start of the start of the viewer's first trial. Once the order had been set, the trials cycled through the list of intensities until the 40 trials were complete. The sequence of events for each trial was as follows:

1. A monitor and a viewer entered a laboratory that contained a table, two chairs, a computer terminal and a covered 14-inch-square frosted glass window. The window served as a projection screen for the tachistoscope in the adjacent laboratory.

2. When the viewer was ready for the session, the monitor initiated an automatic target selection program on the terminal.

3. The computer randomly selected (with replacement) a target from within the set of five for the given intensity, stepped the slide tray to that target and notified the monitor that the trial could begin. Because of the closed tachistoscope shutters, no illumination of the slide was present on the frosted screen.

4. At the conclusion of the session, the monitor collected the response and the viewer opened the screen cover in such a way as to shield the monitor from the feedback material.

5. When the viewer was ready, he or she pressed a button that initiated a single tachistoscope display of the target. One, and only one, display appeared on the translucent window screen. (Electronics prevented the viewer from receiving more feedback after the first button press.) The monitor was instructed *not* to discuss the experience with the viewers in any way at any time.

6. The monitor ended the session, and notified the control program. After the computer had returned the slide tray to zero, then, and only then, did the monitor and viewer leave the room. All target data were preserved in a computer file.

*Detailed Description-Analysis*. The rank-order analysis used in this experiment has been described elsewhere (Humphrey, May, & Utts, 1988), so only an overview is presented here. Using cluster analysis, all 200 targets had previously been assigned to orthogonal clusters of similar targets (i.e., every cluster of similar targets differed from every other cluster.) An assistant prepared packages (one for each viewer) consisting of all the responses randomly ordered. Next, the assistant generated a list (ordered on target number) of seven targets for each response consisting of the actual target and six decoys (a different set of seven for each response). The decoys were chosen from clusters different from each other and different from the target cluster. The decoy clusters were shown randomly from a set of 18, weighted by the number of targets in each cluster. Once a cluster was selected, the decoy was randomly selected from within the cluster. This procedure assured that all targets were equally likely to be chosen as a decoy.

The response material, and the target lists were presented to two analysts for judging. The analysts arrived at a consensus to rank order each set of seven targets for each response in accordance with the best to the worst response/target match. For each viewer, a sum-of-ranks statistic was computed for the sessions. In addition the data were plotted as RV quality (i.e, one minus the assigned rank) versus feedback intensity.

*Detailed Description-Results and Discussion*. Table 1 shows the sum of ranks, associated $p$-values and effect size for the tachistoscope feedback experiment.

Viewers 009 and 177 produced independently significant results (1-tailed). We can combine the data for all viewers in many ways, but the most conservative is a binomial calculation assuming an event probability of 0.05. Two success in four trials corresponds to an exact $p$-value of 0.014. A more realistic estimate is provided by a minimum $p$-value technique (Hedges & Olkin, 1985) which yields $1.4 \times 10^{-4}$. The

TABLE 1

Tachistoscope Feedback Experiment

| Viewer | Results | | |
|---|---|---|---|
| | Sum of Ranks | $p$-Value | Effect Size ($r$) |
| 009 | 131 | 0.012 | 0.357 |
| 105 | 182 | 0.962 | −0.281 |
| 137 | 159 | 0.484 | 0.006 |
| 177 | 104 | $3.5 \times 10^{-6}$ | 0.711 |

important point, however, is that this experiment produced strong evidence for an informational anomaly.

Figures 3 through 6 show RV quality (one is low, seven is high) plotted against intensity of the feedback of the four viewers. Shown also is the regression line and its associated linear correlation coefficient for each viewer. These figures should be compared to Figure 1, the idealized expectations. The result that is easiest to understand in Figure 1 is the positive correlation showing increased RV performance with increased feedback intensity. We did not observe any such correlation with either of the significant viewers. In fact, the linear correlation coefficients were not significantly different from zero.

The lack of positive correlation in the light of significant evidence of RV complicates the interpretation considerably. The most obvious conclusion is that the viewers obtained their data in real time and not from their later feedback. Another hypothesis is that the underlying

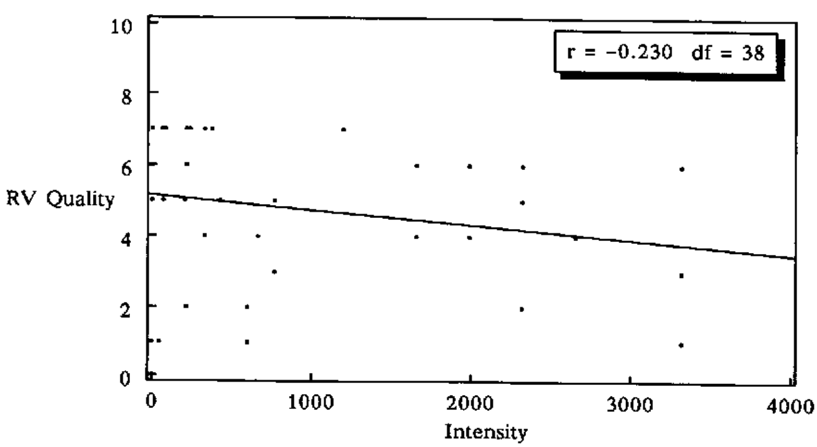


Figure 3. RV quality vs. feedback intensity: Viewer 009

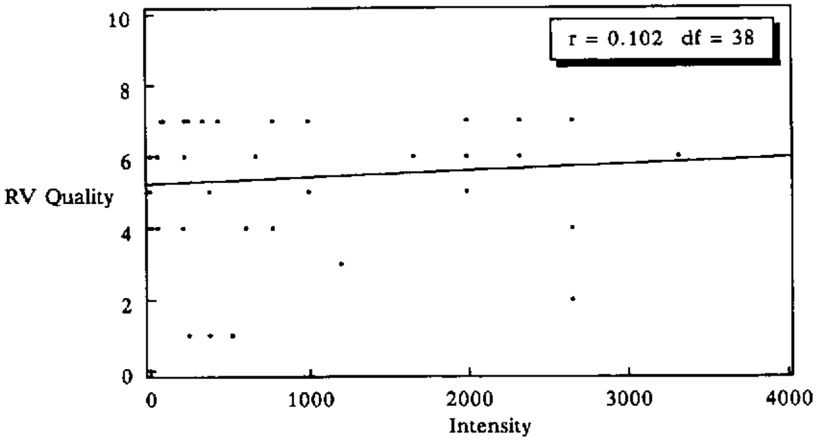


Figure 4. RV quality vs. feedback intensity: Viewer 177

assumption that the cognitive awareness constitutes feedback information is incorrect. If this were true, we would expect to see no correlation with intensity even if the precognition model were correct.

*Conclusions.* Modern technology, correctly applied, allows psi researchers to address questions that were difficult or impossible a few years ago. While there are certain pitfalls, technology's benefits far outweigh its drawbacks. Technology itself, though, may not provide the answer to difficult questions. In psi research, there appears to be a lack of symmetry. Had the experiment described above supported a

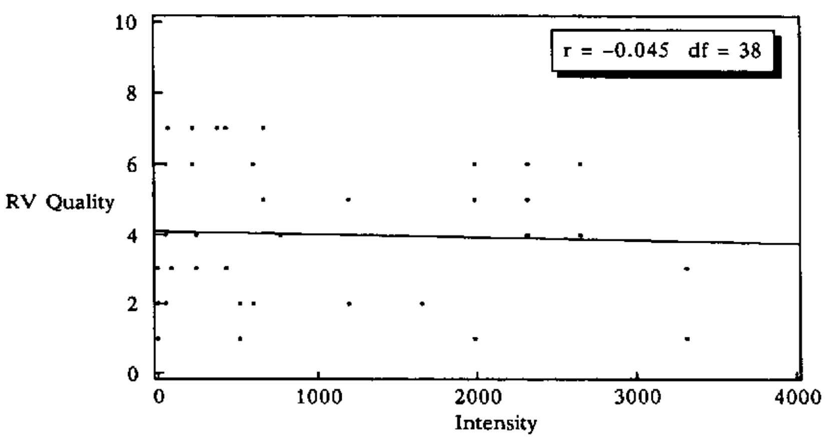


Figure 5. RV quality vs. feedback intensity: Viewer 137

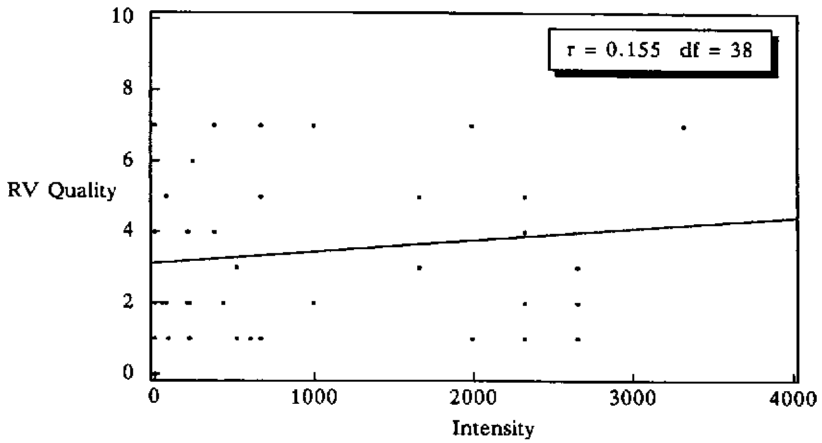


Figure 6. RV quality vs. feedback intensity: Viewer 105

precognition hypothesis, the result would have been far less ambiguous. There would have been little doubt that feedback is cognitive and that RV processes must include a precognitive component. Given that no correlation with feedback intensity existed, alternatives (to real-time) must be considered. In a broad sense, significant correlation between variables implies information about the system; therefore, it is not surprising that this conceptual asymmetry exists.

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## *DISCUSSION*

RAO: Ed, I am very glad you were able to moralize about the possible problems with complex technology in our research. I was a student of Gandhi and shared his concern about the advance of technology to which man might become a slave. So this reminds me of that. But what scares me more is what you said in your philosophical introduction relating to PK research. You say that you spent a quarter of a million dollars to do an experiment to discover so many variables that you do not know what they mean and how to control them. You therefore shifted your research focus from PK to ESP. Now one of the research areas that excited most of us in recent years is the RNG research. Much of it was done with less sophistication than your quarter of a million dollar experiment, but we believe it has provided quite a solid piece of evidence for a facet of parapsychological phenomena. Are you suggesting that we should write off most of this evidence? Or do you believe like us that there is some validity to this kind of experimentation? If there is and it can be done less expensively, why could it not be done again?

MAY: Thank you, Ram, for pointing out to me something that I unfortunately left out of the main body of the talk. First of all to answer your question, yes, the RNG work is, I think, substantial evidence for psi. My criticism of the PK work really—although I did not say it and I apologize to you—was aimed primarily in fact exclusively at what we might call macro-PK investigations. What would be generally called micro-PK investigations involving random number generators or any system where you are basically looking at statistical differences between control and other groups, is perfectly OK. Because there you can control by looking at control groups, but if you are dealing with a high technology experiment where you are trying to, say, do a strain gauge experiment replicating Julian Isaacs' work or trying to do any large scale system, that is where the problems lie. And thank you for allowing me to correct that. That was an oversight in the presentation.

PALMER: One of the points that came to my mind as I was listening to your paper was the whole problem of attempting to rule out with one experiment all the often multitudinous explanations of a particular outcome. I think this is rarely possible, but it seems that in our research, experimenters often attempt to do that, or that is what they claim in their reports. Also, referees often demand it. Someone may do an experiment that is a contribution to knowledge, but since it does not quite rule out all the alternatives it ends up not getting into the literature. Perhaps what we need to consider is experimenters being more modest about what they claim for particular studies and to simply state out front in their discussion sections that not all of the alternative explanations have been ruled out, which should be okay as long as they have made a good faith effort to rule out the ones that are consistent with experimental competence. This approach encourages series of integrated experiments where you successively rule out the remaining interpretations. This is something I would like to see much more of in our research, a programmatic series of experiments. We really do not have anything in parapsychology comparable to the old parapsychological or psychological monographs, where a series of experiments are published together. I think our research literature would be more impressive if there were more of this kind of research being done and published.

MAY: Your point is well taken. First off I think it is maybe impossible to do an experiment where you have excluded all of the alternatives, because you do not know what all of the alternatives are to begin with. The best you can do is the best you can do. I do not mean that flippantly. You can certainly take into account the things that you know about. But one point that I tried to make in the body of the paper, but not in the presentation is that there is a certain asymmetry in the kind of research that we do—maybe in everybody's research. Had this result fallen in line with my particular bias the way that I thought it should, I think then the interpretation of it would not have been quite as cloudy. I do not know if anybody would agree. It is certainly my own speculation that you end up getting a moderately null result—and I mean by that not that you did not see any evidence for psi, but rather you did not see any evidence for psi in accordance with the model you were testing. Then you have much wider opportunity for interpretation. It is less specific than if you had a definite model in mind, tested it and it all fell right along with that model. Then you are more constrained in your analysis. So there is a certain kind of philosophical asymmetry in interpreting results, but I completely agree with you. I mean I do not consider this experiment a failure in any sense. I think it deserves to be reviewed by our colleagues and published.

SCHOUTEN: Let me first say that I am really happy that you pointed out the potential pitfalls in using high technology. But I think there is another side of the coin too. What occasionally happens is that when non-psychologists enter the field they can do things with psychological instruments like scales which are horrible.

MAY: Physics, too.

SCHOUTEN: You manipulated feedback levels and you did it by setting a threshold and I think you used two subjects to set the calibration level. It is of course known that threshold levels vary widely between subjects. Did you check with your subjects whether the feedback levels you manipulated really worked? In your paper you gave two examples where misunderstanding led to conclusions or research which perhaps has basic flaws. About the observation theory, it is supposed that only because the subject observes the outcome you can talk about it in terms of quantum mechanical processes. I always considered that very strange. I was impressed when I read your paper. And what you wrote about it. Does that mean that you consider the observation theories in that respect as invalid? And another question I have always tried to ask physicists but never got an answer to, is that as far as I know quantum physics never said that an observer could change the probabilities as described by the state vector. As I understand the observation theories maintain that probabilities would be changed due to the wishes of the observer. Is that not in contradiction to quantum mechanics?

MAY: You brought up a number of issues there. The last one could be the study of a course in quantum mechanics lasting many months which Dr. Walker would be far more qualified to teach than I. Let me take them in the order in which you gave them if I can try to remember them. First off on the variation of subliminal thresholds, an exquisitely important point, brought up a methodological problem for us. We did not want to ever show even subliminally in later tests the actual target material we used in the real study to try to determine what the individual participant's subliminal threshold was. That was a methodological issue involving precognition. So clearly that would have been a better thing to have done. On the other hand the way that we tried to address this question (and I am a little out of my surroundings here) Dr. Piantanida arranged the contrast ratios of the various slides to make the isometric curve very, very steep so that the intersubject differences in the threshold would not matter too much. Basically all it would do would be to slide the curve back and forth horizontally. Since we were not doing it across subjects, I really did not care where along the tensity access the 50 percent recognition threshold came, so we made the experiment insensitive along that line. On the quantum mechanical issue, the reason I brought the observation models in was really to point out a difficulty that both Dr. Walker and myself have and one that you have which is even worse. One of the exciting and negative aspects of doing the interdisciplinary research that we are all involved in here is that if you take a complex issue such as quantum mechanics where reasonable people can disagree on the interpretation of experiments and the interpretation of the theory, you have standing before you a number of physicists arguing with each other that you have a problem on your hands. You know I think I am right, he thinks he is right and we are trying to do experiments and working very close together with each other to try to determine some aspect of truth on that issue. I can tell you what my opinion is and note that as is well known Harris does not agree with all of this. My opinion is that at least the quantum mechanical aspects of the observation theory are silly. There is, in my view, just no evidence that observation of a large scale quantum system does anything to it. Now I can see him wincing over there, but I have given him his due. The other aspects of the observation theory, particularly from your facility, I have not frankly taken as careful a look at as I should. OK?

MORRIS: First, as I think we have discussed before, there may be a confound about the duration of exposure of the information of the feedback to the viewer. In terms of the viewer's own imagery therefore and the details and the extent of their elaboration of their own experiences, that makes it a very difficult measure ever really to apply.

MAY: Terrible, I agree.

MORRIS: Secondly, suppose the thing had worked. Then a set of alternative interpretations might have been dependent upon when and how the feedback duration condition was assigned in terms of realtime alternative interpretations. I think this is a general problem with a set of strategies whereby you vary the properties of the feedback and attribute meaning to any correlation obtained. It could be that if, in fact, the condition is determined before the person generates his imagery, then that information is then available in real time. So they will generate better protocols whenever the duration is going to be longer because that information is already available. If in fact they have already generated a protocol, then when later on the assignation condition occurs, there could be psi influences at that time lining up the condition to match the good protocol.

MAY: There was an underlying assumption in this experiment which takes up some of the points which you were making. One of the underlying questions was, first of all, what constitutes feedback? That is a question to which I have not a clue. And that is of great interest to me. What I assumed constituted feedback in this particular experiment was somehow related to the subliminal or cognitive realization of getting the answers. Well, we were at the wrong end of the spectrum for that. On account of these things you would not come away with a very profound internal cognitive experience looking at even the most robust of these feedbacks. So one of the criticisms that Piantanida has given us on this particular experiment is the violation of that particular assumption. Maybe we should just have slid the curve way over and varied the more robust aspect of the feedback. Had we done that your comments would even be more true than they are already. So it is a big question as to not only where the data come, from which is of interest to me as a physicist, but what constitutes feedback is even worse. Now just as a point we did not vary time. It turns out that if you are in this weak presentation environment in a regime, you can trade off time of presentation with intensity of presentation, holding time fixed. So our presentation was 50 milliseconds long and we varied the intensity. But it is effectively the same.

HONORTON: Ed, one of the problems that I have become increasingly concerned about in doing the kind of systematic process-oriented, free-response study that you just reported is the impact of target variability. In any kind of free-response situation like this you have a very limited number of trials per subject. To what extent did you know the success characteristics of the particular targets that were used? That would at least minimize the likelihood that you could completely throw off any systematic relationship in terms of the kinds of targets that were used.

MAY: Well, I have to say you got me! I will use that as a reason why we did not get my expected curve out of all of this. The serious answer to that question is that we have only observed preferences for some targets in a casual way and, in fact, I think your criticism is extremely valid. It really calls into question the interpretation of these kinds of results. But, since your work where you have seen variations in some of the target material you used, we carried out last year, and are continuing to carry out, investigations of the differences between dynamic and static targets. We are beginning to see some differences that are similar to what you have reported, but that is an extraordinary confound for the interpretation of this experiment.

HONORTON: I did not mean it as a criticism. I wanted to ask you what do we do for free-response experiments when we want to find out more than that there is a psi effect going on, given the likelihood that there are variations of that type?

MAY: I wish I knew the answer to that. Even though you may take a particular individual who shows a preference for a certain class of targets and design the target pool for that individual, that may not hold for your individual. It may not hold across procedures. I think the only answer must be that you must do within-subjects kinds of experimentation and not look at the global issues. At least you have some way of getting an independent measure, over a long period of time with a given individual, of how well that individual does on these targets and not on those. Maybe you can hopefully control for that condition within a subject. I would throw up my hands, thinking across subjects—too hard for me.

BRAUD: I was going to ask two questions that you essentially anticipated in your response.

MAY: See, precognition is real after all.

BRAUD: Let me ask them anyway, though. You said that the feedback was wholly unsatisfactory to the subjects.

MAY: Terrible, they complained bitterly.

BRAUD: The two questions are one, what effect do you think that so negative a factor could have had on your experiment? And secondly could it be that feedback is having an effect upon performance but that the function is non-linear and you happen to be working at a portion of the curve where you did not expect any differences?

MAY: That was one of the things that occurred to us after the fact. I am not making a big deal out of it because one of our individuals psi-missed, scoring significantly below chance. It turns out—and this I have to qualify as simply a laboratory anecdote—that this individual was the most loudly complaining person about the nature of the feedback. But one point makes not a theory. So it must somehow be connected there, but with four people you can't answer questions like that.

STANFORD: I think we are seeing another example of anticipation of questions here, so you can take another bow about precognition. William anticipated my question pretty strongly. I too was concerned about negative reactivity to low levels of feedback. This is a way of getting you off the hook about precognition, if you do not mind. Obviously it is purely *ad hoc*, but experimenters suppose that subjects are frustrated by lack of feedback and that, in some sense, they can anticipate that precognitively and so they step on the psychic gas a little bit harder. They know that there is an obstacle, that they are not getting all of that feedback. That can directly counteract the curve that you are talking about.

MAY: I do not want to leave the impression that I think this experiment proves or disproves precognition. Clearly after the meta-analysis that we learned about yesterday from Chuck there is no question, at least in my mind, that precognition is a fact of nature. I just wanted to put that in.

ROLL: One of the sections in your paper on geomagnetic effects does not, I think, take into account Michael Persinger's paper that he presented at the PA convention.

MAY: I took some swipes at the geomagnetic correlations that have been reported in the literature by a number of authors—including some of us—claiming correlations between geomagnetic activity and certain psi abilities. Talking with Michael Persinger up at the Montreal PA Meeting and reviewing his paper in detail, frankly in my view he is the only one who has really done the job reasonably well. One of the problems is if you use ANOVA to look at those data it is just a mistake. And it is a mistake because it violates at least one or possibly two underlying assumptions. Number one is that the data from point to point are not statistically independent and a procedure called MANOVA can fix that for you and Michael and others who are beginning to use that. But there is still a question, at least in my mind, because MANOVA still assumes what is called a certain degree of statistical stationarity. In other words, no matter how screwed up the data actually are as you are sampling them, it does not depend on when you sample them. That is a really rough way of describing what stationarity means, with apologies to my colleague Jessica down there. But nonetheless that condition which is a requirement for ANOVA, for MANOVA to be true may not be so strongly violated over the short period of time like seven days that he uses. But it is clearly a mistake if you are going to do any kind of brainwave analysis. Brainwave analysis is very tricky if, in fact, you want to show statistically significant differences in anything, because those data are horrible. The points are not statistically independent and they are by no means stationary. Now there are some mathematicians who can address that from turbulence and hydrodynamics and other areas in physics, who are well versed in how to deal with such crummy data, but if you want to do brainwave research to make those kinds of measurements, please, please be very careful.

BROUGHTON: I would like to make a comment and ask a question. The comment is that I am glad that you are cautioning us about the pitfalls of high technology. I just wanted to note that there are very strong echoes in what you were saying of the last Parapsychology Foundation conference in which I participated—1981—on the use of computers in parapsychology.[4] A number of us who use computers argued the same thing: that we really have to understand what we are dealing with when using a computer program in studies of psi. There is a very strong temptation to just get it off the shelf. Most of us here who work with computers have received unsolicited psi tests some of which were really appalling and made very naive mistakes. If these things go out and people do not really understand what is behind their simple computer program, we end up with some really embarrassing gaffes. On a completely unrelated topic, just to get back to the tachistoscope experiment you mentioned, I wonder if we really will get some of the answers without looking at subject differences. As we talked about this several times during the conference so far, we have been very concerned about how different subjects are going to react to our experiments and we talked about the perhaps negative aspects of the feedback. Following on Bob Morris's comments, is there any way we could really account for things like the Poetzl effect? Even though you are trying to control the conscious feedback, suppose your two subjects who did very well dreamed about the target or incorporated, for hours on end, little aspects of your target 12 hours later. I do not know how you could control it, but it might be of relevance.

MAY: Well Richard, you and I have discussed at length one aspect that I personally find extraordinarily unsettling about models based on precognition. I have a favorite one, Intuitive Data Sorting and the problem with models based on precognition is that you can look into the future and virtually anything you want to have is almost unfalsifiable and very unsatisfying. You gave one nice example of that. I can't control for that. I simply do not know how to do that. So research in a systematic way on precognitive models is exceptionally tricky, very tricky indeed. I want to make just a brief comment about your computer observation. It is not just a problem of computer neophytes learning how to use this new technology. It is a problem that spans all computer disciplines. In

[4] The 1981 conference was *Parapsychology and the Experimental Method*, edited by B. Shapin and L. Coly and published by the Parapsychology Foundation in 1982.

physics there are huge computers called CRAYS that are very, very fast and will do calculations for days on end and come out with a number which is an answer but not quite the answer. How in blazes do you know whether that number is right or not? You built the CRAY in the first place because you can't do it by hand so, do you check it? And if it is making mistakes, how do you know? Those are very serious questions. There are disciplines growing up in physics and other areas where computer models are being substituted for animal models, for example, or computer models are being substituted for nuclear explosions and the like. How in the world do you know your big fancy computer is giving you the right answer? Tricky questions, so we need not be at all apologetic to meet the same questions about psi.

# ON THE USE OF LIVING TARGET SYSTEMS IN DISTANT MENTAL INFLUENCE RESEARCH

WILLIAM BRAUD

*The more it moves, the more it yields.*
- Lao Tsu

For several years, my co-workers and I have been exploring the use of living target systems in our research on distant mental influence. We have found that living systems possess many characteristics that make them exceedingly attractive and useful to the psi researcher. I would like to share with you some of my thoughts about the positive features of such systems and their many advantages for psi research in all of its aspects—experimental, theoretical, and practical.

## *The Theme and its Variations*

In a typical experiment of the type to be discussed, one selects a living organism and isolates it, usually at a distance, from all conventional sensori-motor or energetic influences of the "influencer." In principle, any living organism may serve as this "target." Next, one selects some readily measured aspect of the target organism's activity, objectively monitors that activity over a period of time, and generates a permanent record of that activity. It is, perhaps, desirable to choose an activity which occurs with moderate frequency or intensity and which is relatively stable over time, although this is not an essential requirement.[1] An "influencer" then attempts to influence the organism's activity, mentally and at a distance, in a prescribed fashion and according to a predetermined (and, ideally, random) schedule. Conventional statistical methods are used to compare the organism's activity during periods of attempted mental influence with activity levels during comparable non-influence, control periods.

The procedure may be illustrated more concretely by an experimental technique that we have used over 300 times in our laboratory. A subject sits in a quiet, comfortable room for approximately 25 minutes while his or her sympathetic autonomic activity is continuously assessed by computer-monitoring of the subject's electrodermal activity (EDA). In another room, typically 20 meters away, an influencer attempts to mentally "calm" the distant subject during ten 30-second influence periods, but not during ten interspersed control periods. Total EDA during the influence periods is compared statistically with total EDA during the control periods in order to determine whether the experiment was successful.

This is merely the latest version of an experimental procedure for which there is a long and interesting history. Some of the earliest variations of the paradigm occurred in the contexts of "animal magnetism," "mesmerism," and hypnosis. As early as 1775, Franz Anton Mesmer described informal experiments in which he claimed successful action-at-a-distance effects, explaining them in terms of propagations in a "universal fluid" connecting all things (Mesmer, 1775, 1779, 1799). Similar distance and nonsensory effects were reported to occur in the *somnabules* of Amand-Marie-Jacques de Chastenet, Marquis de Puysegur, one of the chief French disciples of Mesmer. According to Puysegur, his "artificial somnambulism" effects, which he published in his 1784 memoirs, depended importantly upon the mesmerist's firm belief, faith, and confidence in his own powers, and in his strong wanting or willing of the desired effects. Distant "mesmeric" effects were reported by James Esdaile (1852) in India, and by John Elliotson (1843)[2] and Chauncey Hare Townshend (1844) in England.[3]

Perhaps the best known, and best controlled, of these early distant influence attempts were the remarkable le sommeil a distance experiments carried out in Le Havre in 1885 and 1886 by Joseph Gibert and Pierre Janet with the special subject, Leonie B. (Janet, 1886a, 1886b), some of which were witnessed by psychical researchers Frederic W. H. Myers, Charles Richet, and Julian Ochorowicz. Richet (1888) later reported his own similar successful experiments with Leonie and with other subjects.[4] P. Joire (1897), in Lille, France conducted experiments on mental suggestions of specific motor acts.

Between 1920 and 1922, the once heralded, but now, unfortunately, neglected experiments of Brugmans, Heymans, and Weinberg were conducted at the University of Groningen in The Netherlands. These experiments, which involved distant mental influence of motor actions in a special subject, A. S. van Dam, have been re-assessed by Schouten and Kelly (1978).

Controlled experiments were conducted at the Institute for Brain Research of the University of Leningrad in an attempt to determine whether complex motor acts in dogs (specially trained for "will-less" obedience) could be influenced by mental suggestion in the absence of sensory cues. The subjects for these experiments, which yielded suggestive but not entirely satisfactory results, were the trained dogs of Vladimir Durov, a celebrated circus clown and dog trainer; the experimenters were the respected reflexologists, Vladimir M. Bechterev (1920) and A. G. Ivanov-Smolensky (1920).[5]

Between 1921 and 1938, the mental influence research at the Institute for Brain Research shifted its emphasis from dogs to humans, and experiments were carried out, primarily under the direction of Leonid L. Vasiliev, which constitute what is perhaps the most impressive, systematic research on distant mental influence ever to be conducted. Vasiliev and his colleagues found positive evidence that "sleeping" and "waking," swaying, and a variety of motor reactions could be mentally influenced from a distance. A complete account of these experiments was not published until 1962 (Vasiliev, 1962). This book was translated into English, under the aegis of C. C. L. Gregory and Anita Kohsen (Gregory), and published in 1963 under the title, Experiments in Mental Suggestion. A revised edition appeared in 1976 as Experiments in Distant Influence.

It is noteworthy that in each and every instance of the research reviewed thus far, distant mental influence was attempted *only with special subjects*. The "hypnosis at a distance" trials were carried out with subjects who were already known to be especially susceptible to hypnotic influence, and the dogs studied in Leningrad had been specially trained, beforehand, for "will-less" obedience.

Following a hiatus of about a decade, additional reports of distant mental influence began to appear. These experiments almost inevitably involved the distant mental influence of more "primitive" biological systems and were typically carried out in two new contexts: psychokinesis and healing. These investigations now become too numerous to be mentioned individually. Fortunately, they have been well reviewed by Solfvin (1984) and by Benor (1984, 1985, 1988). Both selected and unselected participants attempted to mentally influence the growth or viability of bacteria, fungus colonies, yeast, and plants or to influence the movements of protozoa, larvae, woodlice, ants, chicks, mice, rats, gerbils, and cats. Some experiments involved attempts to influence cellular preparations (blood cells, neurons, cancer cells) or enzyme activity.

A very small number of researchers continued to conduct human influence experiments in the French and Russian traditions. Douglas Dean (1964) reported successful attempts to influence the direction of eye movements (recorded electrophysiologically) in dreaming "target persons." Hiroshi Motoyama (1977) reported successful attempts by one person to influence physiological activities (EDA, plethysmographic activity, respiration) of another person while the two persons were isolated in separate, lead-shielded rooms. In our own laboratory, we have observed successful distant mental influences by one person of the EDA, pulse rate, muscular tremor, ideomotor activity, and mental imagery of another person when deliberate attempts were made to influence these specific reactions (Braud, Davis, & Wood, 1979; Braud & Jackson, 1982, 1983; Braud & Schlitz, 1988). Elmar Gruber (1979, 1980) reported successful attempts to influence the locomotor behaviors of "target persons." Jule Eisenbud (1983) reported the tantalizing results of his attempts to issue mental commands for distant individuals to telephone him.

Of relevance to the mental influence theme are those investigations in which physiological reactions were used as psi indicators—e.g., Dean's (1962) plethysmography studies, Tart's (1963) EEG and GSR studies, the EEG influence studies of Lloyd (1973) and of Targ and Puthoff (1974), etc. Reviews of these studies may be found in Beloff (1974), Millar (1979), Morris (1977), and Tart (1963).

This brief survey was presented in order to indicate the range of phenomena to be addressed in this paper. Common to all of the observations is an influence by one person upon another person (or upon another living system). A number of terms have been offered as descriptions (or, unfortunately, even as explanations) of these interactions. When the experimental outcome involved symptoms of hypnosis, the terms "le someil a distance" and "telepathic hypnotization" naturally suggested themselves. When hypnosis was no longer the desired outcome, those terms gave way to "telepathy at a distance" or "active agent telepathy." "Telergy" and "living target psychokinesis" followed in the wake of PK experimentation. Rex Stanford (1974) suggested "mental or behavioral influence of an agent (MOBIA)." In our own work, we first suggested the term "allobiofeedback" (see Braud, 1978), since, in our initial experiments, one person received feedback for an attempted biological influence of another person. We subsequently observed that the provision of feedback was not necessary to the occurrence of the effect and coined the term "bio-psychokinesis (bio-PK)" to describe what we were interpreting as psychokinetic influences upon living systems (see Braud & Schlitz, 1983). In presenting this work at an international conference devoted to imagery (Braud & Schlitz, 1987), we used the term "transpersonal imagery effect," following the lead of psychologist Jeanne Achterberg (1985) who suggested "transpersonal imagery" for a possible effect upon one person of the imagery of another. However, imagery does not seem to be required for the effect; non-imagistic *intention* may suffice. Hence, I considered "transpersonal imagery or intentionality effect" (Braud, 1987). This term is much too cumbersome, and I am abandoning it in favor of the much more straightforward "distant mental influence," which seems to convey the essence of the interaction with minimal surplus meaning. In a later section of this paper, I shall consider in more detail the various processes or mechanisms that might underlie these phenomena.

*Advantages*

I shall discuss four important advantages of using living target systems in distant mental influence research.

- The findings of research with living target systems potentially have great *relevance* to important and meaningful human processes such as healing and social influence.
- *Motivation* is heightened in participants in living target experiments.
- Distant mental influence of living systems has a certain *plausibility* that experiments on influence of inanimate systems do not possess.
- Living systems may be particularly *appropriate* as detectors of psi influence.

*Relevance*

The findings of distant mental influence research with living target systems may have important implications for healing and for social influence. If arbitrarily selected physiological processes of living organisms can be influenced mentally in an arbitrary manner in experimental contexts, does this not suggest that, in principle, similar mental influences could be directed to bodily organs, tissues, or cells in a manner favorable to health and well-being? If behavioral tendencies can be influenced in the laboratory, does this not suggest that decisions, behaviors, and social actions could be influenced psychically in everyday life? There are a number of "pathways" through which distant mental influence might bring about healing or other practical effects.

1. Physiological or biochemical processes in one person might be directly influenced by another person. Such an influence could correct an imbalanced or diseased process or could forestall possible medical problems.

2. The mental influence of one person might trigger the self-healing capabilities of another person; the influence might instill in the latter an awareness of a problem, an increased wish to initiate or increase

self-healing, or simply provide an opportunity to engage in more efficient self-healing.

3. The mental influence might provide increased motivation for self-healing through instilling greater feelings of self-worth, increased knowledge of reasons for self-healing or simply an increased awareness that significant others truly desire and expect one's improvement.

4. Conceivably, distant mental influence could remove physical or psychological impediments to the physical healing process.

5. Especially in situations in which two or more decisions have approximately the same likelihood, a distant mental influence could bias the decision-making process toward or away from one of the choices. Such processes as walking or driving patterns, purchasing behavior, voting tendencies, and so on might be influenced in this way. The consequences of such influenced decisions could be trivial or profound.

6. Paradoxically, distant mental influence could be used to instill or strengthen attitudes of self-responsibility or internal locus of control which could minimize the subsequent suggestive influences of others. This would be analogous to hypnotizing someone, then suggesting that he or she could no longer be hypnotized.

7. Distant mental influence conceivably could influence the accessibility of memories, alter perceptions, feelings, or the timing and sequencing of actions; such alterations could, in turn, have trivial or profound individual or social effects.

### *Motivation*

The motivation to succeed in living target distant influence experiments is likely to be greater than would be the case in inanimate target experiments. To many people, living targets themselves would appear more interesting or appealing than inanimate targets. The implications of success might be clearly or dimly perceived, and, provided such implications are not construed as threatening, this knowledge could heighten motivation in subjects and experimenters alike. Motivation would be especially enhanced if the experiment were conducted in a psychic healing context, since the benefits or implications of success would be readily apparent. The perceived relevance of a living target experiment immediately would endow it with meaningfulness and importance and lift it far above its possible consideration as a mere laboratory game or curiosity.

## *Plausibilty*

In discussing the plausibility of living target studies, we may distinguish two types of plausibility: (a) plausibility to the subject and (b) theoretical plausibility.

*Subject plausibility.* All of us have had considerable experience in influencing living systems through ordinary means. We continually influence other people and animals through our words and actions. We are aware that we can influence our muscular movements, breathing, feelings, and perhaps even our autonomic reactions through volition, imagery, and the generation of specific kinds of thoughts. We have heard that bodily control may be enhanced through hypnosis, biofeedback, and meditation. We are familiar with the notion that people can become aware of being stared at by an unseen person and may even have personally experienced this phenomenon. Our families, our friends, and the media have exposed us to the notion of telepathic influence. Given these sorts of experiences, the prospect of mentally influencing another person or an animal in a psi experiment does not seem excessively alien or implausible.

We do not have such a network of supporting experiences in the case of awareness or influence of inanimate objects, and it is not surprising that we would be filled with feelings of confusion, uncertainty, doubt and pessimism when confronted with the task demands of clairvoyance or inanimate target PK experiments. To the extent that attitudes influence psi performance, living target experiments would be expected to have advantages over inanimate target experiments.

*Theoretical plausibility.* To paraphrase George Orwell, all psi tasks are impossible, but some are more impossible than others. The simple exercises of wiggling our fingers or reviving specific memories will convince us that our minds influence our own brains. We have no genuine understanding of how these mind-body interactions come about. They are impossible. Nonetheless, they happen all the time, and their familiarity has pushed far from our awareness any disturbing thoughts or concerns about their impossibility. If my mind can influence my own brain, perhaps it can influence other, similar brains as well, even if those similar brains are outside of my physical body. Stated somewhat differently, if my mind can influence my brain while the latter is inside of my skull, perhaps my mind can continue to influence this neural tissue *or similar biological material* even if the latter is removed and maintained outside of my skull or body. Further, the degree to which my mind can continue to exert its influence on a distant target system may depend upon the similarity of that system to my familiar brain. On the basis of theoretical speculations such as these, distant mental influence of living targets becomes more plausible than the influence of inanimate systems. Could the same process underlie both familiar volitional actions (such as muscular movements and memory constructions) and the less familiar volitional actions that we know as "psychokinesis"?

This idea is not a novel one; it has been advanced on several occasions in the history of psychical research. Louisa Rhine (1970) and John Beloff (1979) presented the idea as it was formulated in the 1940s and 1950s by J. B. Rhine (1943, 1947), Thouless and Wiesner (1946, 1947), and J. C. Eccles (1953). Rhine and Thouless were seeking to understand the place in nature of the PK "force" or process which had just been demonstrated by the freshly reported dice-influence experiments; Eccles was attempting to develop a neurophysiological explanation of the action of the will. D. Scott Rogo (1980) reminded us that the idea of PK as a "force" that normally regulates events within the body had been proposed as early as 1909 by Hereward Carrington. Carlos Alvarado (1981) traced the idea back to 1874 and to Serjeant-at-Law E. W. Cox, the pre-SPR psychical researcher who assisted William Crookes in the latter's investigations of the "psychic force" (a term coined by Cox) exhibited in physical mediumship. The following sampling conveys the flavor of these thoughts on the possible identity of PK and "ordinary" volition.

- John Beloff (1979): . . . can PK be regarded as the extrasomatic (and hence paranormal) extension of what, in ordinary volutional activity, is endosomatic (and hence normal)? (p. 99)
- Evan Harris Walker (1975): . . . the action of the consciousness to secure the collapse of the state vector has the physical consequence of determining the subsequent states of that system in a manner that corresponds to the concept of the 'will'. . . . Since the brain is responding to sensory input from events external to the body, physically the brain is tied to and is thus a part of a larger physical system incorporating the external world. Whichever state the brain goes into, it must be one consistent with the state the external world . . . enters. As a result, specification of the $w_i$ [will] variables can effect a change in the state of both the brain and events external to the body. (p. 8, 9)
- John Eccles (1953): . . . a special property . . . is exhibited by the dynamic patterns of neuronal activity that occur in the cerebral cortex during conscious states, and the hypothesis is developed that the brain by means of this special property enters into liaison with mind,

having the function of a "detector" that has a sensitivity of a different kind and order from that of any physical instrument . . . at any instant the "critically poised neurones" would be the effective detectors and amplifiers of the postulated action of the "will" . . . "will" modifies the spatio-temporal fields of influence" that become effective through this unique detector function of the active cerebral cortex. . . . It will be agreed with Rhine (1948) that, if the so-called psi capacities (psychokinesis and extrasensory perception) exist, they provide evidence of slight and irregular effects which may be similar to the effects which have here been postulated for brain-mind liaison, where they would occur in highly developed form. (p. 267, 275, 277, 284)

- Thouless and Wiesner (1947): We wish to suggest . . . that these [paranormal cognition and psychokinesis] are merely unusual forms of processes which are themselves usual and commonplace, and that in their usual and commonplace form, they are to be found as elements in the normal processes of perception and motor activity. . . . I control the activity of my nervous system (and so indirectly control such activities as the movements of my body and the course of my thinking) by the same means as that by which the successful psychokinetic subject controls the fall of the dice or other object. (p. 195, 197)
- J. B. Rhine (1943): The mind or subjective self in its domination of the body exercises a causal influence which cannot be otherwise than kinetic. Thus psychokinetic action . . . is the basis on which every man interprets his routine experience of daily life. (p. 70)
- Hereward Carrington (1909): Now, if mind exists apart from the brain and merely utilizes it to manifest through, it is acting upon it by a species of telekinesis all the time! Every mental state and change—accompanied, as it doubtless is, by molecular action, chemical changes, etc.—is the result of a telekinetic action! There should be no very great difficulty in imagining consciousness capable of affecting the outaide material world, therefore. (p. 295)
- Sydney Alrutz (1909): What is happening in the brain . . . when we move an arm by means of an act of will? . . . Are these entirely electrical and chemical forces? . . . Might there not be . . . some form of energy more closely allied to the psychic acts, constituting a sort of bridge or transition between psychic phenomena . . . and electrical and chemical phenomena? (cited in Carrington, 1921, p. 114–115)
- F. W. H. Myers (1886, 1903): . . . perhaps when I attend to a thing, or will a thing, I am directing upon my own nervous system actually

> that same force which, when I direct it an another man's nervous system, is the 'vital influence' of mesmerists, or the 'telepathic impact'. . . . (1886, p. 127–128); . . . the telekinetic force . . . is generally . . . a mere extension to a short distance from the sensitive's organism of a small part of his ordinary muscular power. (1903, vol. 2, p. 208)

- E. W. Cox (1874): The theory of *Psychic Force* in itself merely the recognition of the now almost undisputed fact that under certain conditions . . . a Force operates by which . . . action at a distance is caused . . . As the organism is itself moved and directed within its structure by a Force which either is, or is controlled by, the Soul, Spirit, or Mind . . . , it is an equally reasonable conclusion that the Force which causes the notions beyond the limits of the body is the same force that produces notion within the limits of the body. (p. 101)

Honorton and Tremmel (1979) and Varvoglis and McCarthy (1986) have recently begun to develop potentially useful empirical methods for exploring the volition/PK theory.

*Appropriateness*

Living target systems possess a number of characteristics that make them especially susceptible to distant mental influence and hence quite useful as "psi detectors."

*Lability*. Early empirical PK work (see Rush, 1976), the various quantum mechanical and noise-reorganization models of PK (see Oteri, 1975; Puharich, 1979), Stanford's (1978) conformance behavior model, and my own lability/inertia model (Braud, 1981) all predict greater psi influences upon random or labile systems than upon non-random or inert systems. Since living systems possess a great deal of lability or free variability, they would seem to be excellent candidates for sensitive and effective detectors of distant mental influence. I was delighted to find the following unexpected passage in one of Carrington's (1921) volumes. The passage is from a presentation of Sydney Alrutz to the Sixth Psychological Congress which met in Geneva in August, 1909.

> When we wish to study the electrical charge contained in any body, we obtain exactitude only when we succeed in transferring this charge to another body; we may then study the nature of the charge under varying circumstances, and establish the influence of the two charges upon one another. It is only in this way that experimentation becomes truly fertile. Should we not apply the same laws to the phenomena of the nervous system, and institute a similar mode of experiment

> for the nervous energies? Under what conditions can we conceive this transference?
>
> The most natural supposition seems to be that it would occur, if at all, in labile organizations; in those subjects which, according to Janet . . . possess an excessively unstable personality; and whose psychic life is characterized by great suggestibility, by instability, and a certain peculiar mobility. Such individuals are also characterized by the great facility with which the functions vary and react upon one another. Binswanger has said that the nervous system of these individuals is characterized by the variability of the dynamic cortical functions; that is to say, by the fact that the nervous segments of their cerebral cortex present a *melange* of greater or lesser irritability. (p. 115)

It was pleasing to find such an early, yet relatively accurate, statement of the lability idea. The term "lability" is sometimes used in psychology and psychiatry to indicate extreme reactivity or extreme variability of mood or behavior. My own use of "lability" should not imply such an extreme case. I use the term to indicate flexibility, ease of expression, freedom to change, free variability. A system that is excessively active is too "driven" to be modulated in an efficient manner; it is constrained by its own overactivity. Such excessively active systems might be as insusceptible to psi influence as would be excessively sluggish or inert systems.

Perhaps the usage of lability that comes closest to my own is that of I. P. Pavlov, who used the expression in his classification scheme of the "types of nervous systems" of his experimental animals. Pavlov and his coworkers classified their dogs on the basis of three major dimensions: *strength, equilibrium, and mobility.* "Strength" referred to the "working capacity" of the cerebral cells, their resistance to powerful external disruptors and stress, and their resistance to the development of a kind of brain-protecting "transmarginal inhibition" due to excessive stimulation or excessive environmental demands. "Equilibrium" referred to the balance of excitatory and inhibitory tendencies. "Mobility" or "lability" referred to the ease and speed with which behavior and brain processes could shift from one state to another to keep pace with changing environmental demands. A labile nervous system changed rapidly in response to a stimulus and rapidly returned to its prior state upon the removal of the stimulus. A good description of lability might be "speedy appropriateness" of responding (this term was not used by Pavlov). The various combinations of these three dimensions yielded several "types" of nervous systems, and these types were observed to respond quite differently to environmental stimuli, conditioning demands, pharmacological agents, spontaneous stressors, etc. Although developed initially to help understand the varied reactions of dogs in the experimental study of the physiology of "higher nervous activity," the typology concept was subsequently extended to human behavior and to the area of psychopathology. Relevant information may be found in Pavlov (1927, 1957), Gray (1964), Kaplan (1966), Lynn (1966), and Sargant (1957). The Pavlovian typology issue is an exceedingly complex one and, perhaps for that reason, it has suffered unfortunate neglect. Many of the Pavlovian concepts have important similarities to the introversion/extroversion and neuroticism constructs of Hans Eysenck (e.g., Eysenck, 1967a, 1967b). Eysenck's personality theory is, in a way, a blending of the typologies of Ivan Pavlov and of Carl Jung. In view of recent parapsychological interest in introversion/extroversion (e.g., Palmer, 1977) and in the Myers-Briggs Type Indicator, which is, of course, based upon Jung's typology (see Berger, Schechter, & Honorton, 1986; Honorton, Barker, Varvoglis, Berger, & Schecter, 1986; Schmidt & Schlitz, 1988), a careful and systematic exploration of Pavlov's typology in relation to psi influence might prove quite productive.

We have been considering the possible psi-conduciveness of the *physical* lability of target systems. Physical lability is almost inevitably accompanied by *perceived* lability, and this latter factor may have an important *psychological* influence upon an investigation's outcome. In any experiment which provides trial-by-trial feedback to the subject, the subject is necessarily aware of the ongoing changes in the state of the target system. The knowledge that develops during the course of the experiment that the target system *can* change, and, indeed *is* changing, may increase the subject's belief or confidence that a distant influence upon the target system is indeed possible; these attitudinal shifts may, in turn, affect psi scoring. Even in distant influence experiments in which immediate feedback is not provided, the simple *knowledge* that a changing living system is involved may have psi-favorable psychological effects. It would be possible to disentangle the usually confounded effects of physical lability and perceived or known lability through the use of special experimental designs in which these factors are manipulated independently and blindly; however, such investigations have not yet been carried out.

*Living systems as detectors/amplifiers.* There is a tendency to think of *detectors* as inanimate, physical devices that respond to the presence of particular materials or energies. However, living organisms themselves can function as exquisitely sensitive detectors of subtle energies and of extraordinarily low concentrations of materials. In biology and in medicine, biological preparations are sometimes used to detect the presence or amount of a substance for which physical detectors have not yet been developed. Such "bioassays" may continue to be used even after the development of appropriate physical detectors. Bioassays have been, and continue to be, useful in the discovery of various hormones, vitamins, and neurotransmitters. Otto Loewi's (1921) use of heart muscle preparations in the detection of "vagus substance" (later identified as acetylcholine) is one of the best known applications of the bioassay. The technique could be extended to yield a "behavioral bioassay" in which observations of changes in the behavior of intact organisms indicate the presence of some agent or energy. Examples of the behavioral bioassay include observations of changes in the aggressive behavior of Siamese fighting fish in the evaluation of tranquilizing drugs (e.g., Walaszek & Abood, 1956) and observations of changes in web-spinning behaviors in spiders in response to minute quantities of LSD, psilocybin and mescaline (Weckowicz, 1967). There have been claims that behavioral bioassays may be used to detect specific memories; however, those claims remain controversial (Braud, 1970; Braud & Braud, 1972; Smith, 1974; Stewart, 1972; Ungar, Desiderio, & Parr, 1972).

This material is offered as background to the conjecture that *the human brain may function as a bioassay for mind, and living systems may function as bioassays for psi.* This conjecture is not essentially different from the proposals of Eccles (1953), Dobbs (1967), and Walker (1975) that the brain may be an especially sensitive detector of small influences due to its vast number of interconnecting neurons, some of the synapses of which may be poised at critical levels of excitability. A slight influence at the synapse of a single "critically poised" neuron could lead to a cascade of subsequent neuronal firings that could in turn lead to gross behavioral, physiological, or subjective reactions. Thus, the system could function not only as a detector, but also as an *amplifier* of subtle mental influence.

It may be possible to construct complex, interactive inanimate systems whose similarity to the brain might allow them to function as mental influence detectors. As early as 1947, Thouless and Wiesner described the requirements of such a physical system.

> . . . the ideal mechanism for studying [psychokinesis] would be one in which very minute forces could start processes in systems of small size, which processes could act as triggers for subsequent processes involving sufficiently large forces to be easily observable. If indeed a physicist could construct for us a mechanism in which there were delicately balanced systems of very small size, which balance could

> be upset by small forces yet was protected from being upset by small forces accidentally impinging from outside, and if, moreover, any change in these small systems could be automatically magnified to a large energy change in some larger system, then we might hope to have the ideal mechanism for the experimental demonstration of psychokinesis. We have not succeeded in devising such a mechanism in our laboratories. (p. 198)

Today, there exist several physical systems that may indeed satisfy the Thouless and Wiesner requirements. One such system consists of computer-based *neural networks* (see Kelly, 1979; Radin, 1988). Other possibilities would include physical systems of the sort being explored in the new discipline of "chaos" theory and which exhibit unusually sensitive dependence upon initial conditions (see Gleick, 1987; Jantsch, 1980).

*Multiple psi channels.* It is likely that distant mental influence experiments with living target systems will yield better psi results than similar experiments with inanimate targets because of the greater number of "psi channels" available in the former. Living target systems may "cooperate" in bringing about the desired outcome through the aid of their own telepathic or clairvoyant abilities, combined with intentional or unintentional self-regulation. This would seem especially likely when other persons serve as target systems. In a "bio-PK" experiment, for example, in which my aim is to reduce the EDA of Person A during certain periods, I may achieve that goal by means of a direct PK influence upon A's EDA. However, it is also possible that A will "scan" the experimental environment and discern the pattern of activity that I expect of him or her. Person A could become aware of that pattern through telepathic access to my influence attempt or through clairvoyant access to a physical record of the influence/non-influence schedule. Person A could then produce the desired EDA patterns in herself or himself through autonomic self-regulation. Of course, both PK and telepathy/clairvoyance may be occurring at once, or the two forms of psi may alternate throughout the trials of an experiment. For one of these "channels," the locus of the psi is the influencer; for the other channel, the psi locus is the ostensible "target person."

We are planning a blood pressure bio-PK study which may cast light on this issue of the true psi locus in such experiments. We plan to assess subjects' abilities to self-regulate blood pressure in an initial screening phase of the study. In the next phase, all of the screened subjects will serve as target persons in bio-PK sessions in which an influencer will attempt to influence their blood pressures. We are quite interested in learning whether and how the self-regulation and hetero-regulation scores of these two phases are correlated. If successful bio-PK is due primarily to receptive psi plus self-regulation, one would expect that the most successful sessions would involve subjects who are very good at self-regulation; a strong positive correlation between the Phase 1 and Phase 2 scores would be consistent with this hypothesis. On the other hand, if no correlation or a strong negative correlation between Phase 1 and Phase 2 scores obtains, such an outcome would be more difficult to explain on the basis of this hypothesis. A strong negative correlation or no correlation would be of greater interest than a strong positive correlation, since the latter could also be interpreted as merely an indication that the blood pressure activity of certain subjects is more labile and *generally* influenceable than that of other subjects. It is recognized that no experimental outcome will point conclusively to one or the other of these two interpretations of "true psi locus," and my suspicion is that successful bio-PK experiments include elements of both processes, and that the real locus of the effect is in neither the influencer nor the subject, exclusively, but in an interactive field in which both participate.

Another approach to the self-regulation issue would involve experiments with response levels that vary in the ease with which they can be self-regulated. Motor activities and breathing are relatively easy to consciously self-regulate, while certain autonomically mediated functions (such as foot temperature or blood pressure) are more difficult to voluntarily control. Would bio-PK studies involving the former be more successful than those involving the latter? Again, outcomes will not be conclusive because of the problem of possible skeletal or cognitive "artifacts" (see Katkin & Murray, 1968), but would be of interest nonetheless.

A third approach to this issue would involve varying the phylogenetic or ontogenetic status of the target organism, or testing organisms under conditions that would be expected to influence self-regulation ability. What would be the outcones of bio-PK experiments in which one attempts the distant mental influence of skeletal responses in: (a) infants who have not yet manifested a great deal of motoric self-control, or (b) persons in REM sleep in which most motoric activity is inhibited, or (c) organisms that are only distantly related to human beings?

We have been assuming that in distant influence experiments, there are indeed changes in the activities of the living target systems which are produced "causally" or "psychokinetically" by the influencer and which would not occur otherwise. In situations in which the *a priori* probability of a particular target reaction is quite low or in cases in which the reaction is relatively complex, this seems to be a quite reasonable assumption. However, in *statistical* experiments in which the target activity has a relatively high probability of occurring naturally, there arises the additional possibility that the influencer *psychically perceives* the present or future activities of the system and schedules his or her "influence" attempts so that they happen to coincide with the system's activities, thus producing an illusion of a causal effect. The precognizing of "favorable" segments of ongoing random events was suggested many years ago by W. E. Cox (Cox, personal communications; Hansen, 1987) as a possible alternative explanation of most radioactivity-based REG "PK" effects. This interpretation has recently been revived, with the new name "intuitive data sorting" or "intuitive data selection" (see Weiner & Nelson, 1987, pp. 136–144).

It is certainly possible that IDS may play some role in some distant mental influence experiments; however, the extent of such influences remains to be determined. Two recent explicit tests of the IDS hypothesis in our own laboratory (Braud & Schlitz, 1987; Braud, 1988) did not yield results consistent with the IDS explanation.

*Levels of influence.* Another advantage of living target systems in distant mental influence research is that such systems possess multiple "levels" of activity which may be targeted for possible influence. The relative susceptibility of those levels could then be compared. For example, experiments could be designed in which one attempts distance mental influence of the thoughts, images, feelings, behavior, gross physiological activity, molecular physiological activity, biochemical activity, or immunological activity of another person. Would influence attempts be equally successful at those various levels of response? One could even attempt to exert a distant mental influence upon the *psychic* activity of another person.

Experiments could be designed to study the degree of co-variation or dissociation among various levels. What happens at Level X when distant mental influence is directed toward Level Y? Are the outcomes symmetrical or asymmetrical? What happens at Level Y when attempts are made to influence Level X?

*Other influences upon the target.* Living target systems permit the study of several nonpsi factors that might influence the psi susceptibility of the system. I was tempted to say that living systems are susceptible to more influences than are inanimate systems, but this is not necessarily true (see below). It would be more accurate to say that living and nonliving systems are susceptible to *different* nonpsi influences. It would be possible to study the influence of a certain nonpsi factor upon both influencer and target system. Would the factor have the same influence in these two cases? According to certain models of psi functioning (e.g., my lability/inertia model [Braud, 1981]; Roll's "systems theoretical" model [Roll, 1985]), certain variables (e.g., level of arousal, degree of cognitive constraint, etc.) are expected to have opposite effects upon different psi processes or upon systems with different roles in psi interactions. Living target system research can provide a testing ground for some of these ideas.

### *Disadvantages*

The use of living systems in distant mental influence research is not without its disadvantages.

### *Logistical Difficulties*

Experiments with living target systems may be more complicated than inanimate target system experiments in that the target systems themselves require additional scheduling and maintenance. In planning an REG-PK session, one has only to schedule an influencer. In a bio-PK session involving a human target person, one has to schedule two people, and, if one of these person fails to appear at the laboratory, the session must be cancelled or postponed. If the experiment involves animals, plants, or cellular systems, one must have additional facilities for their housing, maintenance and preparation. The experimenter also will have to become familiar with the living system and learn its requirements, sensitivities, habits, preferences, etc. All of this makes life more complicated, but also more interesting, for the experimentalist. And what does one do with one's experimental organisms when the experiments have been completed? The most ideal and most humane solution is to borrow one's target creatures from nature for a while, then return them unharmed when the study has been completed.

### *Manipulation of Life Forms*

One must deal with the ethical issue of whether it is proper to influence the actions of other people or of other life forms. We have worked with over 400 people in our various bio-PK studies, and less than a half-dozen of these expressed any concern about the possibility of influencing or being influenced by another person. If other persons serve as target systems, there would seem to be no ethical problems as long as (a) subjects give informed consent, (b) the planned influences are not deleterious to the target person and (c) there is proper debriefing in which the likelihood and extent of distant influence are placed in a proper context for the subjects. Some formal or informal screening might be useful in order to eliminate subjects who might deal with the issue of distant influence in an imbalanced manner. As in any other experiment, clinical interaction, or everyday life situation, problems may be avoided if one uses good judgment and common sense. In our own work, we have dealt with possible ethical issues by choosing target reactions that are generally beneficial to our subjects. In the use of animals or plants in distant influence experiments, one could choose target reactions or activities which are not harmful to those organisms. In healing analog studies, participants are sometimes asked to destroy "harmful" organisms such as bacteria or cancer cells. The same arguments and considerations used by those who use pharmacological or other treatments to destroy these organisms in other, usually medical, contexts would be relevant here. Cost/benefit analyses, "greater good" judgments, and personal attitudes will govern final decisions.

Those who are troubled by the "manipulative" aspects of distant mental influence studies might consider whether a psychic "command" to make a particular movement really differs from a similar "command" of an agent to a subject to *think about* a particular target in a card guessing, ganzfeld, or remote viewing experiment. The major difference seems to lie in which influence might be considered to have a stronger possible impact upon the external, physical world. To assert that the psychic production of a muscle twitch is more powerful or more coersive or manipulative than the production of a "mere" mental image is to ascribe a greater reality status to the former than to the latter. This is certainly questionable. Images are no less real than are muscular movements and may have even more profound environmental and social consequences under certain conditions.

### *Psi-Missing in Healing Studies*

It is my impression that psi-missing occurs less frequently in living target studies, than in inanimate target studies. Still, psi-missing has been reported in healing analog studies (e.g., Grad, 1967; Wells & Klein, 1972). This possibility should be kept in mind by anyone considering practical applications of distant mental influence; the procedure could backfire. This possibility is not really surprising, since any treatment (e.g., drugs) can have reversed or "paradoxical" effects under certain conditions, and no treatment is entirely without possible negative side effects. It would seem especially important to explore the conditions which tend to produce psi-missing in living target influence experiments so that those conditions could be avoided in practical application attempts.

### *Experimental Control*

It might seem that controlling extraneous variables would be more difficult for living than for inanimate target systems and more difficult for complex organisms than for primitive ones. This is not necessarily true. The extraneous variables are simply different in these cases and not necessarily more or less numerous. One has but to read Hubbard, Bentley, Pasturel, and Isaacs' (1987) account of the development of their monitoring, isolation, and artifact detection systems for piezoelectric strain gauge PK targets to realize how difficult controlling extraneous variables can be in the case of inanimate target systems. In the case of living systems, "higher" organisms can filter or screen themselves or compensate for environmental variables which would exert strong influences upon more "primitive" organisms. For example, a subtle temperature change could have a marked influence upon *in vitro* cellular preparations, while even large temperature fluctuations might go completely unnoticed by human laboratory participants. What is a signal at one level of biological development becomes noise at another level, and adaptive pressures have produced and perfected quite efficient noise-cancelling mechanisms.

### *Statistical Issues*

Possible statistical problems unique to living target distant influence research have been discussed by Solfvin (1984) and by Rush (1986). An issue that has not received adequate treatment is the possible *lack of independence* of repeated measurements of the activity of a biological target organism. In electrodermal bio-PK experiments, for example, external conditions or endogenous rhythms could result in relatively long "bursts" of nonindependent activity or inactivity. If successive "samples" of this activity are treated as independent when they are in fact positively correlated over time, statistical tests (such as *t* tests) that assume independent units would be artificially inflated. We have dealt with this possibility in our EDA bio-PK experiments by not treating the many trials of our sessions as units for statistical analysis, but, rather, have collapsed all of the trial activities into a single score for the subject; i.e., the entire session becomes either a hit or a miss (depending upon whether there was more or less overall activity, respectively, in the prescribed direction in the influence trials, compared with the noninfluence, control trials). This amounts to a "majority vote" procedure which eliminates possible statistical dependency problems, but is extremely wasteful of data. Alternative solutions would be: (a) to attempt to determine empirically the nature of the correlation among the data points and include the value of such a correlation as a correction in computing t scores or (b) to attempt to show that successive target activity measures are in fact independent. If the data can be reduced to binary form, a number of statistical tests for intertrial independence or randomness are available (e.g., Davis & Akers, 1974; Dudewicz & Ralley, 1981). For analog data, correlations of activity with trial number (Utts, personal communication, 1988) or the use of autocorrelation techniques (see Braud, 1988) would be helpful.

Trial dependence is problematical in situations in which the very same target organism participates in all trials of an extended measurement block—e.g., placing a laboratory rat in a test apparatus for 15 minutes and measuring its activity 10 times (i.e., for 10 "trials") during that long period. The problem can be reduced or obviated by using different organisms or different biological samples for the different trials, This is analogous to making activity measurements in 10 different laboratory rats placed sequentially in the measuring apparatus throughout a 15-minute period, all rates being selected or sampled from a common group colony cage. One would still have to be careful to eliminate external or internal factors that could bias subsets of organisms or trials in different directions for experimental and control treatments, respectively.

### *Resistance*

Researchers who explore the distant mental influence of living systems will encounter *resistance* in all of its manifestations. The living target system itself, at a *physiological* level, may resist a distant mental influence, especially if that influence opposes a strong homeostatic tendency. Psi influence attempts may be most successful when they are directed in a manner that would assist the organism's return to a balanced condition. Assisting homeostasis should not be confused with the statistical artifact of regression to the mean, about which Child (1977, 1978) has warned us, and against which experimental precautions should be taken.

Of equal interest are the various forms of *psychological* resistance that may be encountered in the subjects and experimenters of distance influence experiments, as well as in the reactions of one's colleagues and critics. Success at distant mental influence may trigger conscious or unconscious thoughts or feelings about the possible abuse of such "powers" which in turn may activate certain psychological mechanisms of defense against the resultant threatening impulses or fears. These issues have been discussed by Eisenbud (1963, 1972, 1977, 1983), Tart (1984), Braude (1986), Inglis (1981), and Braud (1984). Fears of the possibility of "evil" mental influence may indeed be responsible for the dearth of studies of distant mental influence of human subjects, even in the context of healing or healing analog investigations. It has also struck me as curious that the most explicit treatments of the possibility of harmful psi influences in everyday life, i.e., the theories of the Greek psychical researcher Angelos Tanagras (1949, 1967), have been almost totally ignored by parapsychologists.[7]

The issue of psychological resistance is a complicated one and one for which there would seen to be no easy solution. One method of countering defenses would be the provision of a nonthreatening context for one's distant influence experiments, such as healing or another positive application (see Braud, 1984; Benor, 1985). Another method of dealing with defenses would be to attempt to assess the presence and degree of these defenses in various subjects and experimenters (through use of a specially constructed version of the "Defense Mechanism Test," for example) and to study the manner in which this assessed factor interacts with psi performance or ways in which such defenses might be reduced.

### *A Converging Strategies Approach*

In a classic 1968 *Psychological Review* paper, Stoyva and Kamiya proposed a "converging operations" approach to the study of consciousness and illustrated that strategy in the contexts of the experimental study of dreaming and the waking mental activity associated with EEG alpha control. The strategy utilizes the convergence of different types of indicators (i.e., psychophysiological, behavioral, and verbal) in the definition of a hypothetical construct such as a particular state of consciousness. Recently, Rex Stanford has been using a similar approach in his studies of the psi-conduciveness of ganzfeld stimulation (e.g., Stanford, Kass, & Cutler, 1988). I would like to suggest a multi-component strategy in which converging operations of three kinds may be used in elucidating the problems of "consciousness" and "life." The strategy may be illustrated in the context of psychokinesis. Research would be conducted in three areas in order to determine: (a) whether animate and inanimate systems differ in their *susceptibility* to a PK influence, (b) whether animate and inanimate systems differ in their ability to *produce* psychokinetic effects in other systems (or, better, to produce "conformance behavior" in other systems; see Stanford, 1977, 1978; Edge, 1978; Braud, 1980; Varvoglis, 1986), and (c) whether "pre-observations" of random events by animate versus inanimate systems differentially influence the susceptibility of such events to later psychokinetic influence (see Schmidt, 1984, 1985, 1986, 1987). Ideally, many studies would be carried out in parallel in these three different areas by many different investigators (preferably by investigators with different belief systems regarding the studied phenomena). The studies could be done using a variety of life forms (of different phyletic and ontogenetic status) and a variety of inanimate systems (differing, perhaps, in their degree of complexity and the degree of interconnectedness of their component elements). Similar parallel experiments could be conducted with human influencers, influencees, and pre-observers who are in various states of consciousness during their experimental sessions. Outcomes of these studies that would be of great interest and theoretical importance would be: (a) the discovery of specific graded or discontinuous curves relating outcome likelihood to the life- or consciousness-status of the experimental participants in each of the three research areas and (b) a *similarity* of the three obtained functions. Throughout this endeavor, great care would have to be exercised to assure that comparison tests were carried out under identical psychological conditions, using the proper multiple blinds and design considerations. While findings in any one area would be far from definitive, the *convergence* upon the same conclusion of evidence from three different research domains would be more compelling and could lead ultimately to a true comparative psychology of consciousness or mind.

Let me illustrate the use of this strategy more concretely. Let us suppose that we carry out REG-PK experiments with alert humans, drowsy humans, dolphins, chickens, earthworms, protozoa, plants, complex machines, and simple machines as the ostensible subjects or influencers. In some cases, the REG "hit" feedback would have to be transformed into environmental events that satisfy the organisms' needs or allow the execution of some strong predisposition. It is also important that the various experiments be given sufficiently fair tests; i.e., experiments should not be tried only once or a few times and abandoned prematurely because they "didn't work." Next, we conduct distant influence experiments in which these same respective organisms and devices serve as targets. Finally, we have the respective organisms or devices "pre-observe" REG events before the latter are subsequently displayed as PK targets for a human influencer. Let us suppose that the strength of the PK effect (as assessed by some appropriate standardized measure such as effect size) differs for alert *versus* drowsy human influencers and that a similar functional relationship is found in the case of alert *versus* drowsy human pre-observers. Or, suppose all three effects tend to occur for dolphins, humans, and chickens, but not for earthworms, protozoa, plants, or machines. Such convergent outcomes might point to interesting gradients or discontinuities among the systems which then could be explored more incisively.

### *Other Contexts*

We have been considering distant mental influence as it occurs in the context of quasi-experimental or experimental psi studies. In these studies, attempts are made to study the process *in isolation,* without the possibility of conventional sensory or motor accompaniments. A sufficient number of such experiments have been successful, and have yielded sufficiently impressive results, to lead us to conclude that direct distant mental influences upon living systems are possible. There is, therefore, an even greater likelihood that direct distant mental influences upon living systems may occur frequently and strongly in everyday life situations and may be intertwined with more "conventional" control modalities. We influence others by means of our words, expressions, and actions. We influence our own bodies through various neural and hormonal processes. It is not unreasonable to assume that we also influence other persons or our own physiological functioning through direct psychic means, acting in parallel with more conventional means. Perhaps psychic influences modulate or orchestrate the more familiar physical and chemical processes that support and govern our everyday actions. (K. Ramakrishna Rao explores this very idea in another paper of this volume.)

When we succeed in influencing ("self-regulating") our somatic functioning in contexts of auto-hypnosis, autogenic training, biofeedback, visualization effects, or rehabilitation training to recover or compensate for lost muscular functioning, perhaps we are exerting direct mental ("psychokinetic") influences upon our somatic systems. When we attempt to help restore the mental and physical health and wellbeing of other persons in contexts of medical treatment, nursing care, therapy, counseling, and teaching, perhaps we are exerting direct mental influence upon our patients, clients, and students. Similar suggestions may be found in a previous paper of mine (Braud, 1986) and in the writings of R. A. McConnell (see McConnell, 1983, 1987) and M. K. Muftic (1959). Tanagras (1949, 1967) discusses the possibility of direct psychic influences in mundane contexts, as well as in more exotic contexts involving "the evil eye" and the possible effects upon others of negative thoughts and feelings. A more contemporary treatment of possible interactions of sorcery, psi phenomena and stress among certain Amerindian groups may be found in Lake (1987). On the more positive side, direct mental influence may be implicated in extraordinary athletic or martial arts accomplishments, such as those described by Murphy and White (1978).

*Harmful and Unwanted Influences*

Is it possible to prevent harmful or unwanted distant mental influences, and if so, how can this be done? This is an important issue, and one that is difficult to address adequately because of the absence of necessary research findings. In the various experiments on the distant mental influence of human subjects, which have been discussed in this paper, it might be argued that influence is possible provided the subjects give explicit *or tacit* consent to be influenced. In the various "bio-PK" experiments on physiological influence that we have conducted in our laboratory, the subjects knew the nature of the influences which were to be attempted and agreed to let such influences occur. The experiments could be viewed as social agreements or "contracts" in which the experimenter, the influencer, and the subject all agree to play certain roles having psychic components; each participant plays his or her proper role in order for the experiment to succeed. Allowing one's body to be influenced is part of the task demand with which the subject willingly complies. It is in everyone's best interest for the experiment to succeed. It could be argued that even in experiments in which the subject is conventionally "unaware" that influence attempts will be made, the subject may be *psychically* aware of the possibility, and that there are tacit understandings of appropriate roles and useful outcomes. Perhaps such tacit consent to be influenced and resultant compliance would not occur in situations in which the attempted influence is a deleterious one.

There is actually no compelling evidence that bears directly on the issue of whether external psi influences can produce undesirable effects in a person. If the influences under consideration are direct, causal, "psychokinetic" ones, unwanted influence may have a greater likelihood than if the influences are really unconsciously self-produced and merely aided or triggered by telepathic or clairvoyant knowledge of what actions are expected. To the extent that psi manifestations in a "target person" make use of the images, thoughts, and feelings of that person as "vehicles" for their expression, psi influences could be allowed or prevented through the use of the same self-control techniques by which the target person customarily modulates his or her own thoughts, images, feelings and behaviors in more conventional contexts. Processes of intention and acts of will should be just as effective in the psi realm as they are in more ordinary domains.

Psychological and, possibly, psychic techniques could be used to prevent unwanted influences. These techniques involve reminders of self-control, self-responsibility, internal locus of control, and ultimate "veto power" over what one does upon the suggestion of others. Confidence enhancing images of barriers, screens, shields, or other symbols of protection might be used to effectively block unwanted psi influences. We have used such techniques, with initial indications of success, in some of our EDA bio-PK experiments. More extensive studies of "psi blocking" techniques in other contexts are still in progress, and we hope to report their results soon.

The demand characteristics of laboratory experiments make difficult or impossible any final resolution of the issue of whether unwanted or harmful psi influences can ever really occur. The situation is similar to the one that obtains in hypnosis or compliance research: Subjects may, at some level, recognize that experimenters would not allow anything that is truly harmful to occur; i.e., subjects may discern that some experiments may be dramatic instances of play-acting designed to prove a particular point of view of the investigators. Ethical constraints would not allow more realistic experiments or tests in everyday life that are not "play-acting."

Perhaps the most valid evidence bearing on this issue will come from careful anthropological observations in natural settings. Even here, however, certain complexities and alternative interpretations will remain. For example, consider a well-authenticated case in which Person X becomes seriously ill or even dies shortly after being "hexed" or "cursed", *without his or her knowledge*, by Person Y. How could we exclude the possibilities that (a) Person Y precognized Person X's illness or death and then engineered the ostensible "magical" influence in order to convince others of unusual causal powers that Person Y really does not possess, or (b) that Person X actually injured himself or herself as self-punishment for some real or imagined crime, sin, or taboo-violation—i.e., that Person X committed a sort of socially approved suicide, using psi-provided knowledge of an actually ineffective "curse" as an opportunity for this action? Are these reasonable alternative explanations, or are they continuing manifestations of psychological defense against the possibility of truly causal external psi influences of a harmful nature? Could our hope to assign a definite form and definite locus to these effects be a misguided one? Perhaps the most satisfactory interpretation will be one in which psi influence effects are understood as field-like effects contributed by *all* participants and involving several "forms" of psi.

The complexity of designing research or of interpreting findings relevant to this issue soon becomes apparent. It becomes more understandable why so few researchers have grappled with the issue of possible harmful or unwanted psi influence effects.

## *Findings*

I have presented various considerations which favor the use of living target systems in distant mental influence research. I will conclude with a brief summary of some of the findings and conclusions that are emerging from our work with living target systems in our Mind Science Foundation laboratories.

1. Based upon overall statistical results, the distant mental influence effects are relatively reliable and robust.

2. The magnitudes of the effects are not trivial and, under certain conditions, may compare favorably with the magnitudes of self-regulation effects.

3. The ability to manifest the effect is apparently widely distributed in the population. Sensitivity to the effect appears to be normally distributed in the volunteer subjects who have participated in our various experiments. Many persons are able to produce the effect, with varying degrees of success, including unselected volunteers attempting it for the first time. More practiced individuals seen able to produce the effect more consistently. There are indications of improvement with practice in some influencers.

4. The effect can occur at a distance, typically 20 meters; greater distances have not yet been explored.

5. Subjects with a greater need to be influenced (i.e., those for whom the influence is more beneficial) seem more susceptible to the effect.

6. Immediate, trial-by-trial analog sensory feedback is not essential to the occurrence of the effect; intention and visualization of the desired outcome is effective.

7. The effect can occur without the subject's knowledge that such an influence is being attempted.

8. It may be possible for the subject to block or prevent an unwanted influence upon his or her own physiological activity; psychological shielding strategies in which one visualizes protective surrounding shields, screens, or barriers may be effective.

9. Generally, our volunteer participants have not evidenced concern over the idea of influencing or being influenced by another person.

10. The effect can be intentionally focused or restricted to one of a number of physiological measures; it may also take the form of a generalized influence of several measures, if that is the intent of the influencer.

11. A number of target systems have been found to be susceptible to the effect, including the spatial orientation of fish, the locomotor activity of small mammals, the autonomic nervous system activity of another person, the muscular tremor and ideomotor reactions of another person, the mental imagery of another person, and the rate of hemolysis of human red blood cells *in vitro*.

12. The living target systems can be influenced bi-directionally; i.e., their activity levels can be either increased or decreased.

13. The activity levels of at least some of the target systems (i.e., electrodermal activity, rate of hemolysis) and their susceptibility to distant mental influence appear to be influenced by geomagnetic field (GMF) activity; i.e., the systems are more active and more susceptible to influence when the earth's geomagnetic field activity is more "stormy" than during more "quiet" GMF periods.

14. Distant mental influence, in the expected direction, seems more successful when the intentions and images of the influencer are focused *specifically on the desired target activity*, rather than directed toward the target in a more general or global manner.

15. The effect does not always occur. The reasons for the absence of a significant effect in some experiments of a series which is otherwise successful are not clear. We suspect that the likelihood of a successful distant mental influence effect may depend upon the presence of certain psychological conditions, in both influencer and subject (and perhaps even in the experimenter), which are not always present. Possible success-enhancing factors may include belief, confidence, positive expectation, and appropriate motivation. Possible success-hindering factors may include boredom, absence of spontaneity, poor mood of influencer or subject, poor interactions or poor rapport between influencer and subject, and excessive egocentric effort (excessive pressure or striving to succeed) on the part of participants. We suspect that the effect occurs most readily in subjects whose nervous systems are relatively labile (i.e., characterized by free variability) and are momentarily free from external and internal constraints. Perhaps fullness of intention and intensity or vividness of visualization in the influencer facilitate the effect. Additional research, of course, is needed to determine the validity of these conclusions and to explore more thoroughly the various physi-

ological and psychological factors that are favorable or antagonistic to the occurrence of the effect.

We are continuing our laboratory studies of distant mental influence of living systems, being especially interested in exploring the possible limits of such effects and whether the effects can be *strong* and *consistent* enough to yield possible practical applications (e.g., in the area of healing). We hope that this presentation will encourage others to carry out similar investigations.

## ENDNOTES

1. Julian Isaacs (1983) has argued that physical systems with very low spontaneous activity levels would be ideally suited to the direct detection of subtle PK effects.

2. It was Elliotson who introduced the stethoscope into hospital practice.

3. Inspired by these sorts of reports (especially those of Townshend), Edgar Allan Poe featured distant mesmeric influences in two of his short stories, "A Tale of the Ragged Mountains" (1844) and "The Facts in the Case of M. Valdemar" (1845). Distant mesmeric influence also was featured in Robert Browning's poem, "Mesmerism," written during this same time period (see Schneck, 1956).

4. It is not generally known that, in 1889, Charles Richet published a sensational novel, *Sister Marthe*, which featured hypnosis and dual personality; he published the novel under the pseudonym, Charles Epheyre.

5. It was Bechterev, of course, who pioneered the learning paradigm which later came to be known as "instrumental" or "operant" conditioning, and which was later so well explored and exploited by B. F. Skinner and his coworkers; Ivanov-Smolensky specialized in "semantic" conditioning and his investigations are important in the understanding of the Pavlovian "second signaling system" (i.e., the experimental study of language and thinking).

6. Loewi's findings were not immediately accepted because of the difficulties encountered by other investigators in replicating his work. The vagus nerve of the frog also contains a sympathetic accelerating component. The nerve's action is therefore mixed, sometimes accelerating the heart and sometimes decelerating it. Which particular action predominates depends upon the frog and varies with the season of the year. Opposite seasonal variations occur in the toad. Eventually, when these initially occult interactions were realized, Loewi's discoveries were confirmed and are now universally accepted (see Goodman & Gilman, 1956). Perhaps these events will provide encouragement to psi researchers who continue to experience difficulties in their replication attempts.

7. I have been able to find reference to Tanagras' work in the writings of only one psychical researcher, Jule Eisenbud. Tanagras' name does not appear in the indices of the major parapsychological reference works. The one exception is Wolman's (1977) *Handbook*, which gives a single page reference to Tanagras and this is merely to the definition of his term "psychoboly" in the glossary at the end of the volume; interestingly, the page reference is incorrect, and Tanagras is not to be found even on the single page for which he is referenced. Could this be still another indication of psychological resistance to the possibility of powerful negative psi influences?

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*DISCUSSION*

EDGE: I have been very impressed with your work over the years, but I have to admit in the last 25 minutes a bit of skepticism has come in. Richard Broughton and I were sitting here trying to get you to forget your paper, but obviously it does not work so I do not know if it says something about the mental influence on biological systems or about Richard and me.

BRAUD: I have also been doing some secret research on psi blocking that would cover something like that one.

MORRIS: My question is in part about blocking. Any one of us in the middle of a major city probably is within a couple of miles of quite a few people being tormented in many ways. If this information is available to us presumably we have evolved few mechanisms for filtering that information out and for dealing with it. I am wondering whether, in the course of setting up a research study, you may have to deal with a fair amount of potential concern and anxiety on the part of subjects that the awakening of psychic skills may make them susceptible to deliberate intentional manipulation by others, or even just to the daily cares of the world. Also, some of what you talked about may relate to the popular notions of a sense of presence or the sense of picking up good and bad "vibes" from people.

BRAUD: We have done experiments with different kinds of biological systems and I have emphasized only the electrodermal work because there is more of that. We have worked with between 300 and 400 people throughout the years. I was trying to count the number of people who expressed concern either about influencing another person or about being influenced. There were less than a half dozen who verbalized this. Now of course a lot of people may not have verbalized their concerns. My own feeling about all of this is that we have veto power over these kinds of influences. I think there is a very real question about whether harmful kinds of influences impinge upon you if you do not want them to. If you consider this kind of neutral tool, then it makes sense that there could be negative as well as positive influences. It might well be, however, that we have some kind of veto power over the kinds of psychic intrusions that we would allow to affect us. Only if we agree to be sensitive to these would reactions be shown. I think that when the experiments that we have done involve knowledgeable subjects, there is an agreement that I will play the role of an influencer and you will play the role of an influencee and we will all agree that there are certain things that we should do for the experiment as a whole to work out. I think we play those roles and the experiment does succeed. If there were something negative embedded anywhere in that agreement, perhaps those roles would not be played. Even in cases of unknowing influencees perhaps there is some kind of psychic discernment that a game is being played in the laboratory and if the net result is a positive one then we will allow ourselves to be sensitive to these influences. So it is very difficult to know whether these things are even possible. These unwanted influences are extremely difficult to study in the laboratory, or even outside, for ethical constraints and for reasons of demand characteristics. An experimenter would not do anything that is really negative ethically and perhaps a subject can discern that and play along with you. It is similar to what happens in hypnosis experiments where you ask someone to throw acid into someone's face when in reality it is distilled water. Someone knows that and he pretends that he is doing something against his will, when actually he knows what is going on. I think that there is a possibility of negative influences. We can begin to do blocking studies to help people realize that they can choose to follow these distant mental suggestions or not. We can give them some relief. Things are not as threatening if blocking skills are available.

PALMER: I thought you made a very good case for this kind of research. Certainly not too many other labs, if any, are currently involved in it. Take a laboratory like ours, for example, which is not really tooled up now to do this sort of work—what is involved from a practical standpoint in doing it? Is it something that you could do with a reasonably meager budget or would it require a big NSF grant? Do you have to have M.D.s involved? In other words, could you give us some sense of what is involved if a laboratory did want to be able to do this well?

BRAUD: These experiments are quite simple and can be done on very small budgets or you can spend $250,000 if you wish to. One could put a gerbil in an activity wheel and have a micro-switch counting revolutions. That would be one extreme—just a simple animal activity wheel or an electric fish in a tank as we did in some of our early experiments. Very simple apparatus that may be found in almost any laboratory could be used to measure the activity of living systems. Tremor is one that we have used. Simply have a person hold a metal stylus, which would cost a nickel or so, in a hole in a piece of metal and record the number of touches that the person makes spontaneously. At a distance you could attempt to make this person more steady or less steady. So at one extreme you could do experiments involving motor reactions that are quite simple in terms of instrumentation. For autonomic activity, again rather elementary equipment such as strip chart recorders could be used (for visual assessment) or you could interface autonomic detectors with any computer to have a more objective recording. The work we have done with blood cells of course is more complicated. You have to bring in physicians to approve your work, and you have to bring in registered nurses to draw the blood. Again there are some experiments, such as the hemolysis work that we did, that would cost perhaps $2,000 for a spectrophotometer to measure light coming though cells and, in addition, a computer to evaluate that. Expenses would be minimal and almost any laboratory could do these experiments. One of the reasons I am presenting this paper is to try to urge other labs to repeat some of this work, because I think it does work very well and it has some interesting implications. It attracts subjects and it is something that can be done rather easily.

MAY: Just a quick question, William. You probably are familiar with some of the early work of Zoltan Vassy in Hungary where he did an experiment involving mild electric stimuli and what he called a remote Pavlov conditioning experiment. He saw galvanic skin responses to remote light flashing down from a hall, but he did it under a paradigm very similar to Pavlov's style. Since you seem to favor the Pavlov model, are you planning any kind of direct sort of conditioning experiments along those lines? I thought his experiments were particularly clever.

BRAUD: We are not planning any conditioning experiments. I am interested in the Pavlovian model primarily in terms of the individual difference aspect, the ways of classifying subjects. The problem is that to do good conditioning you need some good unconditional stimuli. The best one is electric shock, of course; another is loud noise. Now to inflict shocks or loud noises upon subjects is not a very nice thing to do. To get a good conditional reflex established in the first place would require things that we would prefer not to do.

MAY: I actually was a token subject in Zollie's experiment in Hungary. The paper said a mild electric stimulus was administered to the left hand of the subject. I would like to personally inform you that it was more like testing the electric chair in Sing Sing prison. My fingers were smoldering and it hurt like hell.

ROLL: I too appreciated your paper very much as, indeed, I have always admired your work. I wonder if you could free associate on a related topic. You mentioned the significance of homeostasis for living beings and that we tend to move toward levels of homeostasis. Some of the greatest insights both medical research and other types of research have come from systems that are out of balance. My own explorations have mainly been in parapsychological situations where families or individuals are out of balance. Have you any thoughts on how to learn from those situations? Are there ways of exploring that within a laboratory without causing discomfort to anyone?

BRAUD: I think that might be the best place in which to study these influences. If the system is out of balance there is some predisposition in that system to return to balance. What you might do is deliberately target the direction of your influence to help the system return. In Bernard Grad's experiments, for example, when he was attempting to change the growth rate of plants, he found that these experiments did not work unless the plants were unhealthy and he actually used saline so that their growth conditions were not optimal. Under these kinds of conditions the system itself does not quite return to balance. This is an excellent place in which you can reach in psychically and add that little bit to help the system return to a balanced state. It would bother me a bit to try to move a system deliberately out of balance in order to return it. However, if there is a naturally occurring system that is already at one of the extremes, then you could try to help it return or perhaps bring it into your laboratory and use that extreme reaction as a kind of need within the system to help motivate return, both on the part of the system and on the part of the influencer. You as an investigator are doing the system some good by helping it return to this balanced state. I did not mean that we should study systems that are in homeostasis, but that homeostasis is everywhere and we should make use of this tendency by helping homeostasis psychically.

SCHOUTEN: Your paper deals with effects on living systems but I think it is good to realize that basically we are dealing here with very small effects. To extrapolate the discussion into the possibility that people could influence other people with bad consequences and to start talking about blocking research, I think that is really a big step. If some person in this hotel increases the humidity level here in the building, it probably has a much bigger effect on all our systems than the supposed PK effect. So let us first see how far it gets us, how big the effects are and whether we really can control them before we start dealing with questions of how harmful it might be. My next point is that you have linked this to paranormal healing, to healing effects and that is something I object to. In parapsychology we start with paranormal experiences. What happens is that people assume from these that there should be psi processes and so they happily work on studying psi processes. The same happens with paranormal healing. People immediately jump to the conclusion that it should be based on a PK process or distance influence and so they happily start running experiments based on that model. I disagree with that. I think when you are interested in paranormal healing you should start studying paranormal healing itself and based on the results decide what models apply to that situation. My next point in this connection is that I always have difficulty in understanding the assumed PK effect in paranormal healing. Now, that is a complicated matter. You mentioned hypertension. Suppose I would be able to influence a person by PK. How do I lower the hypertension of that person? It is a disease which has to do with the whole circulatory system. I really would not know what to effect. Hypertension is a very complicated problem. Yet in healing it is assumed that ignorant people suddenly know how to affect the system by PK in such a way that it turns out to be in the right direction. Why I also object to this connection between PK and paranormal healing is that there are an enormous number of different types of alternative healing and paranormal healing is only one of the very many. If you look into history you know there have been an unbelievable number of rituals which have been applied to healing and the funny thing is that most of them worked to a certain extent. Because all these different rituals apparently work to a certain extent it would not be a wise approach to take them all literally and argue that if somebody is healing a person for instance by giving him specific stones or minerals, let us study these minerals as they affect the person. I think what happens basically is that all these rituals apparently have a sort of common effect on the healing system. Perhaps the method in itself contributes a little, but I would say look first for the common factor. That is not PK, that is something different. I think we can say that all the studies I know of point to the fact that psychological factors, whatever that might mean, have a much stronger effect on healing than so-called PK or paranormal factors. I really think that we should be careful in linking healing and PK influences with each other.

BRAUD: First of all, I deliberately called my paper "Distant Mental Influence" because I did not want to tie it to PK. I might have slipped into the PK jargon in the presentation, but in the paper I certainly recognize that psychokinesis is only one pathway of these influences. Even at the beginning of this presentation I mentioned that there are other avenues of influence. I might simply by my attention to you, psychically or otherwise, allow you to activate your own healing abilities or I might do some psychological work psychically. So I am not tying myself to a PK, a direct causal connection, as the only mode of paranormal distant influence. As for the weakness of the effect, of course I agree, I appreciate that point. We do not know how strong these effects might be. That is one of the intents of our future research. We would like to know what are limits of influence, how strong, how persistent these effects can be. Can they be strong enough to be physiologically useful? The only thing I can say at this point is that in one experiment we did a direct comparison between someone's ability to calm someone at a distance and a person's own ability to calm himself or herself using self-regulation strategies. The two effects were about the same in magnitude. So at least under some conditions these are not trivial effects. Now we have had very dramatic effects in experiments and in some cases very small ones. Again it is an empirical question to see how strong these could be. I think it might be a mistake for us to pretend that they are small and inapplicable and so not worry about their possible misuse. It might be good to prepare ourselves in advance in terms of blocking strategies so that we will not be surprised if the effects turn out to be rather strong. In terms of influencing hypertension, we suspect that psychic effects in general and PK in particular are goal-oriented. It is not necessary for us to know which buttons and levers within the cardiovascular system to push or pull to bring about an effect. Perhaps there are just one or two cells within the hypothalamus that could be crucial to influencing blood pressure or any of a number of symptomatologies. There may or may not be much complexity involved but I would hope that the system or the process is wise enough to take care of itself, just as I can increase my blood pressure right now by thinking certain things or by simply willing it to change. Perhaps that same kind of global goal-oriented wish or intention could be effective in healing. I suppose it is a matter of temperament. I think that psychokinesis could play a role in healing. That is open to experiments and debate. It could vary from very small to very large, but I certainly think it has some role and we can explore that.

RAO: William, I thought you made a very persuasive case for extending your PK studies on living systems. But when you were responding to Sybo and made a distinction between PK and your distant mental influence, I got a little bit worried about the very concept of distant mental influence. This it seems to me has some problems. First, you are constraining the mind. How do you know that distance has a constraining influence on the mind? Our minds, if they exist, may do so in a kind of framework where distance does not make any difference. Second, how about time? You could have influence when the target is remote in time instead of distance or both. And finally you yourself mentioned, that one could use this to influence oneself, in which case distance becomes a not very significant factor. Consequently I find that the basic concept of distant mental influence is somewhat misleading. If all that you want is a barrier, remote mental influence might be more appropriate because it is applicable for time as well. It fits also within your own system with the elements of blocking. So I take some issue with your concept of distant mental influence.

BRAUD: True. As I look at distant mental influence to me that is an oxymoron because my own understanding of mind would not restrict mind to a particular location. By distant mental influence I did not mean that the mind was working at a distance necessarily, but that the effect is occurring at a distance from the physical body of the influencer. "Remote" as opposed to "distant" might have some advantages. I have a section in the full paper in which I address the terminology issue. I settled upon this one because I thought it was the most general and the one that had the least surplus meaning. You can consult the paper for a number of different terms that could be used. The problem with all the terms is that there are some exceptions to them. When I first began this research I called it "allobiofeedback" because it was a biofeedback influence of another person, a kind of open loop biofeedback. Feedback is not necessary for it to occur; images alone can produce the effect. So I switched to transpersonal imagery effects which are effects of my imagery beyond my own person. It turns out that imagery is not necessary for the effect, that intention alone may suffice. So I suggested "transpersonal imagery and intentionality effects"—a very cumbersome term. So I thought I would return to the old "distant mental influence" term which would cover all categories. "Remote mental influence" would seem to be an even better term, as you suggest.

# ESP RESEARCH AND INTERNAL ATTENTION STATES: SHARPENING THE TOOLS OF THE TRADE

REX G. STANFORD

Circumstances that favor what Honorton (1977) has called "internal attention states"—circumstances such as ganzfeld and hypnotic induction—have provided some of the most replicable laboratory evidence of extrasensory function (see, for example, Honorton, 1985; Schechter, 1984). Nevertheless, the current evidence fails to establish even that it is the presence of an internal attention state (IAS) in such settings that favors ESP-task success or whether that success is due to something else that is provided by those circumstances (perhaps, for example, expectancy of success or reduction of ego-involvement with the task,). The current evidence is certainly inadequate to establish either that it is an IAS per se that is important to success or that the IAS favors success for particular reasons in such settings (Stanford, 1987a). In the paper just cited, I tried to show that this equivocal state of the evidence should not be a cause for pessimism, rather, that it is precisely what should be expected, given the paucity of any substantial body of methodologically adequate experimental work addressing such problems. There is little reason to suppose, in other words, that the present lack of clear knowledge in this area is because of some kind of intractability of the subject matter being investigated.

That is not, however, to say that productive, replicable, high-quality work in such areas will be easy. There is no doubt that outside parapsychology, research areas such as hypnosis have seen more than their share of controversy and that conceptual disputes about what happens during "hypnosis" still rage in the pages of psychological journals. What is more, the interpretation of particular research findings in the hypnosis suggestibility area is regularly mooted in the pages of nonparapsychological journals. (Some examples of such controversy will be noted and discussed below.) If psychologists are themselves unclear about what is really happening when, say, an individual is "hypnotized" or about what factors are most important in contributing to hypnotizability, perhaps parapsychologists can feel less abashed about not having made quantum leaps in their work using tools, such as hypnotic induction, that could be said to involve "internal attention states."

However, the purpose of the present discussion is not to provide parapsychologists with a warm, comfortable glow about the present lack of understanding of ESP-task success in ganzfeld and following a hypnotic induction. It is, rather, to suggest ways in which parapsychological researchers might take responsibility for insuring that the present state of ignorance does not persist, either with regard to interpreting ESP-task success in such settings or with respect to understanding the psychology of such settings. The latter is, in my own view, a necessary part of the discussion because (a) psychologists researching such areas might profit by some help and (b) without advances in the psychological knowledge of such areas there is little prospect that parapsychologists will be able to advance in understanding ESP-task performance in ganzfeld or following a hypnotic induction.

First it will be useful to consider what conventional psychology might have to offer the investigator of ESP-task performance in settings such as ganzfeld and hypnosis. Which particular such borrowed methodologies might be of real benefit will obviously depend not only upon the particular objectives of the study to be undertaken, but, more specifically, upon what theoretical concepts are guiding the study.

### *Hypnosis and Other Internal-States Areas: What Has Psychology to Offer?*

*Measures of Hypnotic Susceptibility (Hypnotizability).* Honorton (Honorton, 1972; Honorton, & Krippner, 1969; Honorton & Stump, 1969) was one of the first parapsychologists to recognize that if one is to attempt to boost ESP-task performance through the use of hypnosis, one must recognize that merely going through a hypnotic-induction ritual with a subject does nothing to guarantee that the subject has been hypnotized or has, responded favorably to the procedure. Consequently, the use of standard scales of hypnotic susceptibility is highly desirable in order to assess the degree to which individuals are hypnotically responsive and, perhaps, to assess their levels of proficiency at several of the tasks that can be tried with hypnotized subjects. This same theme was reiterated by Tart (1980) who made a number of specific recommendations in this regard. [A number of important methodological issues are mentioned and briefly discussed in the Honorton and Krippner (1969) paper; it deserves re-examination by contemporary parapsychologists.]

For preliminary group screening Tart (1980) recommended the Harvard Group Scale of Hypnotic Susceptibility, Form A (HGSHS:A) (Shor & E. C. Orne, 1962), a scale modeled after the individually administered Stanford Hypnotic Susceptibility Scale, Form A (SHSS:A, Weitzenhoffer & Hilgard, 1959), and which, like it, emphasizes motor-response items. However, objective response to the HGSHS:A is judged by subjects themselves after the session is completed. The HGSHS is certainly an adequate preliminary screening tool; indeed, it is almost certainly the most widely used such instrument in the contemporary literature. [Investigators wishing to use the HGSHS:A should, however, consult a study by Kihlstrom and Register (1984) indicating the importance of including reversibility of suggested posthypnotic amnesia as the criterion for scoring posthypnotic amnesia. Their report also discusses scoring criteria for this item.] The HGSHS:A should not, however, be the single instrument used if one is interested in locating hypnotic virtuosi; it simply does not do well in predicting who will perform very outstandingly on an individually administered, more advanced test such as the SHSS:C (Register & Kihlstrom, 1986). These authors also provide evidence that the ability to identify highly susceptible individuals (such as can be identified by the SHSS:C) through the use of the HGSHS:A might be enhanced—if one is in the undesirable position of using only the HGSHS:A—by using measures of subjective response to the test, not just behavioral response reports.

Tart (1980) has properly suggested that for adequate, in-depth study of hypnotic susceptibility as a factor in psi-task performance, additional evaluation is needed beyond the HGSHS—as well as subjects becoming accustomed to the laboratory hypnosis setting and to hypnosis itself. For such purposes, the use of individually administered scale(s) is desirable, including the SHSS:A, SHSS:B (intended as a parallel form for SHSS:A, in order to allow retest studies), and SHSS:C (a nonparallel form that, much more strongly than either A or B, emphasizes cognitive-perceptual items). Items included in C that are not included in A concern taste hallucinations, dreaming within hypnosis, age regression, anosmia for household ammonia, response to a hallucinated voice, and a negative hallucination (not seeing something actually present) (Hilgard, 1977; Weitzenhoffer & Hilgard, 1962).

Tart does not explicitly say so, at least in the published abstract of his convention presentation cited above (1980), but an advantage in using a scale such as SHSS:C with its advanced list of items is that one is able to identify persons who definitely have the cognitive skills (and inclinations) necessary for cognitive-perceptual effects in the hypnotic context. As a consequence, they are individuals likely to exhibit what

Weitzenhoffer (1974, 1980) has called the "classical suggestion effect," that of experiencing response to suggestions, including motoric ones, as involuntary, as happening "on their own" in response to suggestions, rather than being "made to happen" by the subject in order to comply with the hypnotist's suggestions. (For evidence that strong imaginal [cognitive-perceptual] involvement in suggested effects favors involuntariness see Spanos, 1971; Spanos & Barber, 1972; Spanos, Rivers, & Ross, 1977; Spanos, Spillane, & McPeake, 1976; but see Spanos & McPeake, 1977 for a failure at conceptual replication which, however, probably had an adequate explanation. There is also considerable further evidence to this effect that will not be cited here.)

For hypnosis-ESP (or -PK) work, there may be a distinct advantage in utilizing subjects who have the cognitive capacities to make experientially real to themselves the things that the hypnotist suggests in the study (subjective response) and who can experience (or interpret) those suggested events as involuntary (involuntariness of response. The experience or attribution (interpretation) of involuntariness (combined, of course, with a sense of subjective reality of effects) may be associated with a feeling that one is in a special state, under the hypnotist's benevolent aegis, in which things tend to happen just because they are suggested. If so, this almost magical attributional framework might favor psi events. The rationale for this hypothesis is as follows.

Elsewhere, I have theorized and provided evidence to support the claim that freedom from egocentric involvement with the task favors psi events (Stanford, 1974, 1977a, 1977b), and work on spontaneity and ESP-task performance suggests, at least indirectly, that freedom from the need to manage one's responses on an ESP task allows the spontaneity that favors psi-mediated response. (See Stanford, 1975, 1987a for relevant reviews; also, Stanford, Kass, & Cutler, 1988b, for a recent study of this effect in ganzfeld.) Consequently, the use of subjects in hypnosis-psi work who can develop a solid sense of involuntariness might be important. It might mean, in essence, that they adopt a more passive, less egocentric, less managing, more "let-it-happen" attitude toward the task. The result could be a heightened spontaneity that favors ESP-task success. This is an idea well worth examining, though, to my knowledge, it has not so far been examined in the hypnosis-ESP literature.

The use of scales such as the SHSS: A and B is for prior selection or evaluation of general hypnotic ability. The SHSS:C is appropriate for a more advanced level of screening. The still more advanced scales, namely the Stanford Profile Scales of Hypnotic Susceptibility: Forms I and II (SPSHS: I and II), can be used to identify subjects with particular hypnotic skills (or even patterns of those skills) that might be relevant to one's research. If, for example, one wished to use positive hallucinations as a mediating vehicle for extrasensory information, one might wish to use the SPSHS with special emphasis on its subscale HP (Hallucinations: positive). The SPSHS includes subscales related to (a) agnosia and cognitive distortions; (b) positive hallucinations; (c) negative hallucinations; (d) dreaming and re-experiencing past events; (e) response to posthypnotic suggestions (and evaluations of amnesia concerning such suggestions); and (f) various motor items (including, but not limited to, items involving challenge to start motion inhibited by suggestion) (Weitzenhoffer & Hilgard, 1967). Selection of subjects with high scores on any advanced scale (SHSS:C or SPSHS:I or II) could reasonably be expected to provide individuals who would experience involuntariness. The same might be said with less certainty for scales such as SHSS:A and B. However, none of these scales, of itself, measures involuntariness.

The Carleton University Responsiveness to Suggestion Scale (CURSS, pronounced, believe it or not, "curse") does not have this drawback. It is a standard scale that provides seven suggestibility items (tests). There are two motor items (arm levitation and arms moving apart), two motoric challenge items (arm rigidity and arm immobility), and three cognitive items (auditory hallucination, visual hallucination, and amnesia). Based upon these items, three types of suggestibility scores are generated for each subject. There is an objective score (CURSS:O) based upon the total number of items for which behavioral response occurred that met the specified criterion. The subjective score (CURSS:S) is based upon having the subject rate on a scale of from 0 to 3 how much he or she experienced the thing suggested by each item; then these ratings are summed across items. The objective-involuntariness score (CURSS:OI) is derived by assigning for behaviorally passed items one point for each such item for which the subject also rated involuntariness in the upper half (moderate or great involuntariness) of the 4-point involuntariness scale. (For details, including normative data and various psychometric properties, see Spanos, Radtke, Hodgins, Stam, & Bertrand, 1983.) CURSS:O scores have a bell-shaped distribution, but CURSS:OI scores are quite positively skewed (more or less like a backward J). Sometimes the CURSS is scored for voluntary cooperation (CURSS:VC), in which case a point is assigned for each behaviorally passed item on which the subject reports involuntariness in the lower half of the 4-point scale. CURSS:VC scores reflect the number of scores passed for which the subject felt the response was relatively voluntary; such scores are not suggestibility scores. (For details see Spanos, Radtke, Hodgins, Bertrand, Stam, & Dubreuil, 1983; Spanos, Radtke, Hodgins, Bertrand, Stam, & Moretti, 1983.)

The CURSS is usually administered following a standard 5-minute hypnotic-induction ritual, but it can easily be administered as a suggestibility test (i.e., no induction prior to the suggestions) because the wording of its items does not imply anything about hypnosis or having been hypnotized. (Of course, some introduction is needed to explain to the subject something about what is happening.) Scores on all three CURSS dimensions (O, S, and OI) have reasonable temporal stability, correlate moderately with other standard scales (such as SHSS:C), and correlate with the same psychological predictors as other standard scales (e.g., with Tellegen's Absorption Scale, with measures of expectancy and attitude toward hypnosis, and with Field's "hypnotic experiences" inventory). The CURSS has often been administered as a group test with subjects scoring themselves on the objective dimension, as well as rating both subjective experience and involuntariness. (The exception here is that in objective scoring an experimenter scores the amnesia item, which requires reversibility of suggestion-induced "amnesia" for passing.)

With group administration, as in the report on psychometric properties of the test, CURSS:O yields, following varimax rotation, a two-factor structure (built largely around nonchallenge and challenge items), but a one-factor structure holds for CURSS:S and CURSS:OI. Spanos and colleagues accordingly suggest that there is a single, underlying subjective dimension in such measures. They further state that their results with the three types of CURSS scores (O, S, and OI) suggest that the bell-shaped distributions of hypnotic susceptibility found with scales such as the Stanford scales may result from a lumping of responses experienced as voluntary with those experienced as involuntary (Spanos, Radtke, Hodgins, Stam, & Bertrand, 1983). Weitzenhoffer (1980), one of the authors of the Stanford Scales, has also criticized those scales on the ground that they fail to consider subjective response to suggestions, such as involuntariness, and thus that they do not measure in any pure form the essence of the "classic suggestion effect."

However, Hilgard (1981), also an author of such scales, has, argued to the contrary and has discussed relevant evidence, as has Kihlstrom (1985). Kihlstrom suggests that the Stanford scales somehow tap into the classic suggestion effect (which involves involuntariness), whereas, he tends to think, the CURSS might tend to elicit mere compliance from many subjects. He cites evidence that he feels support such charges, but I suggest consulting that evidence for oneself (and other related evidence). Kihlstrom (1985) correctly notes that the CURSS tends to emphasize readiness to cooperate, which could add a stronger element of pure compliance than is found in some other scales. However, I personally know of no clear evidence that the CURSS is meaningfully different relative to compliance than the various Stanford scales. Some additional work is needed to adequately assess such a charge.

For investigators interested in the study of psi performance under hypnosis, the CURSS might, however, have some drawbacks as a hypnotic susceptibility test if its standard, five-minute induction is employed. Pardon the pun, but this sounds like a rather cursory induction, given the lengths of standard inductions in other tests.

I also have another reservation about the CURSS, namely, its inclusion of an item involving the suggestion of a hallucinated kitten in the subject's lap that he or she is to pet. Such an item might prove harmful to any subject who is both highly suggestible and phobic concerning cats. Please do not laugh! There are persons with irrational fears of cats, and there is considerable evidence in the literature to suggest that phobic individuals tend to have relatively high levels of hypnotic susceptibility or that there are commonalities between the phobic and trance-like experiences (Frankel, 1974; Frankel, 1976; Frankel & Orne, 1976; Gerschman, Burrows, Reade, & Foenander, 1979; John, Hollander, & Perry, 1983; Kelly, 1984; and additional work referenced in these sources).

Those wishing to consider other measures of hypnotic susceptibility than those discussed above might wish to read the recent *Annual Review of Psychology* chapter on hypnosis by Kihlstrom (1985) and study reports cited therein in his section on the assessment of hypnotizability, especially if they are contemplating the use of instruments such as the Creative Imagination Scale (CIS, Wilson & Barber, 1978) or the Hypnotic Induction Profile (Spiegel, 1977), but desire an instrument that will reliably accomplish the task achieved very well by the Stanford scales (especially SHSS:C). Not every measure that has made the pages of the hypnosis literature should be considered as of equal value for the study of general hypnotic susceptibility. The CIS, for example, does not show the psychometric properties of the HGSHS:A, and does not correlate well with it. It appears to access imaginal abilities specifically, rather than the more complex domain of hypnotic susceptibility (McConkey, Sheehan, & White, 1979).

My own opinion is that the Barber Suggestibility Scale (BSS, Barber, 1969), which has sometimes been used to advantage in parapsychological research (Honorton, 1972; Palmer & Lieberman, 1976; Stanford, 1971), has some distinct drawbacks. Perhaps the greatest is the strongly assertive or authoritarian tone of its suggestions. I worry that some subjects who are potentially responsive to suggestions or who are hypnotically susceptible might respond poorly to this scale because its assertive wording causes psychological reactance (resistance). Robert Pancza (1983) whose dissertation work was done under my mentorship, found in his work that the trait of absorption—which has often been found to predict hypnotizability—predicted suggestibility with a permissively worded suggestibility scale, but not to a significant degree with one that was worded very assertively or in authoritarian fashion. This suggests that if parapsychologists are interested in developing hypnotizability measurements that are mediated, at least in part, by imaginative involvement—as is the case with most hypnotic susceptibility scales—they might do well to stay away from the assertive BSS. This might be especially true when testing male subjects (at least with a male hypnotist or suggestor) because Pancza found that among male subjects the suggestibility-absorption correlation dropped to near zero when an assertive scale was used (albeit not the BSS). We do not know the explanation of this finding, but it was clear-cut. Possibly, many males find such assertive suggestions (with implications of extreme passivity and malleability) to be an affront to their traditional role as males.

A final drawback to the BSS is, in my judgment, an item that suggests that the subject's throat and jaw feel like they are clamped in a vise and that the throat is clamped so tightly the subject cannot speak! With extremely suggestible subjects there can be little doubt that the subject could experience considerable discomfort with such a suggestion. In one study I myself found that very effect with an extremely suggestible subject. After completion of that study I have never again used the BSS, at least in unmodified form.

In sum, it is likely that the most useful tool for preliminary *group* screening—and one that would link one's work with a large and growing body of psychological research—would be the HGSHS:A. Individual testing at the same, rudimentary level might best utilize the SHSS: A or B. More advanced screening (as in the search for genuinely outstanding subjects) or subject classification might best be accomplished through the SHSS:C, which is widely regarded as the state-of-the-art tool for anything above the level of rudimentary screening. Even the SHSS:C might profitably be supplemented by one or more measures of subjective response to the test items. For indications of responsiveness to a variety of high-level "cognitive" suggestions SPSHS:I or II should prove useful. The CURSS is certainly useful as a shorter test (7 items), and it has the advantage of providing three types of suggestibility scores (O, S, and OI). However, it has some potential drawbacks mentioned above. If an investigator wishes to assess subjective response and involuntariness, as is done with the CURSS, there is no reason that post-hypnosis-session inquiries (modeled after those used in the CURSS) cannot be applied to the HGSHS and the various Stanford scales (see, e.g., Farthing, Brown, & Venturino, 1983).

Incidentally, there may be some reason to think that the probability of involuntariness (experienced or inferred by the subject) varies across types of suggestions. Farthing et al. (1983) found an unusually high proportion of subjects reporting that the posthypnotic suggestion was voluntary—a finding that concurs with the experience of Pancza (1983) in his dissertation, which involved work with another scale. Thus, in studying involuntariness-voluntariness, investigators might do well to examine any specific items of special interest to them, in addition to developing an overall index of this factor.

The parapsychological literature already contains indications that level of hypnotizability or suggestibility can be an important factor in subjects' ESP-task performance following a hypnotic-induction ritual (Honorton, 1972), and this topic needs further investigation, especially work using standard scales, and, ideally, one less authoritarian than the BSS. In the study of ESP in hypnotically induced dreams just cited, self-reported quality of hypnotically induced dreams was a successful predictor of hypnotic-dream ESP-task performance. It is worth noting that SPSHS:I and II each have an item concerned with hypnotic dreaming and, thus, might provide a preselection device for selection of promising subjects for hypnotically induced extrasensory dreams.

Parapsychologists' interest in finding good instruments for measuring hypnotic susceptibility might be heightened through awareness that Graham and Evans (1977) found, as they had expected, significant correlations between hypnotic susceptibility and subjects' ability to generate a random sequence. In the case of the HGSHS:A, the correlation was -.35; in the case of the SHSS:C, it was -.46. (In terms of the data in question, the negative correlations mean better randomizing ability among high-susceptibles.) Let us be clear here that these investigators were looking at the ability to produce random sequences. Such findings might have relevance to Stanford's suggestion (1975) that hypnosis might accomplish some of its psi-favorability by freeing susceptible subjects, at least, from some of the rational, patterning constraints that normally affect ESP-test-taking behavior. Sargent (1978) found that hypnotic-condition subjects did *not* show a significant tendency to balance calls across symbols in a forced-choice ESP task, whereas waking-control subjects did. Sargent also mentioned (but did not report statistics for) other evidence that hypnotic-condition subjects behaved with less rational constraints than did waking-control subjects. Interestingly, in Sargent's work subjects were not randomly assigned to the hypnosis and control conditions; there was room for a self-selection factor that might well have favored high-susceptibles being tested under hypnosis. Thus, having been subjected to an induction procedure was potentially confounded with unknown individual differences that might have included hypnotic susceptibility, which is now known to be related to the ability to produce relatively "random" sequences.

*Nonhypnotic Measures of Cognitive Skills Important to Hypnosis.* Probably the most commonly studied trait predictor of hypnotic susceptibility has been psychological absorption. Absorption, as measured by the Absorption Scale (Tellegen & Atkinson, 1974), represents the capacity and inclination of an individual passively and more or less exclusively to engage attention in some object or experience, either external or imagined. A high-absorption individual might become so enrapt in reading a novel that he or she would not hear someone call. For such a person, listening to music might become a totally absorbing inner experience or watching a waterfall might be wholly captivating. There is a long tradition of work related to this concept in hypnosis research. Another way of stating this is that many highly hypnotizable people are individuals with a tremendous capacity for imaginative involvement in stances outside the hypnotic context. (For relevant reviews see Lynn & Rhue, 1986, 1988.) There is a very respectable body of literature reporting a positive correlation between hypnotic responsiveness-behavioral and subjective—and Absorption Scale scores, which measure the trait of absorption. The essential idea here is that high-absorption persons have the capacity to make real to themselves the things suggested by the hypnotist and to maintain this sense of reality without distraction such that the experience of hypnosis is an intensely subjective one that involves involuntariness.

Why the trait of absorption should be associated with a sense of involuntariness is at the heart of a long-standing conceptual debate within hypnosis research concerning whether hypnotized subjects are or are not in an altered or alternative state of consciousness (or, similarly, are dissociated in responding to suggestions) or whether imagining the things suggested by the hypnotist strongly implies that a suggested movement is passive and is happening to one, rather than being done by one. The former position is that of the state (or special-process) theorists (e.g., K. Bowers and E. Hilgard); the latter, that of the social psychological theorists (e.g., N. Spanos). None of these parties has actively disputed the correlation between hypnotic susceptibility and Absorption Scale scores. Indeed, both camps have contributed evidence of the correlation, but they vehemently disagree about how such findings should be interpreted (see, e.g., Spanos, 1986).

The absorption construct has received considerable attention within parapsychology (for a review see Stanford, 1987a; see, also, Stanford, Kass, & Cutler, 1988a, 1988b), but there has been until recently (see below) little evidence that this cognitive variable, which is important to really successful hypnosis, constitutes a successful predictor variable for ESP-task performance. However, this internal-states-related variable has proven very useful in a recent psychometric effort to pinpoint time-locked verbal markers of entry into and, function within an internal attention state during ganzfeld (Stanford, Kass, & Cutler, 1988a). Presently, I am exploring possible relationships between these objective, verbal markers (identified through use of Absorption Scale scores) and ganzfeld ESP-task performance, first using data available from the study just cited and, second, through a new study currently getting underway.

Investigators who use the Absorption Scale in parapsychological work should bear in mind that that there is considerable and mounting evidence that (a) women score higher on the Absorption Scale than do men (e.g., de Groot, Gwynn, & Spanos, 1988; Spanos, Brett, Menary, & Cross, 1987; Yanchar & Johnson, 1981), and (b) the correlation of hypnotizability and absorption (or similar measures) is sometimes reported to be greater for women than for men (de Groot, Gwynn, & Spanos, 1988; J. R. Hilgard, 1979; Spanos, Brett, Menary, & Cross, 1987). Although the psychological explanation of such gender-related findings is presently unclear and deserves study, such findings clearly indicate that it is not advisable to examine Absorption or related measures as predictors without either separating the sexes or statistically taking into account such differences between them. Failure to do so could favor some very misleading conclusions. This circumstance has not, to date, received recognition in parapsychology (or in much of the psychological research on hypnosis and suggestibility). I have just (begun to reanalyze the data from our recent ganzfeld study (Stanford, Kass, & Cutler, 1988a, 1988b) in light of such considerations. Preliminary indications from that work are that gender may be an important factor and that failure to consider it obscured some interesting findings. It is conceivable that failure to consider gender is one reason efforts to use Absorption Scale scores to predict ESP-task performance have been largely unsuccessful. (In a related vein, parapsychologists should perhaps also take into consideration that women tend to score higher on scales measuring belief in the paranormal; for discussion of some such evidence, see de Groot, Gwynn, & Spanos, 1988. Efforts to use such variables, as predictors of ESP-task success that do not consider the gender factor could conceivably produce misleading results.)

Despite a very respectable body of literature reporting a positive correlation between hypnotic responsiveness—behavioral and subjective—and Absorption Scale scores, a serious challenge has recently been lodged against the traditional interpretation of that correlation.

The claim has been made (Council, Kirsch, & Hafner, 1986) that, "Administering the Absorption Scale to hypnotic subjects may implicitly suggest that imaginative processes are important in hypnosis, which in turn could influence levels of expectancy for hypnotic responding" (p. 188). The Absorption scale has usually—but not always—been administered in a setting in which its link with the hypnosis study was transparent and, additionally, the scale has usually been given "unbuffered" (my term, not that of Council et al., 1986). That is, its items have not been embedded among items unrelated to the construct. The latter fact is perhaps even more troubling because all the items of the scale are keyed in the same affirmative fashion. A "yes" answer always contributes positively to the Absorption score. Under these circumstances, it is reasonable, following Council et al. (1986), to suppose that persons respond to the Absorption Scale, infer a connection between its content—intense inner focus of experience, including imaginal absorption—and hypnosis, and, then, infer something about their own likely level of response to hypnosis on the basis of such information. Then those expectations might influence their actual level of performance. Such are the claims of Council and colleagues. They (Council et al, 1986) report data, including expectancy measurements, that they claim support such an interpretation—at least when they are considered in light of earlier findings from other investigations that they review.

These methodological flaws are potentially serious ones and we can only be grateful to Council and colleagues for bringing these problems to the attention of the research community. Of course, such problems need not imply that any artifactual contribution to the Absorption-hypnotizability correlation is mediated by or only by expectancy. Demand characteristics and related compliance might play a role in the Absorption-hypnotizability correlation when the Absorption Scale is given unbuffered in the hypnotic context. Another possible factor is that taking the Absorption Scale in circumstances in which its relationship to hypnotizability is transparent may favor higher hypnotizability scores among high-Absorption people because it provides cues as to personal inclinations and skills that can actually be used to advantage in the responding to the test suggestions that will subsequently be given. Let us term this the hypothesis of skill-relevant education. Demand characteristics and skill-relevant education could play roles over and beyond the possibility that such procedural errors have effects mediated by expectancy. Indeed, it is even possible that expectancy effects are themselves mediated by demand characteristics.

What I regard as the first clear-cut evidence against the claim that the Absorption-hypnotizability (or Absorption-suggestibility) correlation is spurious for any of the several reasons considered above came from our own laboratory at St. John's University in the dissertation research of Robert Pancza (1983), which I supervised. This evidence emerged prior to the publication of the Council et al. (1986) report. Pancza found the expected correlation of Absorption and suggestibility when (a) the items of the Absorption Scale were embedded among a majority of items measuring other types of traits (in accord with a recommendation from the author of the Absorption Scale, A. Tellegen), thus making the Absorption items less salient, and when (b) the Absorption Scale was administered in another experimental context by another experimenter, usually days or even weeks prior to the measurement of suggestibility, thus presumably eliminating any of the possible context-related artifacts.

The validity of the Absorption-hypnotizability correlation has been further supported by more recent work by Kihlstrom, Hoyt, Nadon, and Register (1987), work that used a different set of operations than those of Pancza (1983) and which therefore strengthens the conclusion that the debated correlation is valid, not artifactual. It now seems clear that the correlation is a valid one (although this does not mean that artifacts related to demand characteristics and/or expectancies would not occur under suitable circumstances). Parapsychologists should, therefore, not feel intimidated about using the Absorption Scale in their research, at least if they follow the general precautions outlined above in connection with Pancza's dissertation research (1983). Even if it should not prove possible to administer the Absorption Scale in a separate context from the dependent variable it is intended to predict, it is easy enough to administer it in a buffered form (i.e., with additional, non-absorption items to disguise the intent as much as possible). That is a nonoptimal solution, but it is better than nothing.

Psychometric work on the Absorption scale has continued, and there is now some evidence that for certain purposes its items might fruitfully be divided into those concerned with absorption in inner events and those concerned with absorption in external events (Balthazard & Woody, 1987). These investigators found that although there is no clean two-factor structure to the Absorption Scale and the items seem to form a continuum, the continuum can be thought of as having two ends, one constituted by absorption in inner events and the other by absorption in objective or outer events. In their work, absorption in outer events did not make a meaningful contribution to the prediction of hypnotizability beyond that contributed by absorption in inner events, yet absorption in inner events did make a meaningful contribution to prediction of such events beyond the contribution from absorption in outer events. This development, if it is reliable, might contribute considerably to refining the use of the Absorption Scale as a predictor in settings such as ganzfeld in which imagination is a very important factor. I would note, however, that Balthazard and Woody used a lengthier version of the Absorption Scale than have most investigators, myself included. I am currently seeking details on the above developments, including the expanded scale. Presently I have available only the abstract cited above.

In summary, the Absorption Scale is still of considerable interest to parapsychologists, despite several failures in efforts to use it as a predictor of ESP-task performance. Such failures could derive from irrelevance of absorption to ESP-task performance or they could derive from problems in the studies. In my recent review I discussed several possible reasons for failures, including unreliability of ESP-task scores and efforts to find the correlation in settings in which subjects are given a rather active operant-type set, rather than the passive one in which the trait of absorption is supposed to have relevance. In this commentary I have pointed to an additional possible reason for such failure, namely researchers' not having separated the data of males and females for analysis even when gender is known to affect both Absorption Scale scores and the likelihood of that scale correlating with hypnotizability. The predictive power of the scale might be enhanced by use of items referring to absorption in internal, rather than external events. Certainly, further work with the Absorption Scale (or selected items from it) is warranted because of its centrality as a trait relevant to internal attention states and because it has already proven useful as a trait measure that can help to identify time-locked verbal indicators of entry into and function within an internal attention state during ganzfeld (Stanford, Kass, & Cutler, 1988a). There are even preliminary indications in my own work mentioned above—which need confirmation—that the Absorption Scale does have predictive value for ganzfeld ESP-task performance, but that such findings might sometimes have been obscured by failures to recognize gender differences in both Absorption-Scale scores and in the capacity of such scores to predict external criteria (such as hypnotic susceptibility).

There are other measures of cognitive styles—probably quite highly related to absorption—that hypnosis researchers have sometimes examined as predictors. One of the most commonly used is the Inventory of Childhood Memories and Imaginings (ICMI), a test developed by Wilson and Barber (1981) and that has proven very useful (reviewed by Lynn & Rhue, 1988). The test is supposed to measure fantasy proneness. It has received some attention in parapsychological journals. For example, the ICMIC (children's form of the ICMI) correlates at a relatively low level (with undergraduates as subjects) with OBE reporting (Myers, Austrin, Grisso, & Nickeson, 1983) and, more generally, with the reporting of ostensible psi 'experiences (Council, Greyson, Huff & Swett, 1986). However, such measures of fantasy proneness are neither conceptually nor empirically highly distinguishable from measures of absorption; correlations of greater than .70 have been reported (Lynn & Rhue, 1988). Council, Greyson, Huff, & Swett, 1986 reported one of .68.

*State Reports.* Tart (1980) also advocated studies of hypnotic depth using probes during the actual hypnotic session as the basis of making inferences about depth. Elsewhere, Tart (1972) had discussed in detail the use of such scales. As he recognizes (1972), the use of such scales requires certain theoretical and methodological assumptions. (Although he does not mention it, some of these are controversial in the field of hypnosis and others are doubtful on grounds of psychological scaling principles.) These assumptions include that there exists one or more dimension(s) of profundity of hypnosis along which subjects may change during a session (as when the induction procedure deepens hypnosis successfully). One must also assume, as Tart notes, that positions along this dimension have either experiential correlates or can be tapped through unconsciously mediated responses (in giving scale reports). Tart also notes that the depth dimension must have shared commonality across subjects in order for the scale reports to be useful across subjects.

In my view, there are two very important assumptions that Tart does not mention at all. Even if all the above assumptions were true—which I seriously doubt—we would have to meet two very important scaling assumptions, namely that (a) the scales are used in the same way by all subjects and (b) by each subject in the same way across time. These assumptions would, at least, have to be met in order to do the types of group, process-oriented work that most of us desire to do in parapsychology. Additionally, there are other serious considerations to be discussed below that militate strongly against the use of such scales in research or, minimally, against making particular types of process-oriented conclusions on the basis of the use of such scales.

I will take as a case in point to illustrate the presence of several of these difficulties the Long Stanford Scale (LSS) (Tart, 1970, 1972), a scale that has received some use in the literature even very recently (e.g., Council, Kirsch, & Hafner, 1986 who used a modified version of the LSS). As used by Tart (1970), the LSS has subjects rate their depth of hypnosis by using a scale from 0 to 10. The lower anchor on the scale in this case referred to being in a normal state of consciousness, defined for the subject as being awake and alert. [Note the interesting and extremely controversial assumption here that being hypnotized means definitely not being awake and alert. Note that this is an odd "dimension," since it begins with awake alertness, which raises the question of whether the dimension should end in obliviousness (which hypnosis researchers would surely reject). I suggest that "awake" is a bad place to start for a scale of profundity of hypnosis since it is built around the myth, rejected by everyone, that hypnosis is sleep-like. Note also the assumption that the normal state of consciousness is awake and alert for everyone (or, if it is not, that they are walking around partially hypnotized?). What about individuals who suffer from excessive daytime sleepiness (EDS) for reasons such as narcolepsy? By conservative estimate, EDS afflicts somewhere between 1 and 4% of the general population, according to Anch, Browman, Mitler, & Walsh (1988) and increases considerably in likelihood with advancing age, due in part to respiratory problems that impair sleep. Interested readers can examine for themselves the full text of this scale (see, e.g., Tart, 1972, p. 256–257) to see how ill-defined are descriptors for the intermediate portions of the 11-point scale. The descriptor for "10" just indicates that subjects are deeply hypnotized and are, essentially, ready to do anything suggested to them. The hypnotized = compliance statement would seem virtually to insure that anyone giving the "10" rating (or, actually, any high rating) before any particular suggestion would try hard to succeed on that subsequent suggestion. It also has the very undesirable consequence that it biases the whole test toward finding a correlation, not only between the number of previously passed suggestions and self-rated depth, but also between an immediately antecedent rating and the likelihood of passing the subsequent behavioral test. Indeed, since the entire scale is defined ahead of time for the subjects, it would seem to imply to subjects (among several other things) that the higher the rating you give of your depth, the more items you should pass. Tart (1972, p. 457) also indicates that all subjects were told that if they feel more deeply hypnotized, they should expect to experience more hypnotic phenomena (read "pass more test suggestions"). Such circumstances seem, truly, an arrangement almost guaranteed to produce (spurious) evidence of the construct validity of the depth scale—that is, by causing it to correlate artifactually with behavioral measures of hypnotic susceptibility.

Is it, then, very surprising that Tart (1970) should have found that across subjects the average state report that preceded passed items was greater than that for items not passed? If subjects report good depth just before a suggestion, they might appear to have been dissimulating in giving that report if they did not subsequently give concordant behavioral responses. Also, if they reported poor depth prior to a test item, responding to that item might itself appear to be faking. Such reactions on the part of the subject are definitely to be expected, given the way in which the depth scale instructions fundamentally link depth and behavioral response and given subjects' well known desire to be both self-consistent and anxious to manage impressions of themselves (not even to mention the possible pressures associated with the demand characteristics here). (The problem would be there even if the definition were not explicit, since it becomes implicit when inductions speak of "going deeper" and, after depth has been ostensibly achieved, test suggestions are introduced.) Of course, Tart also found that passing of items *prior to* depth measurement predicts depth reports in the way one would expect under either his theoretical orientation or a social-psychological (attributional) one.

The modified LSS scale used by Council, Kirsch, and Hafner (1986) reads as follows: "One means you're not hypnotized at all, 2 indicates a light trance, 3 means a medium trance, 4 is deep, and 5 is very deep" (p. 183).

When the LSS is thus modified, as used by Council et al. (1986), to a 5 point scale, there are potential problems of a somewhat different kind because of confusion about how the dimension to be rated (depth of hypnosis) is actually defined and a lack of anchor(s) for the scale. Here the lower end is potentially somewhat anchored (by the possible feeling "nothing unusual has happened to me as a result of the induction"), but the upper end is not. Here a lack of any clear guidelines is an open invitation both to subject differences in how the scale is used and within-subject changes during the session in how to use it. Problems with using scales such as these are that they (a) provide the subject with no meaningful, clear definition of the dimension itself and (b) no way to anchor their responses relative to any dimension that they might somehow discover and, thus, no way to assign numbers consistently and systematically. This is an undesirable circumstance, psychometrically speaking.

It should be made clear, however, to the credit of the authors of this important, ground-breaking study (of which this particular scale is only one item of interest) that the instructions given to subjects prior to hypnotization did provide indications about what being hypnotized might be like. (Dr. Council has kindly provided at my request a copy of the instructions preceding the hypnotic induction.) Such information is usually given prior to an induction because it helps reduce possible fear and apprehension concerning the unknown experience. The provision of such information should make it easier and more meaningful for subjects to make judgments about whether they are hypnotized or about depth of hypnosis (defined in terms explicit and implicit in the instructions given them prior to hypnotization), and it might reduce inter-subject variability in how the scale is used. It cannot, of course, guarantee that the scale is measuring anything like true hypnotic depth (if that is a meaningful construct)—and I think that Council and his colleagues might agree with me here. Very important, the provision of such prior information cannot provide any clear or easy basis for subjects' making one-dimensional 5-point-scale judgments about their depth of hypnosis. At some level this must remain a complex, difficult and uncertain task, with possible spin-offs to be discussed below.

Let us consider these matters very generally now, without reference to any particular study. With such scales subjects are required to make judgments about an extraordinarily vague idea, hypnotic depth, about which psychologists themselves argue vigorously regarding its degree of conceptual appropriateness for what actually happens during hypnosis. It is this requirement of making ratings of depth of hypnosis in the face of exceedingly vague criteria that poses additional problems. When subjects must make judgments in the face of uncertainty, they are motivated to use all the information available to them.

I have already indicated the likelihood of their comparing their experiences with information given prior to the session in order to fit the match, somehow, into their judgments. What is potentially very important here is that we are really asking subjects to draw conclusions, make inferences or attributions, about the causes of their behavior and experiences in the situation. They are in a sense being asked to decide in what degree their behavior and experience are attributable to being hypnotized. They presumably reflect upon these matters (consciously and/or unconsciously) and report a judgment. But it is very important to realize that this judgment is something that *comes into existence* because of a need to understand what is happening in order to respond to the query. It is a potentially very central attribution in this setting, and the outcome of this attributional analysis (be it done quickly and intuitively or more analytically and thoughtfully) will, probably, have a decided influence upon subsequent behavior and interpretations of the setting.

Let us clearly understand the potential functional and theoretical significance of this, if it occurs. Hypnotic investigators of the state-theory persuasion (i.e., who believe that hypnosis involves an altered or alternative state) opine that as subjects pass test suggestions this "deepens" the hypnosis. However, this claim is troubling and puzzling because of its vagueness. How or in what sense can state be changed by observing oneself pass a test suggestion? Perhaps state theorists can pose a clear answer to this question, but I have never seen one. My own inclination is to accept some variant of the social psychological view that passing a suggestion behaviorally *in the presence of a sense that the response occurred involuntarily* provides potential feedback to the subject that says, "I am hypnotized." (Social psychological theorists have explanations of how response in such settings can seem involuntary, e.g., Spanos, 1986, or Angelini & Stanford, 1987, the latter a view based upon cognitive-attentional factors.) This is because part of the socially agreed upon meaning of hypnosis concerns perceived involuntariness of response or perceived automatic occurrence of the things suggested by the hypnotist. Once the subject concludes that there is evidence that he or she is hypnotized, subsequent processing of the situation is different, less critical, and is focused upon accepting the things said by the hypnotist in a relatively literal way and a readiness to experience them as such.

If this is true of observing oneself respond to items on a hypnotic susceptibility scale, it might also be true of clear, direct attributions made in response to queries about one's state as things progress. (Here some such process seems, indeed, relatively certain because the investigator forces one to draw a conclusion about what is happening, or, minimally, to act as if one has. In merely responding to behavioral tests in hypnosis, such attribution may not be as actively favored, though it would often occur spontaneously.) In short, giving state reports might well represent, not a passive testing of something that already exists, namely depth, but, rather, a kind of intervention that can either facilitate or perhaps interfere with the total process. If state reports go "well" (i.e., indicate increasing depth), the process tends to feed on itself, further facilitating the entire process. This is because state reports represent an active judgment and because a series of them indicating deep or deepening hypnosis may tend—like a series of passed suggestions—to convince the subject that he or she must really be hypnotized.

Interestingly, making such judgments tends to remove the subject's attention from the experimenter as a source of influence upon behavior (i.e., as a source of pressure for compliance) and refocuses it upon the subject (as, for example, a hypnotized subject experiencing the suggestions). The question, after all, is "How deep are you?" not, "To what degree were you influenced by a perception that I wanted you to respond a certain way?" Consequently, after making state reports indicating hypnosis as being successful, the subject may tend to forget about the hypnotist as a source of social pressure for behavioral compliance and may simply make the judgment that his or her state reports, like behavioral responses to suggestions, must have been influenced by inner observations.

A general consequence of the above is that giving a state report is reactive; it is virtually certain to influence what follows.

It is interesting that Tart (1970, 1972) consistently found a very substantial, but nonsignificant ($p = .11$), tendency for the correlation between state-report and behavioral response to hypnotic suggestions to be higher for experimenter-requested "instant" (or "automatic") state reports than for experimenter-requested "deliberate" reports in which subjects are asked to make a conscious estimate of the best rating. A slight, but real, effect (perhaps significant with a more substantial sample) is what one would expect if subjects are forced by "instantaneous" instructions to make quick judgments about depth. Why? In such a case we would expect subjects to rely upon the most salient and accessible information as the basis of making their judgment since this would speed up response (as demanded by the instructions). It seems quite obvious that one's behavioral response to previous items is the information likely to be most salient and accessible in such a setting. Experiments outside the hypnosis area—for example in word association (Horton, Marlowe, & Crown, 1963)—have demonstrated that time pressure increases the use of accessible information.

In another sense, too, giving a state report is an active process by the subject that is likely to influence what subsequently happens. It represents a public commitment (i.e., one made openly to the hypnotist) that "This rating is how hypnotized I am." Even if the subject, due to subtle social pressures from the hypnotist, has given a state report better than what actually appeared to be the case (to the subject), he or she is likely to come to believe that state report because of the need for reducing cognitive dissonance engendered by such reporting (Festinger, 1957).

In a different vein, if one gives a state report, there ensues a real sense of pressure to act in accord with that report. To do otherwise would be to suggest dissimulation. In other words, state reports force the subject into a situation that requires impression management or self-presentational strategies. (For reviews and conceptualization of impression management or self-presentational strategies, see Baumeister, 1982; Schlenker, 1980; Tedeschi, 1981.) Self-presentational actions—even dishonest ones—might in such a situation lead to self persuasion through more than one channel.

In summary, the use of state reports in internal states research is fraught with difficulties. (And we have not even discussed here the potentially ready influence upon such reports of demand characteristics.) It suffers from very great psychometric problems including, but not limited to, the differential use of such scales across subjects and within a given session. The state-report approach is definitely reactive in character and thus has serious shortcomings relative to internal validity.

Possibly one response to this from some persons knowledgeable about the parapsychological literature would be that a considerable number of studies have found state reports, including state-report shifts, to be predictors of ESP-task performance. (See Palmer, 1978 for a review.) Let us assume, as I am inclined to, that this is a true, valid pattern of findings. Its interpretation is what is considerably problematic. At face value it seems to suggest either that a more altered state or a greater change in alteration of state as a consequence of session procedures favors ESP-task scores that differ from mean chance expectation (albeit, according to Palmer's review, not necessarily or always in the psi-hitting direction).

The discussion above about the problematic character of state reports, although developed in the context of hypnosis work, applies equally well to ESP testing in a number of settings that favor altered or internal attention states. It suggests alternative, and perhaps equally interesting, scenarios as interpretations of the covariation of state reports (or within-session shifts in state report) and ESP-task performance. Various scenarios could be developed, but I will discuss only one here as an illustration of a plausible alternative. If subjects are influenced by making state reports (and reflecting upon the information that is used in making them) in the ways suggested above, it is clear that a subject giving a state report indicating that consciousness has been properly altered would, in settings such as typical altered-states-ESP studies, also come to believe that he or she is in a suitable setting for extrasensory things to happen. (Why, after all, the elaborate manipulation—ganzfeld or hypnosis—and why the state reports? Experimenter expectancies in such a setting are transparent.) If the experimenter expects things to start happening and he or she is an expert—as surely he or she is—then the subject will come to expect that they will happen. This automatically results in the kind of freedom from egocentric striving, combined with expectant, passively interested, intrigued watching that may provide an excellent circumstance for ESP-task success. The subject can, figuratively and even literally, lean back, relax, and watch the psi start to roll in. Scientifically interpreting this scenario, the use of state reports has served as a "clever" manipulation, one that forces the subject to develop his or her own conclusions about state, and these conclusions in turn influence psi-favorable task attitudes and expectancies.

In short, when the ESP-task is set in an altered-states-favoring environment and state reports are elicited, these are not merely measurements. They are active manipulations or interventions that can influence the subject's performance in a way that would not have occurred in their absence. For ESP work, state reports could well be intrinsically reactive methodology, an approach that has sufficient methodological ambiguity to seriously temper any conclusions drawn from such methods—at least in the absence of ancillary work intended to resolve ambiguities introduced by them.

Taking a step further back and looking at this a bit more philosophically, it is easy for the investigator to believe that the subject can be passively measured in all kinds of ways relative to altered states and that through such methods one is simply discovering "the truth." It is easy even to believe that there are almost no limits to what one can explore in these wonderful, direct, introspective ways. One can find out about all kinds of wonderful and esoteric things like shifts in consciousness as they occur throughout the session. A kind of scientific imperialism can develop in which one fatuously believes that more or less the whole world of the mind is lying there, passively waiting to be measured. This dangerous delusion that one can very directly and meaningfully measure the mind without compromising its integrity gains a heady sway. Observation without interference is a laudable goal of science, but sometimes it is more apparent than real. The fact is that eliciting state reports—as many measurements in altered states settings—has an active character that inevitably affects subsequent events. Accurately interpreting research patterns—however reliable or replicable—obtained with these reactive methodologies requires great caution and, probably, supplementary work.

The use of potentially reactive measures in current hypnosis research does not end with elicitation of depth reports. It extends also to the very important study (see below) of the role of expectancies in response in the hypnotic setting. The Council, Kirsch, and Hafner (1986) study mentioned earlier is an example of a potentially reactive study with regard to subject statements about expectancies, as well as with regard to depth reports. In it, subjects were asked not only to rate their post-induction depth of hypnosis (as discussed above), but to predict prior to induction a number of allegedly unsuggested hypnotic effects and their responsiveness to particular suggestions that were very similar to those used on the dependent variable. They had again to predict responsiveness to specific suggestions following the induction but prior to administration of actual suggestions. The use of such methods here, as in a number of hypnosis studies by other investigators, seems to imply that we can measure pretty much what we want related to subjects' internal processes, but still not change anything. That seems doubtful. This particular study is far from unique among hypnosis studies in using methodologies that might be considerably reactive. (And, despite these potential problems, this paper makes exceptionally important contributions because of its examining possible consequences of methodological problems in earlier studies of absorption and susceptibility.)

In the event that the above remarks have been overinterpreted or misunderstood, let me voice a caveat. Nothing said earlier should be construed as indicating that subjects do not or cannot have internal access to cues that are somehow related to internal state or depth—assuming, for the moment, that those are useful constructs. It is, rather, to say that even if this is true, those cues are likely to be diverse and that such information is likely to be complex, confusing, and difficult to interpret on a simple, unidimensional scale. For this and other reasons, the use of depth scales poses many problems discussed above. The use of such instruments, if it is to be done at all, must proceed with the greatest caution and realism.

Nor should these remarks be seen as an indictment of introspectionism in all its forms. For example, I will discuss below what I regard as better examples of such methods. In general, introspectionism works best when what subjects are asked to report are, phenomenologically speaking, straightforward experiences that are meaningful to people and that appear as part of the common language. Subjects, for example, have realistic, meaningful insights about how physically relaxed they are (Braud & Braud, 1974), as evidenced by correlations between "state reports" on relaxation and recordings of actual muscular tension. Asking for reports on hypnotic depth is definitely not an example of introspectionism in its classical and respectable form. It requires complex integration of and judgments concerning information, judgments that can only be based upon vague and ill-defined criteria. As stated earlier, it really, in essence, forces the subject to make interpretations or attributions about the cause of his or her behavior.

### *Measures Based Upon Post-Session Retrospection*

*Inventory of Hypnotic Depth.* This (Field, 1965) consists of 38 true-false items about allegedly unsuggested[5] cognitive and perceptual distortions or changes experienced during hypnosis. It is given at the end of the hypnosis session. The sum of items endorsed is supposed to indicate the degree of alteration of consciousness experienced during hypnosis. This instrument has found considerable use in the hypnosis literature. It has the special value that it is unobtrusive relative to the actual hypnosis session and is not reactive in the sense that in-session elicitation of depth reports is. (One could, however, ask questions about the psychometric wisdom of combining, by addition, the number of endorsements, as though this constituted a direct measure of a single dimension, depth.) Although this instrument does ask for introspections by subjects, it asks about particular possible kinds of experiences, not a judgment or attribution about the causes of one's behavior. Its content is quite face obvious and is potentially subject to many demand characteristics. Perhaps these can be reduced or obviated by appeals for forthright reporting. There is a sense in which this is not very different from the series of questions parapsychologists often ask subjects at the end of a ganzfeld session. Perhaps this instrument also warrants study for ways in which some of its ideas might be generalized to ganzfeld work, because it is obviously intended for hypnosis research. Parapsychologists might find it useful in hypnosis-psi studies.

This inventory has the advantages that it has been used in numerous published individual investigations, that its correlates are well known, that it is easy to use, and that it is unobtrusive and can probably be used in a nonreactive way, given that it occurs at the end of the session. However, one cannot be sure that answers to all its items represent

[5] I say "allegedly unsuggested" because it seems to many hypnosis theorists with a social-psychological orientation that some of these effects are at least implicit in the suggestions typically included in a hypnotic induction or in test suggestions administered.

actual experiences during the session, for some may represent retrospective interpretations of those experiences. I feel, however, that it has potential use in parapsychological hypnosis studies and that it should receive some attention. I am not aware of any published review covering all the work with this instrument.

*Phenomenology of Consciousness Questionnaire.* Ronald J. Pekala and various co-investigators have developed an instrument that is intended to measure several alleged dimensions of subjective experience associated with altered-states induction procedures (like hypnosis or meditation). The questionnaire is intended to measure particular elements of such experience in a given setting. In a recent paper (Pekala, Wenger, & Levine, 1985) it is stated concerning the Phenomenology of Consciousness Questionnaire (PCQ) that: "It is a 60-item inventory[6] with each item consisting of two statements separated by a 7-point Likert scale. Thirty-seven of the PCQ items adequately assess nine dimensions of consciousness. These dimensions (and associated subdimensions) include altered experience (body image, time sense, perception, meaning), attention (direction, absorption), awareness (self-awareness, state of awareness), imagery (vividness, amount), volitional control, internal dialogue, positive affect, negative affect, and memory" (p. 127). Four items, as an average, compose each of these nine dimensions.

This instrument sounds, at least on the surface, as if it might be useful to parapsychologists using techniques like hypnosis or ganzfeld. While I have suggested elsewhere (Stanford, 1987a) that this instrument might have potential use for parapsychologists, I am not very favorably impressed with the research report cited above and with some other information that I have examined on this instrument. I would like to know much more about any evidence concerning the reliability and validity (including discriminant validity) of the supposed dimensions and subdimensions. Correlations used as the basis of conclusions are often startlingly small, but significance is achieved through large samples. Furthermore, Pekala and colleagues seem inclined to use as the target of subject retrospections (using the PCQ), very short periods (e.g., four minutes) of, for example, persons sitting with eyes opened and with eyes closed. The study of this instrument could, it would seem to me, benefit by using it with extended (greater than four-minute!) meditation periods with experienced meditators and with situations such as ganzfeld (along with trait predictor-variables like absorption,

[6] I have in my possession contradictory information about the number of items in the PCQ. It appears to be in a state of flux due to revisions being made on it.

such as has already been done). This scale could, in my judgment, be more properly viewed as a valid and useful instrument if it showed meaningful results in such more realistic settings for extended periods. It has been used following hypnotic induction, apparently, with some success. Study of this instrument with factor analysis, rather than *a priori* judgments, would also, in my judgment, be useful, if that has not already been done—some of the research on this instrument has not been examined by me.

It is difficult to assess the promise in this overall, ambitious program because the pattern of significant results emerges from among many possible significant outcomes. Some findings that one would expect do not appear, whereas others that appear as significant are a bit surprising. For these and several other reasons that cannot be discussed here, I consider that this program is very interesting, but that the case has yet to be made for this being an instrument that would have potential payoff for investigating problems of interest to parapsychologists.

Certainly, this effort at mapping consciousness (as Pekala tends to call it) is fraught with many methodological problems and conundrums and has as a major difficulty the problem of very serious demand characteristics. I do not personally feel that questions related to demand characteristics have been adequately addressed in the research reports on this instrument with which I am familiar, despite some suggestions to the contrary.

This program does not have some of the problems associated with depth reporting in an ongoing session, but it surely has problems of its own. Perhaps with further testing and refinement Pekala and colleagues will have an instrument that will prove of value to parapsychological investigators. Or perhaps parapsychologists might wish to use the instrument now and attempt to aid in its investigation. If so, I urge great caution with regard to the problem of demand characteristics. Pekala's general approach, has, however, the very attractive feature that it seems to promise a considerably more rich analysis of the topography of internal states than has heretofore been offered. It is built, in large degree, around Tart's conceptual analysis of altered states (e.g., Tart, 1975), which is, to my way of thinking, a very thoughtful and potentially productive analysis that helps make altered states a manageable area for research and conceptualization. Another asset of the PCQ is that it is not limited in its application to a single altered-states-conducive setting.

It would be of interest to try this instrument with subjects at the end of a ganzfeld-ESP session in order to learn whether any of the dimensions predicts ESP-task performance. (Discovering the proper interpretation of any such finding would, naturally, require additional research.)

Potential researchers with the PCQ should know that it is not, apparently, available for use gratis as a research tool. Information on the cost of the instrument, instruction sheets, various other materials and the user's manual can be had through writing to Pekala, whose address appears on his publications.

*A Special Methodology Associated With Hypnosis Research: The Real-Simulator Design.* This design was first discussed in the parapsychological literature, to the best of my knowledge, by Honorton and Krippner (1969). It is the creation of Martin T. Orne (1959, 1972). Without getting into details, it represents an effort to discover what are the consequences of any demand characteristics present in one's design. It involves careful identification, *a priori*, of two groups of subjects. One is highly susceptible to hypnosis and the other definitely is not, as determined by at least two standard tests. The former become the "reals" in the design, and the latter, the "simulators." In the real-simulator design there are two experimenters. The one who is the hypnotist and who takes the dependent measure is blind as to which subjects are "reals" and which are "simulators." For the simulators the nonblind experimenter provides instructions to the effect that they are to fake being hypnotized in the session with the hypnotist-experimenter, and they are told that although the latter will know that there are some simulators, he or she will not know who the simulators are. Simulators are further told that if the hypnotist-experimenter detects their simulation, the experiment will immediately be terminated. Simulators know that there are also reals (susceptible persons) who will be in the study. Reals do not have simulation instructions and do not even know of the existence of simulators in the study. Nor are they told that if the hypnotist-experimenter should ever think they are faking the experiment will be stopped.

Trance or state theorists have seemingly loved this design because a sizeable number of studies have shown differences in behavior of real and simulating subjects when they are given the same suggestions and are seemingly acting under the same demand characteristics. Such differences have often been taken in the literature as evidence that something nonartifactual and real is happening with the real group, something that indicates a special characteristic associated with being "hypnotized" that allegedly does not occur due to the social psychological variables so much invoked by social constructionists in trying to explain the hypnotic domain. Readers should consult Orne's 1972 paper to learn his own views on the requirements of the design and the legitimate conclusions that can be drawn from it (and to learn what he thinks are some illegitimate applications to which it might be put). He claims that the method is basically for the purpose of assessing what will be done by nonhypnotized subjects when they are motivated to do so by the experimental setting. He sees its uses as including evaluation of claims that hypnotized persons can do things they could not do if not hypnotized or will do things that they would not do if unhypnotized. He sees it as also useful when there is the possibility that subjects will see through experimental deception.

It is my personal judgment that the hypnosis literature is replete with examples of persons overinterpreting real-simulator differences found in studies. The method has its own special problems. These are discussed in some detail by Spanos (1986). The real-simulator design, as Spanos (1987) notes, neither controls for demand characteristics nor insures that they are the same in so-called real and simulating subjects. The latter would seem to be a requirement of the design if it were suitable for the objectives often alleged. In what follows, I make no claim that my analysis necessarily follows that of Spanos.

Investigators need to understand the many kinds of limitations in the real-simulator design. In the first place, it is confounded heavily with subject characteristics (Barber, 1969). (Orne knows that, but many investigators who write reports on their findings with this design seem to ignore it.) There can be no doubt that real and simulating subjects possess different cognitive inclinations and/or skills, precisely because several such differences have been identified between low- and high-susceptible subjects.

A study has recently appeared that illustrates this potential confounding and how investigators and readers sometimes ignore its potentially problematic implications (Nash, Johnson, & Tipton, 1979). These investigators were studying hypnotic age regression and had as dependent variables three measures of how regressed subjects, under hypnosis (reals) or simulating (low susceptibles), related to a transitional object. Significant differences were found on all three dependent variables. This, in the opinion of the investigators, showed an effect due to hypnosis.

Such a conclusion is, however, unjustified. Differences in cognitive skills and inclinations in high-as contrasted with low-susceptible subjects might account for the differences observed, as could other possible differences such as childhood experiences, that might affect hypnotizability. Would this pattern of results, for example, have been favored if the reals simply had much more vivid imaginations that allowed them to experience more clearly the situation suggested by the hypnotist?

Or what about the possibility that subjects who remember having had (actually had?) transitional objects tend to score better on susceptibility tests? It is to Nash's credit as a thoughtful and honest investigator and reporter that in a second paper on this general topic (Nash, Lynn, Stanley, Frauman, & Rhue, 1985) he does discuss (only) the latter possibility and considers evidence from other work that is relevant to it, but he did not seem to recognize this possibility at the time of writing the first report. Neither, apparently, did Kihlstrom (1985) in reviewing age regression work, for he indicates that this is the single study providing strong evidence of a reinstatement effect during hypnotic age regression. It is certain that the real-simulator design is inadequate here to sustain the kind of conclusion that many commentators would have liked to have had.

It is good to be able to report that Nash and colleagues in the second study cited above also point out that real-simulator differences are quite problematic of interpretation and that ancillary designs are needed for definitive conclusions. Regretfully, real-simulator designs have sometimes been misused and misinterpreted in the literature and, too often, with no such sage caveats forthcoming from their authors. As noted by Nash and colleagues in the later study (Nash et al. 1985), the real-simulator differences they observed might simply be due to engaging in simulation (as contrasted with passively being a hypnotic subject), rather than to the effects of hypnosis per se. It is rare to see such appropriate reserve in interpreting a set of new and exciting findings. It is also too infrequently found in the real-simulator work generally.

The latter remarks—concerning the task or processing demands of being a simulator—also bear comment. This is one of the major problems with the real-simulator design. The processing load for the simulator is heavy. There is a need to understand the suggestions given, to anticipate just what would be proper hypnotic response, and to enact such response. This is all done under the psychological pressure that if one is not good enough at one's performance, the show will abruptly be stopped. One will be caught in the act of faking and the show will not go on. The privileged reals, on the other hand, are not even told that if their performance is less than up to snuff, then the whole act will be canned! One wonders what might happen if they were told, "If the hypnotist should for any reason conclude that you are faking this, rather than really being hypnotized, the experiment will be terminated." Of course, it would by argued by some that this would surely interfere with hypnotic responding. A social-psychological theorist would point out that the strong public self-consciousness thus engendered might interfere with performance of any role, including that of the simulator, who is actually exposed to such a threat. (Interestingly, and not surprisingly in light of the above discussion, simulators often overplay their role.)

Sarbin and Coe (1972), two leading theorists of hypnosis as role playing, note that since reals and simulators get decidedly different instructions (with different background information relative to those), they could be expected to play different roles. Much of the alleged evidence for a special role of hypnotization might thus be explained. In a closely related vein, Wagstaff (1981, pp. 107–109) does a masterful job of explaining how differing role construals mediating the compliance of reals and simulators provide a cogent alternative explanation of the results of a classical study of the alleged compulsion induced by posthypnotic suggestion (Orne, Sheehan, & Evans, 1968). Readers might wish to consult the details of this account for the sheer intellectual stimulation of seeing how a little imagination can make some fairly mysterious looking outcomes seem relatively prosaic.

There is also the problem that simulators know that they cannot "genuinely" perform on the hypnotic tasks, so they are already at a moral disadvantage. (On the other hand, reals are even protected from the knowledge that simulators are involved—on grounds that their feelings might be hurt by the thought that they are not trusted and that this could result in impairment of their hypnotization. Simulators, as noted earlier, know that reals are also involved.)

Martin Orne is well aware of the possibility that such factors might play a role in influencing real-simulator results, but investigators using the design have been less cautious in too many instances. Furthermore, any caveats put forth in discussion sections are likely to be ignored by many readers and, very often, even by those preparing literature surveys or reviews. Orne (1972) discusses possible alternatives to real-simulator designs and other approaches to controlling for the demand characteristics problem.

It is fair to say that, in sum, the real-simulator design has very great limitations, has been overused and may well have outlived its usefulness except for some very specialized types of problems. Spanos is surely right that it does not handle the demand characteristics problem suitably (Spanos, 1986). There is a need for additional work on finding alternative, less problematic designs. Parapsychologists might lend a helping hand here and thereby show that they can make important contributions to nonpsi areas of research. It is possible, as I will suggest below, that real advancement in determining the effects of demand characteristics could depend upon conceptual developments.

With such problems for the real-simulator design, I find it surprising that it has had and continues to have such popularity. It seems to me potentially wasteful of investigators' time in many instances. Surely we can find ways of motivating our subjects without telling them that they cannot really do something but will have to fake it! T. X. Barber's original task-motivational instructions (e.g., Barber, 1969), which were intended to provide a control for the motivation imparted to hypnosis-condition subjects by the instructions and psychological setting, have been properly criticized (e.g., Bowers, 1967) because of their misleading subjects with regard to normative data on performance by past subjects (saying all of them could do the things suggested) and because they introduce some rather extreme strong-arm tactics to get subjects ready to go along with the suggestions (implying that one might be sub-normal if one did not accomplish the things suggested and that a lack of response would waste peoples' time). Various studies have demonstrated that persons respond to such strong-arm tactics by out-and-out compliance. However, we have in our laboratory (in nonpsi work) been using a task-motivational approach that does not attempt social arm-twisting or induce possible guilt about noncompliance. I would suggest that new approaches to motivation-relevant controls are needed in hypnosis and suggestibility research.

It is important in this context to note that parapsychologists may not face all of the same problems vis-à-vis demand characteristics as do investigators who use dependent variables (behavioral ones) that are under volitional control. In a psi experiment, even if the subject can guess the investigator's hypothesis thanks to demand characteristics of the study, this does not mean that he or she can or will therefore deliberately bias the results of the study. Although success at such deliberate biasing of psi outcomes is not inconceivable, it seems doubtful that most subjects would even try to shape their psi results to meet experimenter expectations. This is because most subjects almost certainly have no belief that they can deliberately manipulate their psi-task performance in such a way or would believe that they know how to go about it. They are busy enough just trying to have some extra-chance success.

If demand characteristics play a role in psi experiments, it is, therefore, likely that they do so, not through deliberate attempts at compliance, but through the mediation of situation-induced expectations. The cues that tip off subjects to what kind of performance the experimenter expects in the setting at hand can also lead them to expect that such performance will actually be forthcoming. This can happen if subjects accept such expectations as "expert" and correct opinion and, as a consequence, have their psi-task performance shaped (through whatever means) by those cue-based expectations.

This is not an improbable scenario. The problem of what to think about one's ability to perform in the ESP-task setting is an ill-defined one for the subject, who is usually unfamiliar with such situations and what is likely to happen therein. Such ill-defined situations are where social comparison processes are likely to come vigorously into play (Festinger, 1950, 1954), precisely because we have a need to evaluate our own opinions and abilities. In such a case, said Festinger, persons evaluate their own opinions and abilities by comparison with the opinions and abilities of others, to the degree that more objective means of doing so are not available. In the present case, a subject presumably asks himself or herself, in effect, "What can I think about the likelihood of my doing well in this task? I have no past record upon which to base expectations. What information is present here that could tell me what to expect, how well I will likely perform? Oh, yes, the experimenter obviously thinks people will do well here. Otherwise, why would he go to the trouble of hypnotizing everyone? I suppose I will do well, especially since the experimenter obviously has some good reasons to feel that way." If the subject thinks this way, it might influence his or her psi-task performance.

If the above reasoning is valid, the parapsychologist's problem vis-à-vis demand characteristics is one of somehow controlling for any expectancy effects generated by them. This suggests that investigators need to be sensitive to the expectancy-related consequences of their manipulations. Such consequences are confounds when we intend to manipulate something other than expectancy. This circumstance may also suggest the need for manipulation checks relative to expectancy. William Braud has been one of the leaders in paying attention to this possible problem through the use of manipulation checks. Many of us have lagged behind, but here is a good reason for "throwing in" an expectancy-relevant question or two, even if we are not interested in the sheep-goat effect per se.

In light of the centrality of cue-induced expectations in parapsychological studies, it is of interest that there are growing indications that cue-induced expectancies are of importance, also, in altered-states work, especially in hypnosis studies and, probably, in psychological studies in general (e.g., Council et al., 1986; Kirsch, 1985; and other studies cited in those publications). In particular, the available evidence (Council et al., 1986)—though there are methodological problems discussed elsewhere in this paper—suggests that the hypnotic induction itself might serve as a major manipulator of expectancies as subjects observe what happens during the induction and make attributions. These expectancies can, in turn, strongly influence behavior. The claim that expectancies are major mediating variables in response in the hypnosis setting has long been claimed by the social-psychological theorists of hypnosis (e.g., Barber, 1969; Barber, Spanos, & Chaves, 1974). What is of special interest here is that it would now appear that expectancies may have become the focus of psychologists' efforts to understand various effects in the hypnotic setting and, in particular, that the major effects of experimental cues that tip off subjects about experimenter expectancies (i.e., demand characteristics) may be mediated through the expectancy factor. (The discussion of expectancy effects in the literature is, however, considerably broader than the demand characteristics area.) This, by the way, shows even more clearly that the real-simulator design may be misconceived in its exclusive focus upon effects of demand characteristics that are not really mediated by expectancies (but by self-conscious compliance).

Here, then, is a major new development in the demand characteristics area. Many—though that is not to say all—effects related to demand characteristics may be expectancy-mediated. In short, experimenter expectancies become transferred to subjects, and thus subsequently affect both behaviors and subjects' interpretations of the meaning of their behaviors. This is a highly attributional analysis, but the theoretical framework of such thinking is far from complete. In my judgment, it does not at present adequately address the task before it. Far more conceptual and empirical work needs to be done to explain in some detail how expectancies can affect overt responding, in addition to explaining in detail how they affect subjects interpretations of their behavior in the hypnotic setting. Angelini and Stanford (1987) have developed and tested ideas to explain how the experience of involuntariness arises in such a context, but it is not predicated in a direct way upon expectations, but, rather, upon attentional factors and subsequent attributions. Our concept can easily explain how expectancies affect attributions of involuntariness because it is reasonable to assume that expectancies affect the locus of subjects' attention during administration of suggestions. With some additional, fairly simple, assumptions, our conceptualization might also explain how expectancies can facilitate behaviors. However, it seems to me that the leading expectancy theorists have a potentially difficult unaddressed problem before them that they have not addressed: Through what means do expectancies affect overt behavior? Some very explicit conceptualization is needed here. Here is another area in which parapsychologists might make contributions that could have direct relevance both to general psychological concerns and to parapsychology.

Thus, in hypnosis work much of the discussion of hypothesis-relevant cuing (demand characteristics) is presently developing around the concept of expectancies. Aside from the theoretical bluntness that presently exists in the expectancies area, there are also serious methodological difficulties. These center, as I have indicated, around the use of measures of subject predictions of suggested and not-directly-suggested effects (as well as the solicitation of hypnotic depth self-reports) that are reactive by their nature. Despite this fact, there seems little recognition in the field that having subjects make predictions about subsequent behavior may be reactive in the sense that they might never have made such predictions had they not been asked to do so and that the predictions in turn influence behavior. (Similar remarks about reactivity apply to the often-solicited depth reports.) What is needed now is a broader recognition that the operations used to study such effects are, to use a medical term metaphorically, invasive. They potentially bring into play in the hypnosis research area the problem of self-fulfilling prophecy, even while they may measure much more than the preexisting expectations of subjects. (Of course, none of this is to say that the present methods, even if they are reactive, have no value at all for their intended purposes. What is more, they may actually be examining the role of self-predictions and expectations, but in a way that is not immediately relevant to the original, intended question.) Here, again, is one of those very difficult problems of methodology that should challenge psychologists and parapsychologists alike to rise to the occasion.

Much has been said about possible methodologies that might be borrowed from psychology, especially from the hypnosis area. However, in pursuing the literature in hypnosis, I have developed a clear impression that methodologically, as well as theoretically, that field, like parapsychology, has a long way to go. The perpetual theoretical battles among special-process or state theorists and social psychological or social-constructionist theorists have also contributed to many heated discussions about methodology. Not surprisingly, researchers sometimes seem to prefer methods that support their theoretical predilections! Even the hypnotic susceptibility scales selected sometimes seem to vary according to theoretical camp of origin.

Hypnosis researchers at times still do studies without adequate controls for demand characteristics, even when they claim that they have instituted such controls—as was noted above under the real-simulator design. Sometimes, too, they have introduced potential confounds in using a design intended to provide such control. Very important, researchers have sometimes used highly reactive methodology. These trends continue to the present.

As concerns the continued and often quite naive use of the real-simulator design, there seems to be an attitude of methodological imperialism that says, "If a method has been around long enough, it has claimed the territory and is not to be dislodged, whatever the cost!" Of course, we see this in all fields and parapsychology has been no exception.

There has been some real methodological progress in the hypnosis field. A hallmark here has been the recognition of the importance of getting reports about the subjective aspects of response to suggestions after the entire session has ended. Probably both special-process and social-psychological theorists will agree about this (if about nothing else). Special-process theorists need such reports in order to try to ascertain whether the defining characteristics of hypnosis are fully present, whereas social-psychological theorists need such reports in order to learn what features of the total situation influence the kinds of attributions that lead to reports of, for example, involuntariness. Whether or not the subject experienced inwardly the things suggested by the hypnotist and whether suggested behavior was felt or inferred to be involuntary are topics that have come to the fore in the last two decades of hypnosis research. The development of methods for assessing such things has led to important empirical advances, even if it has not erased theoretical conflicts.

The most important reason for relatively slow methodological advance in hypnosis and related psychological fields may actually be the absence of any better suggestions about how to do things. There is no doubt that altered states research (or internal states research, to use a somewhat different concept) is intrinsically very difficult. It should be clear that in the case of the hypnosis-suggestibility area, the parapsychologist might be able not only to borrow tools and concepts, but to contribute to the refinement of the methodology and to the empirical and conceptual development of that domain.

## *Pitfalls and Possible Solutions*

*The Delusion of Operation Omnipotence.* The belief that if one applies a fixed set of "altered-states-favoring" operations to one's subjects, they will develop such a state, is the delusion of operation omnipotence. This belief can be implicit in one's actions, as when one acts as though such operations must be effective for all subjects. Much of what was said earlier about the importance of finding test instruments to identify individual capacity and/or inclination for internal attention states or, perhaps, altered states of consciousness was predicated upon the assumption that merely putting an individual through a hypnotic-induction ritual or exposing him or her to the ganzfeld does little if anything to insure that this person is consequently in either an internal attention state or an altered state of consciousness. If we through prior testing have evidence that the individual is capable of successful response to an hypnotic induction (through use of a standard hypnotic susceptibility scale) or is inclined toward experiencing inward attention states (through administration of the Absorption Scale), it is reasonable to assume that if other considerations are right, the person will respond positively to the set of operations to which he or she is exposed and will thereby enter an "internal attention state" or perhaps an altered state of consciousness." However, there is nothing to guarantee that this will happen, even if it is relatively likely (as we know from prior psychological testing).

If, as in the case of many psi studies, the experimental hypothesis more or less assumes that the operations do affect the individual in the desired way, we should either (a) use measures of whether or not the subject actually does respond as desired to our operations and/or (b) use additional measures to further increase the accuracy of our prediction that the individual subject will respond well to our set of operations. Let us consider the latter first.

*Better prediction.* Investigators in the hypnosis area have over the years amassed considerable evidence that motivation for hypnosis (e.g., interest in and freedom from potentially frightening ideas about it) and a trusting relationship with the hypnotist are important. (For a review of some related evidence see Barber, Spanos, & Chaves, 1974.) These are precisely the elements that are never measured in many studies involving hypnosis. It is reasonable to assume that even persons interested in experiencing hypnosis—as are most persons who volunteer to participate in ESP-hypnosis studies—might not be ready to let themselves go and respond as a "deeply hypnotized subject" if there is something about the hypnotist-experimenter that evokes a negative or uncertain reaction of any kind. Much the same can be said with regard to the experimenter in charge of ganzfeld.

These special situations in which the subject feels very passive and even dependent upon the hypnotist or experimenter may be likely to stir up anxieties or uneasiness, even of a nonverbalized kind, unless there is a feeling of trust and a degree of liking for the experimenter. This might always be true in some degree in experiments, but it may be especially likely in those in which the subject is uncertain about what he or she should really be doing (as in trying to use ESP) and so feels a special dependency upon the experimenter. There is, however, every reason to think this would be especially true when an uncertain task like taking an ESP test is combined with a somewhat mysterious and potentially conflict-producing situation such as hypnosis or ganzfeld. We probably need to know much more about the interpersonal elements in such studies than we know at present.

Ideally, reactions to the experimenter (or hypnotist) should be carefully assessed. This might often best be accomplished by some other person than the hypnotist or primary experimenter. (Of course, it must be assessed before the subject has any feedback about ESP-task performance.) Additionally, motivation for or interest in experiencing the particular procedure used should probably routinely be assessed. Such assessment must, of course, follow efforts to insure that the subject understands what the experience will probably be like. Since such assessment of liking-disliking for the task can be influenced by social demands, it might be best to measure subjects' preferences (or even select subjects) by having them rank-order their preference for available studies after having been somewhat familiarized with them. Such measures could help to enhance prediction of who will and who will not respond favorably to a special situation such as hypnosis or ganzfeld.

*Measuring the Desired State of the Subject.* Probably one of the most useful ways to measure the degree to which the subject actually experienced hypnosis in the traditional sense of that term is to use post-session retrospection relative to classical elements of the hypnotic experience, such as involuntariness, and subjective realness of the thing suggested. We have earlier discussed how such things can be measured. Such measurements may be especially important for learning whether the subject responded favorably to suggestions of the type(s) deemed to be most important to the psi task and whether the experience of involuntariness really occurred (since the latter may help the subject to break away from egocentric approaches to the ESP task). The use of Field's (1965) Inventory of Hypnotic Depth might be a useful general tool here, as was noted above.

In the case of operations, such as ganzfeld or meditation tasks, in which the experimenter is less active than with hypnosis, knowing whether the subject entered an internal attention state or an altered state (if one believes that either construct is useful) poses greater difficulties. Earlier I mentioned the possibility that Pekala's Phenomenology of Consciousness Questionnaire should perhaps be explored, but I have considerable reservations about the present state of the evidence as to the validity of this instrument. Still, its use in an exploratory way would be warranted. I believe that in the long run parapsychologists (and psychologists) might make their most meaningful contributions to solving such problems by using a method that is both nonreactive and much more free from demand characteristics than any form of contemporaneous or retrospective inquiry. I am referring to the use of transcripts of session utterances to develop measures of the psychological condition of the subject, including internal attention states. This approach is in its early infancy, but analysis of session transcripts represents a growing methodological tool in psychology generally. It is surely a tool that should hold promise here if we are willing and ready to devote the required time and effort to its development.

Stanford, Kass, and Cutler (1988a) found significant, replicated evidence that a certain temporal trend in the rate at which subjects speak in the ganzfeld session may be indicative of entry into and function within an internal attention state, as evidenced by its consistent correlation with Tellegen's Absorption Scale, the latter being probably the best indicator of the trait that favors the development of such attention states. This development should greatly encourage the effort to find verbal indicators of entry into and function with an internal attention state. There are no good reasons why this approach cannot be developed much further than it has been at present. If parapsychologists become involved in this effort, they might simultaneously enhance the investigator's ability to predict psi-task performance and contribute meaningfully to the emergent psychology of internal attention states.

The problem here is a tricky one. We are trying to develop methods of measuring entry into and function within special states, but we do not already have any proven, reliable measures of those things against which new measures can be compared as criteria. I nevertheless believe that progress is possible, but this is not the place to discuss precisely the approach that my student colleagues and I hope to use in our own laboratory. Suffice it to say that we hope to make use of convergent operations in trying to pinpoint the markers that will be useful indicators of the states we are seeking. I would suggest that investigators in this area not be blind to the possibility that some such markers could be somewhat idiosyncratic to the individual subject. Therefore, longer-term work with individual subjects might prove useful, as well as work with groups of subjects, the latter being what we have done to date.

*Failures to Test the Assumption of Functional Equivalence.* Recently, thanks in part to financial support from the Parapsychology Foundation, Inc., I have been assembling and critically examining the entire published literature on scientific studies of out-of-body experiences (OBEs). The effort to digest that material is still underway. An OBE must represent an altered state of consciousness, almost by definition. Clearly, an individual in such a state is processing the usual information about the world in an uncharacteristic way and is presumably overlaying it with imaginal information. Given this obvious commonality among OBEs, it is perhaps not surprising that almost all of the empirical work on the psychology of that experience has operated off the assumption that all OBEs are functionally equivalent, regardless of the state of mind during which they were reported to have developed and regardless of the external circumstances that seemed to have supported or initiated the OBE.

This assumption may often be untenable on empirical grounds—i.e., it may simply sometimes be counterfactual—and it can, in any event, be questioned on conceptual grounds. Much of the research on psychological correlates of the OBE is aimed at discovering what developmental, personality, or cognitive factors (in interaction with life situations) either allow or predispose an individual to experience the OBE. It seems reasonable to assume, as a starting point that is itself subjected to empirical examination, that OBEs developing out of differing states of mind will in many instances not correlate similarly with psychological variables. This is because, for example, the cognitive skills and personal inclinations that allow a person to experience an OBE while ostensibly wide awake with eyes open could be radically different than those that allow such an experience while the individual is dreaming or, perhaps, even while the individual is falling off to sleep. Reality testing is very different in the latter situations and, especially, during dreaming.

The potentially dangerous assumption of functional equivalence usually leads to *a priori* pooling of data for a correlational analysis regardless of the state of consciousness in which an OBE has been reported to have occurred. The implicit assumption behind such pooling must be deliberately and selfconsciously examined. Otherwise, we may find ourselves entertaining some very erroneous conclusions. The pooling of data across such varieties of experiences can only be justified by *a priori* demonstration that classes of OBEs thus pooled are for these specific purposes functionally equivalent. For example, it must be demonstrated, if one is examining whether a history of severe childhood punishment is associated with subsequent OBE reporting, that the classes of OBEs thus pooled to observe their correlation do not have differing degrees (or directions) of correlation with the variable of interest (childhood punishment). If they do differ in this regard by some liberal statistical criterion, pooling them could lead to extremely misleading conclusions.

This is a point that seems to have been lost in almost all of the scientific reports to date that examine psychological correlates of spontaneous OBEs. The point is of more than academic importance because in a recent study I found that developmental correlates of the OBE differ according to the state of consciousness in which individuals typically report OBEs as having occurred (Stanford, 1987b). We are in our laboratory currently examining the outcomes of a more recent such study in which state of consciousness appears to be a critical moderator of certain of the correlations between OBE reporting and psychological variables.

While the previous discussion has concerned the psychological study of the OBE, there are reasons to suppose that the same concerns about state of mind have relevance to learning whether ESP-task performance is superior among persons who have experienced the OBE. The same concerns should also be generalized to efforts to predict, say, waking ESP-task performance on the basis of questions about ostensible spontaneous psi experiences. Should persons who report all or almost all of their spontaneous "psi" experiences as emerging from dreaming be assumed to have the same likelihood of performing in a waking ESP task as those who report such experiences as emerging entirely or largely from a waking situation? I do not believe that sufficient attention has been given to state of consciousness (and perhaps setting) factors in work that seeks to predict ESP-task performance in the laboratory. Whether psi manifestations "prefer" one state of mind in a given individual is an empirical question. Whether there is a general psi proclivity that transcends state of mind is also an empirical question. The central point here is that we must be aware of the assumptions we make and must subject them to empirical examination.

### *Two of Parapsychology's Biggest Problems in Internal States of Research*

*Improper Use of Same-Subject Designs.* Elsewhere (Stanford, 1987a), I have discussed at length how the rather persistent use of same-subjects designs in hypnosis-ESP work has created several kinds of ambiguity for interpreting the findings contrasting a hypnosis and a control condition. Readers are referred to that discussion for details. Continued use of such designs can only retard progress in this area unless they are used for the explicit purpose of illuminating the undesired consequences of such designs in this kind of work (or for some of the few other questions for which such designs would be appropriate).

*Subject Selection Problems (Non-Random Assignment).* Ironically, in the five instances in which parapsychologists studying hypnosis have been able to avoid the pitfall of same-subjects designs, they have stepped into another, one at least equally dangerous, but in very different ways: the problem of violating the requirement that subjects be randomly assigned to conditions (e.g., to hypnosis and control conditions) (Stanford, 1987a). Random assignment is a requirement both for statistical evaluation of such work and for interpreting the meaning of any observed difference in performance across conditions. Efforts to meet this requirement involve a number of complications related to recruitment, informed consent, and keeping the psychology constant for experimental and control conditions. There is not the opportunity to discuss these issues here.

### *The ESP Measure in Internal States Work*

Other of the speakers will hopefully address the problem of developing more sensitive measures of ESP-task performance in the free-response setting, the setting typically used in internal states research. Here I would note that all manner of refinements in the areas discussed earlier about predicting and measuring internal states will yield little net gain for parapsychology unless our measures of ESP-task performance are reasonably sensitive and reliable. That is a difficult and complex assignment and one that requires considerable further work. The free-response ESP-task success of a particular subject will, if the subject is his or her own judge (of picture-utterance similarities), depend upon, minimally, the following factors: (a) encoding of psi-mediated material into conscious cognitions and/or perception-like units; (b) attention to the psi-mediated material as contrasted with the various sources of noise in the mentation; (c) reporting of the psi-mediated information (in some balanced relationship to irrelevant information); and (d) the complex and difficult task of judging picture-utterance correspondences.

The latter may be a particular source of error variance (or systematic bias, depending upon what problem one is studying) when the subject is judging. This is why I have opted, recently, for judging by more than one experienced outside judge when doing process-oriented ganzfeld-ESP research. In order to help insure maximal access to subjects' thoughts during the session, the external judges have also had access to supplementary comments by subjects, comments made after their hearing read back to them notes on their spontaneous utterances during the session, but while they are still in ganzfeld (except that noise is no longer introduced). This approach is worthy of further exploration and comparison with results using subject judgments.

It is possible that some of the refinements in judging and/or score development being discussed by various researchers will help further reduce the problems of error of measurement in free-response ESP-task performance, even when subjects are their own judges. I would also note that choice of pictures (or other stimuli) for targets and foils (or decoys) is a critically important matter to which much attention needs to be devoted. Probably others will address this matter to some degree.

### *Re-examining the Alleged Resistances of Subjects Who Seem Ready to Let Us Down*

There has been considerable discussion in the parapsychological literature about alleged deep-lying fears of psi phenomena, especially of strong psi phenomena. (For reviews, evidence, and related discussion see Tart, 1984, and Tart & Labore, 1986.) My purpose here is not to dispute that fears of such events sometimes exist and can affect the performance of experimenters and subjects. I have no quarrel with that. I do wish, however, to suggest that some behaviors on the part of subjects that have sometimes been construed as indicating fears and related resistances to demonstrating psi phenomena might have another interpretation, one now widely known among social, personality, and clinical psychologists, but which has never, to my knowledge, been discussed in parapsychological writings.

Let us now look more closely at the subject behaviors sometimes interpreted as being due to fear and/or resistance to producing psi phenomena. Subjects who have been succeeding at psi tasks in the laboratory sometimes seem strongly inclined to do things that would seem to lower expectations (their own and those of the experimenter) for subsequent success on the psi task. Some parapsychologists have privately indicated to me that they are sure in such instances that subjects, having discovered their powerful psi abilities, are becoming ridden with fears about those events. I would suggest that there is an alternate explanation that should be considered for such a turn of events.

The proposed alternate explanation is known as "self-handicapping." Basically, self-handicapping occurs when a person either creates or allows a situation to develop that will allow a handy excuse for failure at some task. In an early paper, Berglas and Jones (1978) considered self-handicapping to be "any action or choice of performance setting that enhances the opportunity to externalize (or excuse) failure and to internalize (reasonably accept responsibility for) success" (p. 406). This means that if failure occurs, it can be blamed on something other than inability or incompetence, but that if success occurs, real ability is strongly inferred. The implicit ideas here go back even further to early attribution theory (Kelley, 1973). H. H. Kelley proposed that if more than one cause for a given event is present and the event occurs, the presence of one or more alternative plausible causes means that in interpreting the event, a given single possible cause for the event will tend to be discounted or given less emphasis. This is known as the *discounting principle.* One deduction from this principle is that if both an obstacle to performance and a lack of ability could explain a person's failure on a task, failure in the presence of an obstacle to success is less likely to be attributed to lack of ability. On the other hand, if something happens and there are present both facilitating and inhibiting causes, the role of the facilitating cause will be emphasized in attempts to explain it. This is termed the *augmentation principle.* Thus, if a person is trying to do something (facilitative cause) and the task is very difficult (inhibitory cause), success will mean that the facilitative cause will be given emphasis in explanation—the person will be seen as having great ability. If one can arrange a strong inhibitory cause, then success means that one can think very well of oneself. If failure occurs under the same circumstance, one need not think badly of oneself; one can continue to believe in one's competence. (The same analysis applies to others' making judgments about oneself under such circumstances.)

It is, therefore, of considerable interest that several parapsychological investigators with whom I have talked have reported what appears to be self-handicapping behavior by subjects after those subjects have already established some reputation at the task. (This is why some investigators have suggested that this is all "psi resistance" due to strong fears stirred up by the feeling that the subject might have strong psi.) The subject, in terms of the self-handicapping construct, had begun to build a sense of private self-esteem (not to mention, public image) around being a "good psi subject." The continued experimental work posed a threat to that private self-esteem (and to that public image), but the use of self-handicapping would help reduce the threat.

It is of considerable interest here that the psychologists who study self esteem have discovered that the research paradigm that most reliably elicits it is one of prior *noncontingent* success, in other words, success not clearly dependent upon the magnitude or quality of the subject's own efforts. Anyone familiar with psi research will know that this is exactly the distressing circumstance of the vast majority of subjects who succeed at our laboratory tasks! They are having success and would like to continue it, but they just do not know what to do to help insure success because they are unsure of how they accomplished their success in the first place.

Researchers have found that with feedback about noncontingent success—essentially, what one has in psi-task performance, from the standpoint of most subjects' perceptions—subjects will, if given the opportunity, work under a difficult circumstance (such as performing under the influence of an inhibiting drug, Berglas & Jones, 1978, or under some distracting condition) or may elect to participate in a task that has no diagnostic ability (relative to the area in which success has been established) (Sachs, 1982). Interestingly, subjects who are faced with such prospects of failure will sometimes elect to take drugs that would deter success, such as alcohol, prior to performance (e.g., Kolditz & Arkin, 1982; Tucker, Vuchinich, & Sobell, 1981, as cited in a review in Leary & Miller, 1986, Ch. 4, "Self Processes and Behavioral Problems"). All this might remind parapsychologists of some things that have happened with certain of our special subjects.

Roy F. Baumeister and Steven J. Scher (1988) in a review of self-handicapping and other topics related to self-defeating behavior regard self-handicapping as "tradeoff" that sacrifices one's chances for success in exchange for attribution-related benefits (protection from implications of failure, but special credit for success). In support of this tradeoff concept, they cite work by Greenberg, Pyszczynski, and Paisley (1984) that showed self-handicapping when the stakes were low, but not when a large amount of money was potentially available. In the latter case, persons dispensed with self-handicapping and apparently tried their best. This suggests a cost-benefits analysis relative to the choice of self-handicapping or not (for at least certain subjects).

Every teacher knows that if a student has doubts about his or her ability to deal with the material and thus has doubts about success, such a student will often exert minimal or no effort to succeed. Here, self-esteem could be protected and apparently is, even at the cost of a poor grade. This is surely a form of self-defeating behavior, viewed from the perspective of the outsider. (By the way, no teacher would be ready to allege fear of success in such a situation!) Some studies have shown a reduction of effort to be a way of avoiding the negative implications of failure (e.g., Harris & Snyder, 1986; Tice & Baumeister, 1984, cited in Harris & Snyder, 1986); however, the Harris & Snyder findings held only for male subjects who were uncertain of their answers to questions related to self-esteem).

There are, however, hints in the literature about how one can get persons who are frustrated by previous failure on a task to engage their efforts and work very hard. This is by giving a task that is described as very difficult, for it carries its own excuse for failure that protects private self-esteem (and public self-image) (e.g., A. Frankel & Snyder, 1978). Such a task can be preferred over one of moderate difficulty, according to the research cited.

Other self-handicapping strategies include insufficient practice or preparation prior to an important evaluation of some kind, something that apparently happens mainly among individuals who are inclined to use self-handicapping as a protective strategy (Rhodewalt, Saltzman, & Wittmer, 1984). Some self-handicapping researchers have developed and used inventories to pinpoint individuals particularly prone to such strategies, as in the study just cited, which concerned the preparation of athletes for contests. Interestingly, individuals inclined to use such strategies appear to be more likely to use them prior to important contests. Parapsychologists might benefit by the use of one or more such inventories, especially when they are undertaking longterm work with particular or "special" subjects. This could alert one to which individuals are likely to use self-handicapping strategies in the face of the uncertainty that always attaches to parapsychological success (just as it attaches to success in athletic contests, for which ultimate success depends upon one's own abilities, one's efforts, *and* those of the rivals, not to mention "chance" factors).

Baumeister and Scher (1988) in reviewing self-handicapping note two general forms it can take: the creation of obstacles to one's own success that can serve as an excuse in the event of failure; and citation of external excuses that can be conceived to have interfered with success. The first is obviously in some sense self-defeating in that it at least increases the likelihood of failure. It can take many forms, some of which I have mentioned above. They include taking alcohol or drugs, failure to practice adequately and deliberate low effort. Very insidiously, this can include the choice of a very difficult goal, whereby one cannot be blamed for failure (Greenberg, 1985, cited in Baumeister & Scher, 1988). The second form mentioned in the review just cited can take the form of reported test anxiety or poor mood. As Baumeister and Scher note, whether such excuses are truly self-destructive in character will depend upon whether they are merely cited retrospectively or are somehow actively fostered in anticipation of possible failure. In practice, it is difficult to know whether such excuses refer to realities or fictions. However, there is evidence to suggest that persons can make such things as test anxiety, bad moods, and troubling bodies quite real to themselves on habitual bases, using them handily when they would help to preserve self-esteem (or allow impression management). (For a most interesting, even exciting, review of such research, see Leary & Miller, 1986, Ch. 4, pp. 56–58).

Whether self-handicapping really subserves private self-esteem or public self-image (impression management) is sometimes debated in the social psychological literature. Kolditz and Arkin (1982) addressed this issue by manipulating whether others did or did not know about the self-handicapping circumstance. (If there is no role of public self-image in all this, such a manipulation should make no difference; everything would be predicated upon what one can believe about oneself.) Their study supported the impression-management interpretation. However, there is currently no reason to suppose that self-handicapping is only of an impression-management kind. It would in many instances simultaneously serve both impression-management and self-esteem-protective functions.

The issue of self-esteem versus impression management is, in some respects, a somewhat artificial one. There can be little doubt that self-esteem is meaningfully and regularly affected by our observations of the impressions we make on others and by how they react to us and our efforts. Even the very concept of self has in the view of some commentators been fundamentally linked to how we see others react to ourselves; the self sense can only develop in interaction with others, according to symbolic interactionists such as George Herbert Mead (as discussed in Baldwin, 1986). The question does take on some pragmatic interest for parapsychological researchers, however, because if self-esteem is the primary instigator of self-handicapping, it is possible that subjects will self-handicap and we will never learn about it. If impression-management is involved, on the other hand, even if the selfhandicapping occurs in private, it would seem likely that the experimenter would somehow be let know about it. Otherwise, the ploy would not work. If investigators are clever and are insightful about junctures at which selfhandicapping is likely to occur, they might be able to control the type that occurs and thereby manage the situation a bit. Some systematic study of the consequences of deliberately providing self-handicapping opportunities is needed. I would also suggest the importance of learning who is most inclined to use such a strategy and who is not. One way of identifying persons inclined toward self-handicapping is to give the subject the opportunity to select a self-handicapping circumstance in a situation that could be expected to trigger self-handicapping in persons thus inclined.

The implications for actual task success of having a self-handicap are less clear, at least on the basis of my personal knowledge of the current literature. Probably the answer is quite complex, but this should not deter parapsychologists from looking for an answer. Actually, that answer would probably depend upon a combination of circumstances and, as such, might vary from situation to situation.

What is clear from the above analysis is that there is every reason to expect to find self-handicapping in parapsychological settings, especially at critical junctures in a subject's work, because of the experienced non-contingency of success. As noted by Baumeister & Scher (1988), "The central cause of self-handicapping appears to be some form of induced insecurity about future performances, especially when coupled with high external expectations for success" (p. 8). The latter consideration combined with the experiential lack of a sense of outcomes being contingent upon what one does must surely make self-handicapping a major concern of many psi-test subjects and, therefore, of psi researchers. Researchers need to bear these things in mind and not automatically construe as "resistance" to success the potentially harmful things subjects do to themselves, claim about themselves, or select as test circumstances. Resistance to the perception of personal failure may be more likely than resistance to success, given the experiential noncontingency of the whole situation!

What has this to do with research on internal attention states and parapsychology? First, self-handicapping in psi research is a broader issue and, as such, is worthy, in its own right, of mention in a conference devoted to methodology. It provides a potentially helpful new way of interpreting certain behaviors frequently noticed in subjects. It could lead to more understanding ways of responding to such situations. When the subject really fears potential harm to his or her image in the eyes of the experimenter, but that experimenter feels sure the problem is resistance to psi, this represents a form of social insensitivity to the consequences of what we are doing to subjects. (Of course, this does not mean that fear of psi and resistance to it may not sometimes emerge as real factors. My personal guess is, however, that self-handicapping related to perceived noncontingency is more often the culprit.)

Possibly self-handicapping is most likely to appear in internal states research when there is extended experimentation with a given subject or when the subject already has a reputation to defend in the laboratory. For first-timers in hypnosis-ESP work or ganzfeld the magic of being exposed to an elaborate setting or technique that is intended to "cause it to happen" might lessen the sense of ego-involvement that can favor self-handicapping. The subjects' interpretation of the situation may be such that they tend to view themselves as very passive and waiting for the magic to make psi happen. If it does not happen, maybe the effort just somehow did not "take." On the other hand, continued, regular success in such a setting creates a reputation—the ability to function as a psi specialist in such a setting—that must somehow be defended (even if one is unsure how one "makes it happen," but has only some half-baked ideas that never get solid confirmation). The same thing might be true with a self-proclaimed psychic in these settings. Here some opportunity for self-handicapping might be welcomed and might help minimize the egocentric involvement that might deter success. Sometimes it might come in the form of a statement from the experimenter that, "Of course, I recognize that you have never worked under precisely these circumstances before."

Chronic self-handicappers might well be recognized by their readiness to come to the session with a built-in excuse for failure, such as "I sure didn't get much sleep last night, but I thought I would come up and try anyway." Perhaps I am wrong, but my intuition says that one will find quite a few self-proclaimed psychics ready to use self-handicapping. Part of this might derive from their intrinsic uncertainty about how they do what they do and the difficulty of their meaningfully preparing for a session—it is not quite like practicing a difficult piece on the piano before a concert. There may be other factors here, too.

One of the nice things about techniques such as ganzfeld and hypnosis is that they, as special settings, do not directly raise expectations about the person's ability to do "psychic things" in other settings. Even considerable success in such a setting need not increase the subject's feelings that the experimenter will automatically expect success in another setting. Thus, the subject does not have to worry about being "put on the spot" in that regard. If the subject does try a different task (e.g., a non-ganzfeld one), he or she comes to it without any prior commitment to success—it is *so* different as compared with ganzfeld—and he or she need not be embarrassed about failure.

Another way in which altered-states-related self-handicapping can occur is use by subjects of drugs such as alcohol or marijuana that, according to folklore, can *in just the right amounts* facilitate psi performance by "taking off the edge" and getting a person to relax, be unanalytical, and spontaneous. However, with *just a bit too much*, the same drugs, according to the same folklore, can deter psi performance. So, what better self-handicapping device in the face of a potentially threatening psi task than to tell the experimenter—whether or not it is truthful—that one has been "priming" for the study and hopes one has gone just far enough but not too far? With success, one might look clever and resourceful, but failure would mean, not that one's powers had failed, but that one had accidentally gone "over the line" in a special preparation one had tried to make. "Sorry!" Of course, this can also protect one's self-image as a successful psychic.

The concept of self-handicapping deserves the serious consideration of the working parapsychologist, who should probably make every effort to examine the relevant psychological literature. It might provide a much-needed new way of looking at some old problems in psi research, problems that are almost unavoidable when a subject has created some degree of reputation in the parapsychology laboratory or initially comes to the laboratory with a reputation to defend. The opportunity for the parapsychologist comes in understanding how this problem might develop and manifest itself in a particular study or, perhaps, in the individual subject.

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## *DISCUSSION*

EDGE: I think you provided us with good recommendations that parapsychologists, whenever possible, ought to stay away from the CURSS, from the Barber Suggestibility Scale.

STANFORD: I did not comment really negatively on the CURSS; actually the CURSS is all right, except perhaps for the very brief induction.

EDGE: . . . and from naked absorption. Now, that is what we have to look out for.

HONORTON: I am not sure really what all this has to do with the kind of internal attention states research that predominates in parapsychology today. And I am wondering whether you would say a little bit about the various versions of the hypnotic susceptibility scales. What does this have to do with ganzfeld research?

STANFORD: Hypnosis research?

HONORTON: Are you suggesting basically that we should be focusing much more on hypnosis per se or using models and methods from hypnosis? What really is the message here?

STANFORD: I will not discuss the paper that I completed and then hid somewhere. You can have a copy of that, too, if you want. In this particular paper I am focusing on the idea that we need to know something about the skills of the people who come into an internal states experience setting. Do they have the skills that would allow them to be the kind of people in that setting that we wish them to be when we invite them in? Hypnotic susceptibility scales can help us to know for sure, especially a prudent choice of scale. If we want to use suggestions of a particular type, such as hypnotically induced dreaming, there are ways of premeasuring that capacity using some of the standard scales. We need to establish that subjects actually experience involuntariness before we can test the role of involuntariness. There is a real chance that the experience of involuntariness is very central to getting something to happen under hypnosis vis-à-vis psi because of that kind of extra ego feeling that something is happening to the individual that is interesting and exciting.

HONORTON: What constitutes an adequate control condition for an internal attention states experiment?

STANFORD: What constitutes an adequate control for an internal attention states experiment? What constitutes an adequate control depends on what hypothesis you are testing. If you have a hypothesis about why, say, ganzfeld favors ESP task performance then you have obviously got to have a control group that tries to hold everything constant except that particular factor. I am not going to say that is always necessary, but that is what you have to got to do. But the notion that there is an abstract kind of magical control group that you need for ganzfeld experiments is, in my opinion, simply wrong. It depends on the type of conclusion you want to make from ganzfeld experiments. But if you are just asking if this condition is favorable to the occurrence of psi, you just find out if psi occurs there or not. But that does not tell you anything about why it occurs.

SCHOUTEN: Well, I agree with Rex in the sense that my feeling is that of course you are never certain about what the control is. But in the case of control of the ganzfeld specifically, I always had the naive feeling that the procedure itself, isolating the perceptual input, is supposed to create the altered state which was supposed to be psi enhancive. So I figured in this case that logically the first control would be to compare two situations in which either the subject is isolated in a specific ganzfeld way by having the ping-pong balls on versus having exactly the same situation without the ping-pong balls on. Now that is not a perfect control, I immediately agree to that, but let us say at least it would tell you something about the assumptions mentioned above if the experiment had given a clear answer which I would have loved to see. You can expand on that, but the point is I think that is the first sort of control I would look at.

STANFORD: That is probably because you wonder if a uniform visual field is an important factor. If that is one's hypothesis, it seems to me that is the route one would go, if you think that uniform visual stimulation is an important factor.

SCHOUTEN: We are dealing here with the state itself. That is supposed to be the real factor. Now ganzfeld is supposed to enhance that state, to bring subjects into the state. If you have an independent way to measure the state then I would say that that is the way to go with it, but I doubt whether you have.

STANFORD: That is one of the things psychology lacks for a given setting, be it hypnosis or in our case perhaps most important the ganzfeld right now. We do not have an assured way of knowing whether a person is in an internal attention state when he is in ganzfeld. We can predict whether they might be by using things like the absorption scale, inventory of childhood imaginings and memories, fantasy proneness, other personality scales and maybe hypnotic susceptibility all bundled up together. Then we would get people who are high on all of those and almost guarantee that you would find somebody in the ganzfeld who would experience an internal attention state. But maybe they do not like the person who is running the ganzfeld, they do not want to open themselves up or something like that. We have been working with trying to develop objective measures of entry into an internal attention state. I do not have time to get into this here, but in the Stanford, Kass and Cutler paper presented at the 1987 PA Convention you will find our work on this. I think we are making headway and we have some interesting leads that we want to follow-up, too, and we are going to continue work on it. It is not an easy question. I discuss this in my paper to some degree.

HONORTON: I would argue, Rex, that with respect to the ganzfeld in particular, the ganzfeld is by definition an environment in which the individual is in an internal attention state because he is either asleep or he is in an internal attention state. There is no other possibility in the ganzfeld.

STANFORD: Well, I do not think that that is necessarily true. I have had subjects sit in there and talk about the setting, the room or the chair or their personal comfort or this or that. When we speak of an internal attention state we mean a certain type of passive attention.

HONORTON: Well, my definition of it is in my Wolman chapter which is what I have been using all these years. Simply, an internal attention state is a condition in which the individual is awake and is alert, but is focusing on and responding to internally as opposed to externally generated stimuli.

STANFORD: My response to that is you can lead a horse to water, but you cannot make him drink.

SCHOUTEN: I think so, too. The little experience I have had in this area is that it is perfectly possible that a subject who has the ping-pong balls on starts fantasizing and triggers all sort of fantasizing processes which you can do with or without the ping-pong balls. The ganzfeld in itself is not a guarantee that subjects enter an altered state. Just defining an altered state by the method one applies doesn't really help us.

STANFORD: Some of our people on occasion sleep quite a bit.

MAY: On this point I have to say I disagree with Chuck. To assume that someone is focusing his attention on internal processes is an enormous assumption. I am expanding on Rex's comment to you because, personally, when I get in there I am looking at the damn ping-pong balls. I am not paying the slightest bit of attention to what is going on in my mind. For me personally the procedure is very distracting. Now is that an internal attention state?

STANFORD: If you know in advance about the blank-out effect that occurs where you cannot tell whether your eyes are open or closed, you can be sitting waiting for that to happen. It is kind of cute when it happens but it is actually a physiological event.

MAY: But is that internal attention? You see that might fit his definition of internal attention.

STANFORD: I do not think so, not in the sense that Chuck really wants to get at things that are being generated in the head that might match up with targets.

HONORTON: I think it is important to understand in terms of the origin of the ganzfeld that the idea was not that the ganzfeld produces some kind of weird state. The idea was that the ganzfeld would provide a way of approximating the kinds of "altered states" that have traditionally been associated with psi, particularly dreaming.

STANFORD: Well, I understand that, but one thing I have an unshakable conviction about is that the ganzfeld does favor psi. We get people in the ganzfeld in our lab who experience an internal attention state there that fortunately they do not do in my class lectures. We have people who are zonked, plastered to the chair, so to speak and we have people in the ganzfeld who are very much, by almost any definition you use, in their ordinary frame of mind when they are talking in there. And this is why we need ancillary devices to try to pinpoint what is happening internally to help to predict it. This is not to detract from the importance of the ganzfeld as an instrument in parapsychology. I think it is extremely important precisely because it does set up a circumstance that very few of us have ever experienced where we just do not have distractions.

HONORTON: That is the point.

STANFORD: And that is very, very important.

BRAUD: There is a suggestibility scale by Sheryl Wilson and Ted Barber called the Creative Imagination Scale. It is a very non-authoritarian one. I would like to know your thoughts about that scale.

STANFORD: Yes, the Creative Imagination Scale is great provided you are interested in what they are interested in, which is basically fantasy proneness as such. It might be really useful for something like the ganzfeld where you are interested in fantasy proneness as a moderating variable of the effects that you get in ganzfeld. But if you are really interested in hypnotic susceptibility, it is probable that you should use one of the standard scales and, I would suggest, supplemented by inquiries about subjective experience. The reason is that it has been shown in the literature that the CIS does not have the same psychometric properties as the major hypnotic susceptibility scales. There are a number of studies that show that now. There does seem to be a pure imaginal factor in the hypnosis domain. For instance, if you score the CURSS for subjective or objective involuntariness it gives you a one-factor scale. Nick Spanos is the original author of the CURSS; he believes it is related to fantasy proneness. But if you look at the behavioral response, it is something else. Now behavioral response may be important in addition to subjective response. There are people who do not produce behavioral responses in the CIS, as you know, but the behavioral response may be important because it feeds into the attributional process. It gives feedback to the subject that something outside his ego is happening. This may really be important for the consequences of our hypnosis experiments in particular. So for that reason I would suggest using one of the standard scales, ideally the Rolls-Royce of the scales the ISHSS:C and supplementing it with some subjective reports afterwards. By the way, I have got to mention something that is related to what you said. If we want to find out what people experience under hypnosis, I suggest we not do it by interrupting the process with a bunch of state reports. That is another problem with state reports. What we need to do is to use tools like Field's (1965) Inventory of Hypnotic Depth that ask questions related to standard classical effects that people experience under hypnosis. This has been used many times in the literature, a lot is known about it. It could be extremely useful to us as an after-the-fact retrospection about whether a person was experiencing classical hypnosis or not.

PALMER: This paper is very timely for me because I have been considering using hypnotic susceptibility scales as a screening device for my subliminal perception research. You are not the first one who has recommended the Stanford C in that connection.

STANFORD: Tart told us that years ago.

PALMER: I have not gotten into this in much detail yet, but my understanding is that many, if not most of, the Stanford C items are rather hard to pass because it is an advanced scale. When you are using it as a general screening device you may find your few virtuosos, but also you are going to give a lot of other people failure experiences that could conceivably influence their mental state. This could be a problem if you want to test them in other kinds of experiments. One possible solution is to give the Stanford A and B first.

STANFORD: Some people do that, but there are a lot of studies where they do not. You could do that because it does not have those items and you can use the other for later screening purposes. You can use a stage process, but often we do not have time for that; also it is expensive.

PALMER: Well, that is the problem, so we are probably going to end up using just the Stanford C. I would like your thoughts about whether the concern I have just expressed is something I ought to worry about.

STANFORD: I think it would depend upon what you actually wanted to do with the results of it. We know that hypnotic susceptibility is pretty stable over a period of time. People are as susceptible as they are. They are going to find out sooner or later in your study. If you fully measure ahead of time how susceptible they are, there is the chance for negative feedback. You could conceivably modify any scale so that you can null out the effects of negative suggestions. But let me point out that all these scales are arranged in theory as a kind of Guttman scale where the items get progressively more difficult in a step-wise order as to how many people pass. People are not affected by failure up to a certain point on the scale and that is a useful thing to keep in mind.

MORRIS: I would like to hear more about self-handicapping. Can you tell us more about some of the literature that has been built up around it in terms of how analogous it may really be to our research and whether there are variables such as proneness to self-handicapping or whether it is task dependent or varies with social context. Is there enough there so that we can begin making some predictions?

STANFORD: There is quite a literature. In some aspects of the literature there are definitely some contradictory unresolved results. I have cited some surveys of it in my paper that you can look at and there is a number of others that I did not cite. I am not up on all the literature on this. But let me say first off that a bottom-line boundary condition for the effect it seems is usually that a person should have some sense of success at something and have an opportunity to do it again, or something similar, and not have the slightest idea of how it operates. The effect has been studied in many different contexts, and one thing that is clear is that it definitely functions as an impression-management strategy. In other words, for instance, we know that people self-handicap in order to make a good impression on other people. That is one thing that we know. It has some interesting implications. It implies for instance that if they self-handicap, they are probably going to report to you ahead of time about the self-handicapping situation that they have instituted. If you do not allow them an opportunity for that, you are depriving them of the opportunity to self-handicap. That is one kind of implication. But there is also some evidence to suggest that they do it to manage their own kinds of self-esteem. All right, let me sneak in something here.

People have been asking how this applies to parapsychology. Let me tell you one angle where I think it really does apply. Some of us have encountered this in our laboratories, I think. Now parapsychologists, of course, never encourage subjects to go out and alter their state of consciousness for an experiment in ways that are illegal, illicit etc. But sometimes we have seen people who go out and do it anyway. They take some kind of drug beforehand, including one that according to legend, can in just the right amounts facilitate psi performance. But the legend also says that in the wrong amounts, just a bit too much of it deters psi performance. So that if you come into an experiment and you have soused yourself with something ahead of time, this is a super self-handicapping strategy. I really think that we parapsychologists need to get into that literature and try to tease it apart for ourselves and start considering this. I would be extremely surprised if many of the things that we have not interpreted as resistance are not attempts to protect one's own impression in the eyes of the experimenter and those around him or one's own self-esteem. If we do not recognize that in our work we are being very insensitive to our subjects socially. There is a little bit of self-flattery that comes in, in a way, that it excites us to say, "Wow, they are scared to death of the powerful effect that we have been getting." That makes us feel we are onto something really hot, so to speak. And I think that it may be a kind of a back-of-the-mind motivation that makes us accept, almost without question, that just about everything subjects do that is untoward is out of fear. I am not suggesting that fear is not a factor. I believe that it is, but we need to be careful. Now one of the most interesting questions concerns what you do about all this in your experiments. Do you allow subjects the opportunity to self-handicap or do you not? Well, it depends upon what you want to find out in you study for one thing. But also you know already from the literature the point at which it is likely to start to happen. It is as soon as the experimenter and all the information feedback the person is getting says you are a reliable performer, you have a reputation, then it starts to happen. We have some old folklore in this field related to this. When I was working at the University of Virginia Medical School I had people who would come in and start telling me about a seemingly genuine psychic experience before the experiment. I made it a rule to ask them if they would please tell me that afterwards. I am extremely interested, but we need to go into the procedure right now and please be sure to tell me afterwards. They wind up going out on a limb making a reputation to defend if they say it ahead of time. Now, I think we need some studies to look at this kind of thing in our field because we know that this interfaces with parapsychology in terms of egocentric effort. Subjects do not know how to make it happen. They do not have any insight into the process because this stuff that we are dealing with is ego alien and then you get in there and you have to make it happen. What do you do? You may try all kinds of crazy things that mess up the process. I am just trying to open up the discussion to this kind of thing. I do not have a lot of answers for you, Bob, at this stage. Maybe at another conference I will, but I am going to be digging a bit more into the literature myself. I thought it was really important to get this out since I just thought about its application to parapsychology within the last year and half. Let us see where it leads.

SCHOUTEN: I guess your paper is "must reading" for everybody who wants to go in that direction. What surprises me though is that I had expected that to measure hypnotic susceptibility experimenters would also use psychophysiological methods. I can well imagine that you know the extent to which a person is able to influence his own autonomic processes and that might be used as indicative of susceptibility. I have always been skeptical about hypnosis. What convinced me that something real was there was a few instances where people were hooked up to an EEG and I saw very dramatic effects in EEGs and other autonomic measures. They were so impressive that I thought that something must certainly be there.

STANFORD: May I ask what kind of effects? What were you looking at? What was the occasion of the effects?

SCHOUTEN: Well, those were not experiments, so it was a very personal impression. But you see the recordings following suggestions and simply with the eye, you can see that the EEG becomes enormously different. Things like that really impress me, but that is no valid evidence whatsoever. Did you look into that literature?

STANFORD: That could lead to a study. The hypnosis literature does not suggest that there is any physiological measure that per se is indicative of being in a state. The state theorists would love that to be true, and it has been looked for a lot, but it simply is not true so far as we know. What you can get on hypnosis in terms of physiology is a response to suggestions. One investigator did a study in which he gave a suggestion that was supposed to frighten highly hypnotically susceptible subjects. They got all kinds of heart reactivity, wild variability and so forth in response to suggestions that were supposed to produce physical anxiety. You can get real physiological changes in response to suggestion with this. It might be a way of seeing if it is catching on. It might catch on in the same person if he was just asked to imagine it outside of hypnosis. But these kinds of scales are not measuring state per se but how much people become involved in the suggestion. But that is obviously very important in our studies where we use suggestions to try to get parapsychological effects. I think that is a very good constructive direction we might want to take a look at.

## MORNING GENERAL DISCUSSION

## DAY TWO

CARPENTER: This is really another question for Rex. It seems to me you are moving the focus away from internal attention states to both conditions and individuals that facilitate moving away from ego behaviors to behaviors that do not feel as though they originate in one's self. And if that is an important aspect of what seems to facilitate psi, and it seems to me that it probably is, do you have an interest in the study of multiple personality disorder? People who are afflicted with that disorder are enormously vulnerable to shifting away from the ego and also sometimes enormously flexible at doing it at will. And another question is what do you think about the importance of what seems to me to be therapeutic elements in the environment of a laboratory, that put people in testing situations like this? In my own experience in the ganzfeld, for example, I experienced initially a certain amount of conflict because the instant I closed my eyes I was flooded with primary process material most of which I wouldn't tell most of the people around me most of the time. It seems to me that the therapeutic safety that we rely on in clinical settings is probably an important aspect of the more successful free-response laboratories.

STANFORD: Absolutely. I appreciate both of those questions. Let me comment on the last one first. I could not agree more. In internal state settings, be they hypnosis or ganzfeld, we are asking the subject to be passive and, by implication, dependent on the setting, the experimenter, etc. This is potentially a very conflict-ridden situation for many individuals. Let me give you an example. There was a word association experiment that was done some years ago in which people were just asked to associate to a single word. It was found that if subjects were asked to lie back, a certain class of neurotic individuals tended to give high commonality responses, go to the popular response, stay away from that stuff that might be the deep dark unconscious. Defensive behavior, in other words. When those same neurotic individuals sat up in a chair, they did not show such defensiveness. Well, we are in a very passive splayed-out situation with ganzfeld. The experimenter-subject relationship takes on the greatest importance. I suggest in my paper that one thing we do have to do is to start probing the subject afterwards using probably another person other than the person working with the subject, unless that person is extremely skilled, almost like a clinician, to be able to bring up all kinds of reactions because that may account for a lot of the variability of what goes on here. I quite agree and I hope we look at that. Regarding the multiple personality, that is one that hypnosis and internal states researchers have been getting very excited about lately. They find that that relates to some of the same childhood experiences that hypnotic susceptibility does. For instance, there are a number of studies showing hypnotic susceptibility relates to being rather severely punished as a child, including physical punishment. The same thing applies, as I am sure you know, to a lot of multiple personality cases. There are definitely commonalities. I do not know whether you want to try to get multiple personality people into our experiments because you know that might have some threats for them. But, yes, I agree. When I go into a good ganzfeld setting at PRL or somewhere like that I not only feel safe—I feel wanted. I do not feel as though I am being used. I feel that this is a wonderful participation in something and somehow or another we have got to get hold of those social aspects. I believe it is triply important in situations where you are asked to be passive and dependent like this.

HONORTON: I have a couple of comments on some things that William was talking about. Someone suggested that we really should not generalize so much from these very small effects, because they are after all very small effects. But as I recall William said that the overall effect size in his living systems work is on the order of .28. Once again I will remind you that that is a magnitude of the recent heart attack-aspirin study that was discontinued because it was absolutely conclusive and ethically inappropriate to continue a study with such a consistently strong effect. Robert Rosenthal likes to use what he calls his "binomial" effect size display to point out that in correlational studies, for example, people very frequently get in the behavioral sciences correlations of a magnitude of .35. A lot of people say, "But that only accounts for about 10% of the variance." But another way of looking at it is in a health-related study. If the treatment is effective relative to the control such that the correlation is about .35 and that is the same order of magnitude as this effect size William is talking about, that means that if you are in the control condition you might have a 10% greater chance of living than if you are in the control condition. So whether it is a large effect or not depends on who you are. So I would suggest that we start thinking about small effects in a little bit broader context rather than just in terms of the percent above some chance baseline.

The other thing I wanted to ask William is if he has done any kind of meta-analytic comparison of his living system work with the other researchers he has been involved with? Are you getting stronger effects there or are they about the same? And finally have you considered the possibility of using the living system paradigm as a way of assessing the noise reduction model? For example, you said you were planning to do some auto-regulation work. Would some of these systems be more or less amenable to psi interaction if they were trained down to a very low noise level, so that for example with GSR you had a very quiet skin state to begin with?

BRAUD: A meta-analysis of the bio-PK literature is now in progress. We have not yet been able to compare it with the other kinds of procedures. In terms of the last question it seems to be easier to activate someone than to calm him or her at a distance, so your idea of calming one down beforehand, lowering the noise and then bringing the signal out of the noise might be a good strategy.

HONORTON: It might also be a way of testing the noise reduction model by starting with a very noisy system or a very quiet one.

PALMER: I would also like to go back to William's presentation and the exchange between him and Sybo. There is one point where I have to disagree with Sybo. That is where he said it is inappropriate to speculate about the possible relevance of William's kind of research to healing. It seems to me that either directly or indirectly our basic research in parapsychology is sponsored by society at large, particularly in terms of funding. Thus, it seems to me that we have a social responsibility to inform society about the long-range implications of our research. William pointed out in his response that if you carry this thinking to its logical conclusion his research might well have relevance to healing. The one proviso, though, is that we have to be very frank about what the research findings at present allow us to conclude. We have to be very clear that we are talking about *possible* implications not *definite* implications. I think a lot of the uneasiness people feel when we speculate about possible implications is really addressed to the exaggeration of current findings. I think it is important to keep these two issues separate.

SCHOUTEN: I think there is some misunderstanding here. I thoroughly agree research in parapsychology should be more directed towards social issues. The only thing I am saying is that as far as paranormal healing is concerned, I think research should be directed at paranormal healing first and then try to find out which are the factors which contribute. To turn it around and say that a specific factor like PK might have relevance, is a little bit as if you say, "Well, you know, there are many road accidents so let us only look at mechanical failures because we know they might affect accidents" and as a consequence perhaps leave out that 80% of other causes which play a much more important role. Look at paranormal healing. If PK is found to be an important factor go ahead with it, but do not start the other way.

RAO: By paranormal healing are you implying that it is psychic healing or that it is PK healing? You are not talking about psychosomatic healing. You are also not talking about other forms of non-conventional healing. By saying paranormal healing you are talking about healing mediated by paranormal processes. No?

SCHOUTEN: No, actually.

RAO: Then use some term other than paranormal healing.

SCHOUTEN: No, because I am not the one who defines it in society. People act as healers and call themselves paranormal healers and have an effect on patients. That is the issue. Those people think they do it by paranormal means, but they can not prove it of course. And all I am saying is we should not accept that immediately as the model to start with.

RAO: I think we should be conceptually clear.

BRAUD: I am going to put this in a historical context, about why we first did what we did. We started doing these living system experiments because they seemed to satisfy three conditions all at the same time. One is that the target systems were very variable and I was in the midst of thinking about lability at the time, so it was a natural kind of system to use. Secondly, the motivation is extremely high in these experiments and anything that makes our subjects or ourselves more interested or involved in the experiments should facilitate the results. Thirdly, we saw this as a very convenient way of studying healing through the back door, as it were, by making these experiments healing analog studies. It was almost an historical accident that these three kinds of motivations or influences came together in these living system PK experiments. So I did not really set out to study healing or to study psychokinesis but this was a fortuitous convergence of a lot of factors. Maybe even in ordinary healing there is a psychic component. If you want to begin at the beginning, maybe influencing my own breathing rate or my immune system in the most ordinary way possible may have imbedded in it some psychic influences. So we do not even have to go to extraordinary healing to begin.

# PSI RESEARCH AND THE CONCEPT OF VOLITION

ROBERT L. MORRIS

## *The Concept of Volition*

Volition as a major concept is an intimate part of our daily mental life, yet it is under represented both in psychological research and in parapsychology. We can talk readily about research in sensation, perception, cognition, memory, emotion, and motor response. Within cognition, we can discuss reasoning, problem solving and decision making. We can describe circumstances that facilitate the development and refinement of motor skills, including verbal skill, and we can consider motivational and emotional factors that appear to influence the selection of patterns of motor behavior to be implemented. But there's still a set of concepts left over, generally considered under the term volition [or conation, as MacDougall (1923) and others within psychology might have it]. Volitions include our wishes, wants, hopes, desires, intentions, and so on, activities often associated with the concept of will.

Such terms may appear frequently in our ordinary language descriptions of our mental activities, but rarely in any formal description, and very rarely (MacDougall once again the most obvious exception) in formal psychological theory. We do not have an extensive vocabulary to describe such mental activities, although we can be quite poetic in our informal and artistic expression of our desires and longings. Our volitions tend not to be easily operationalized. Overt behavior can be induced by manipulating input relevant to some presumed needs of the organism, and we can infer that some sort of volitional activity went on. But the induced behavior patterns do not necessarily tell us anything very specific about the volitional activities that preceded them. They can be described indirectly through verbal report, but our language is notoriously imprecise and verbal report unsupported by converging measures of some sort is generally regarded as problematic for detailed research purposes.

Historically, the concept of volition has been linked to "will," to the problem of free will, and (once it became an issue) to the whole mind-body problem. For Aristotle, for instance, it was important for understanding ethical issues to differentiate between voluntary and involuntary actions. If various of our actions can be shown to be predetermined by various external factors rather than the product of our own independent volitions (e.g., free will) then we should not be held responsible for them.

Later on Spinoza (Durant, 1961) regarded will as an abstract term pertaining to a series of actions or volitions, a volition being an idea which has remained long enough in consciousness to pass over into action. All ideas become action unless stopped in transition by a different idea. Will in part is an impulsive force determining the duration of an idea in consciousness. Spinoza goes on to describe an entire causal hierarchy. Will can be equated with desire, desire is an appetite or instinct of which we are conscious (but we are not conscious of all instincts) and instinct is a device developed by nature to preserve us. Thus the need to survive determines our instincts, instincts produce desires, desires produce thoughts, including volitions, and there is no free will. We are conscious of our volitions and desires but ignorant of their underlying causes.

Schopenhauer (Durant, 1961), on the other hand, regarded will as a striving, persistent vital force, a spontaneous activity. For him, will was basic and dominant. Will, via continuity of purpose, gives unity to consciousness, holding together its ideas and thoughts. Body is objectified will. Will is the essence of man and, perhaps, even of the universe in general.

Even in recent years, the concept of volitions has been linked with will, the concept of free will, and the implication that with free will there is an independent mental existence capable of interacting with and influencing the physiological body. Ryle (1949), for example, in arguing against the idea of free will and separate mental events, defines volitions and intentions as mental acts. "To describe a man as intentionally pulling the trigger is to state that such a mental thrust did cause the contraction of the muscles of his finger." He then offers two objections to the ideas of volitions and intentions, arguing that we should no longer use those terms or talk of them as though they exist. His first objection is that people do not actively describe their own conduct or mental activities by referring to volitional activities. We do not talk of ourselves as having performed a set of volitional acts when we get something done. Novelists do not write eloquently of the volitions of their characters. It is hard to think of adjectives to use in describing volitions, or to regard them as trainable, as taking place at one time, but not another, and so on. A second objection is that one person can never witness the volitions of another,and can only infer them from overt actions. Even when we notice what appears to be an act of will in ourselves, it is impossible for us to demonstrate that that act was indeed responsible for the overt action with which it appears to be identified. Thus volitions appear to be supported neither by an experiential nor an empirical basis. Thus there is no reason to take them seriously, either philosophically or as part of any programmatic psychological research.

In recent years, advances in two major areas have dominated present-day thought on the nature of mental processes and our understanding of them. One is in what has loosely been termed the cognitive sciences, as exemplified by information processing models, artificial intelligence and cognitive linguistics. Volition does not figure prominently in the cognitive sciences. There are no models of volitional activity as there are models of cognitive activity. We talk of strategies for problem solving, for decision making and thus for goal accomplishing. There is thus the implicit notion of a basic wish, desire or intention of some person or persons to accomplish a goal. Occasionally someone will query whether or not a computer can be said to have goals, or even to have intentions. But it is generally agreed that computers do no more than carry out what their programs compel them to do, and there are no programs yet written (to the author's knowledge) that tell the computer to do whatever "it wants to do" at any stage in its activities. Some programs, such as chess-playing programs, may incorporate sources of noise (generally pseudo-random number generators) which are periodically accessed to provide a random selection from among a finite, prespecified range of options. Although a certain level of indeterminacy is thereby provided, there is still no sense in which the computer is "freely choosing" among its options. Cognitive linguistics as well does not deal in volition. There is an assumption that those who engage in verbal behavior intend to express themselves, or intend to communicate, but the primary concern is in the formal properties of utterances. In short, the cognitive sciences, like other branches of psychology and related disciplines, are happy to acknowledge that organisms can have needs, goals and therefore intentions. But nothing further is done about the properties or characteristics of those intentions and they themselves do not figure further in the main models of mental functioning.

A second major area of recent and continuing activity is psychobiology. Churchland's (1986) *Neurophilosophy* is a landmark attempt to synthesize recent achievements in cognitive sciences and psychobiology, applying them to basic issues of philosophy. From her standpoint, beliefs, desires, goals, hopes, thoughts, intentions, expectations, interests, aims, and so on, can be regarded as sentential attitudes. They are mental states referred to in the explanation of behavior, assumed to mediate between input and output, important in causing behavior. They are indispensable for psychology, but unfathomable by neuroscience. Some such as Stirk (see Churchland) regard categories such as belief and desire as being the product of folk psychology, having internal deficiencies such that they must be reconfigured to be of any use to a scientific psychology. At best they may emerge as reconfigured analogies.

Of the above concepts, Churchland does find the notion of intention to be productive in understanding the neurophysiology of behavior. Intention in this context is regarded as the mental activity involved in the final planning of a motor behavior pattern about to be carried out. We know that there is an area of the cerebral cortex in front of the central sulcus known as the motor cortex, such that induced electrical activity there will produce movement of particular parts of the body. There is also indication that if someone is asked to intend to move a limb but stop short of actually doing it, there is detectable change in the electrical activity of that area (Libet, 1985). We also know that the cerebellum, an extraordinarily complex part of the brain, is responsible for the coordination of sophisticated patterns of motor behavior. But how is cortical activity linked to cerebellar activity and thus to the production of behavior? One model which Churchland finds particularly interesting is the tensor network theory of Andras Pellionisz and Rodolfo Llinas (e.g., Pellionisz & Llinas, 1985). They posit that the cerebral cortex produces a complex output, an intention vector, which specifies positions in a sensorimotor coordinate system. At the cerebellum this incoming vector is transformed via a tensor into an execution vector. This new vector in turn specifies the detailed sequencing of muscle cell activity, which in turn determines the cerebellar output and message down the spinal column to produce final patterned behavior. Thus intention has usefulness in specifying an output from higher centers and to describe the brain state just prior to the initiation of movement. But what led to that intentionality vector itself is still too complex for systematic neurophysiological modeling.

In summary, the concept of volition has found little direct use in psychology, although it is agreed that some terms such as belief, desire and so on can be invoked in some general way to contribute to the explanation of behavior. Within the literature of motivation, one can talk about and catalogue various needs and the organization of behavior to accomplish them. But volitional states, volitional strategies, volitional styles and the like are not involved with any specificity. This may be in part because they have been so strongly associated with the notorious free will-determinism debate, with perhaps the assumption that to take volition seriously is to take a stand in favor of free will. It is also argued that volitional states are not readily described in our language of experience and are not directly observable. They may thus be irrelevant and unworkable as components of organized scientific psychology. But the same could be said in part of cognition, yet we find cognition a useful concept (although not without its own controversies).

For the remainder of this paper I will attempt to argue that volition can be a productive concept, both in psychology and parapsychology, that it may be a vital, but underemphasized component of parapsychological experience and research, and that parapsychology may even provide some effective tools for helping psychology (and philosophy) reinstate it as an important element in our understanding of experience and behavior.

## *Volition and Psi Research Strategies*

The concept of volition has become important for us in designing parapsychological studies for two separate sets of reasons, one methodological, the other theoretical.

From the methodological perspective, psi research tends to involve erecting circumstances in which we assume someone is motivated to interact psychically with a target of some sort. Generally subjects are aware they are participating in that we have asked them to try being psychic. Sometimes we give them considerable advice on how to try and we do things to motivate them, to give them a reason to try. However, our experimental reports rarely reflect these aspects of our studies in any detail. This makes it hard for us to assess their overall importance or the relative effectiveness of different modes of trying and also presents problems for the would-be replicator of especially successful studies. Procedurally, we need to examine the volitional component of our studies in much more detail. Like many other aspects, it has been underemphasized and underexplored.

From a more theoretical perspective, one of the more interesting and thought-provoking criticisms of parapsychology, stated simply, is "If psychic functioning is that easy to obtain and observe, why haven't researchers discovered it a long time ago?" This is most specifically a criticism of the general claim that experiments with unselected subjects asked to do ostensibly trivial tasks are actually employing a meaningful measure of some genuine human faculty. Why should we expect that someone will display detectable psychic functioning in our controlled laboratory experiments, just because we ask them to and temporarily give them a reason to want to? Don't a lot of people accept the possibility of psychic functioning and try to use it now and then? Aren't there many real-life circumstances (such as personal crises, military conflict, etc.,) when people want very strongly to influence something or to learn about it, but without success? If psi exists, we have certainly not learned as yet to use it or detect it in daily life circumstances. it clearly is not available readily to us at our beck and call. In our experiments, we erect circumstances that are trivial. They certainly are volitionally trivial. We have little reason to expect them to succeed, when all is said and done and, as we know far too well, our experiments tend to produce at best rather noisy and weak results. Should we then change some of our experimental characteristics, and, if so which ones? Volitional features stand out as a major possibility. It is an underexplored, underemphasized aspect of our work, as mentioned earlier. We obviously must do something different than merely recruit our participants from among a host of willing volunteers and request them (politely) to come visit and please be psychic between 4 and 4:30 next Tuesday, thank you very much. We thus must explore the nature of volition as it relates to parapsychology and pursue ways that we may incorporate it more fully in our efforts.

If we are to understand the role of volition in parapsychology research, we should have some general idea of what the concept covers. For present purposes, we can regard it as mental activities involved directly in the acts of willing, wishing, hoping, desiring, intending or, perhaps most broadly of all, wanting. We have many words in our vocabulary expressing variants of volitional activity. In general, they refer to a loose set of experiences involved in thought about the achievement of goals, the satisfaction of needs. It is easiest to draw the line as we approach the fine experiences of intention that immediately precede actual behavior. At the other end, the boundaries between cognitive and volitional acts become harder to specify and we may eventually decide that there are no sharp boundaries. Information processing, problem solving, decision making, all tend to be regarded as cognitive acts. Yet we also spend time, at the same time, in thoughts about our needs or goals themselves, the values we place upon them, our emotional involvement with them, what it is like before we have achieved them, what it would be like during and after achieving them, and so on. Such experiences are often intertwined with problem solving and decision making about how to accomplish the goal, if the goal is not something that is about to be immediately and/or easily achieved.

However, we also know what it is like to wish or desire or long for a goal whose achievement is problematic or uncertain at best, or impossible at worst. We can indeed have a wide range of volitional experiences yet, although we do have words to describe some of their different types, it is not easy to define them with precision. We know hoping is not exactly the same thing as intending, but we may not be able to label some of our experiences as clearly one, but not the other. When we try, we essentially find ourselves engaging in introspection and then sharing our introspections with others. A discussion of the variety of our volitional experiences could become quite lively indeed and, as noted earlier, poets and writers can express the joys and sorrows of unfulfilled longing with great eloquence. Such experiences can preoccupy us with their intensity for considerable periods of time, and there do appear to be great individual differences in "volitional style." But Ryle is right, at least in part. Our vocabulary is not especially precise, and we have no real means of measuring volitions and volitional differences directly. Introspectively we may detect them and label them in ourselves and discuss them with others. By analogy to ourselves and our general understanding of needs, goals and motivating factors, we can infer their existence indirectly in others.

We can now turn to how volitions enter into our experimental research strategies. They are potentially important in anyone for whom some component of the research is relevant to a need, want or desire. This certainly includes the researcher(s), may well include additional observers of some sort, both direct and indirect and, presuming the researchers have done their job properly, includes the designated psychics. The above are people for whom the success of the experiment is volitionally important. However, there may also be others involved in the overall experimental system, such as individuals involved in the subsystems, contributing to the determination of the subject's experience or the target characteristics, who may be unaware that they have contributed to the experimental outcome in any way. Nevertheless, if their volitions involve any contributing system components, their volitions may well be important.

## *Volition in ESP Experiments*

We assume that the subject is volitionally involved, that he or she is motivated to interact with the target. Otherwise there is no reason to expect evidence for such interaction and no point to the experiment. Generally, the experimenter assumes the subjects are volitionally involved given that they have responded to whatever recruitment process was employed. That recruitment process helps to define the subject's construance of the purpose of the experimental endeavor and the subject's own role in it. The basic goal may be to learn whether or not one has psychic skills, to prove that oneself is psychic to self and/or others, to have fun, to meet new people, to explore the mysterious, to validate (or evaluate) a metaphysical system, to show it's all nonsense, to improve one's own skills and so on.

Once the subject is recruited, the experimenter will then generally define a specific task involving linkage of some sort with a target. The task then sets a challenge for the subject, sets a goal which the subject presumably wants to achieve, a goal much more specific than the general ones listed above and not always compatible with them. Someone who merely wants to have fun or meet new people or evaluate a skill or metaphysical system may not necessarily care whether or not he or she is successful at the specific assigned task. Those with the latter two goals, of objective evaluation, may want very much to get an accurate, meaningful result, whatever it may be.

The experimental environment may have a variety of effects upon the motivation of the subject and the resultant volitional activities. The physical and social environment may be supportive or unsupportive, impressive or mundane, competent and inspiring or sloppy and dull and so on. The specific exchanges between researchers and subject may lead them to be seen as compatible or incompatible in goals and likely volitional activities. The specific instructions may define a precise, well understood, sensible, interesting task, completely compatible with the subject's volitions. Or the reverse may be true, or the subject may wonder if deception is involved. In some cases, there may be specific instructions with regard to volitional activities, including differing volitional strategies.

For ESP experiments, the receiver's task may be framed in a variety of ways, too many to list here in detail. In restricted choice studies, the task may be straightforward, to guess correctly among known alternatives. This may initiate a set of volitional activities, peaking at the moment of guessing and at the moment (if any) of receiving feedback. In free response procedures, things may become more complex, depending on the evaluation procedure and what the subject is told of it. The subject's task may be to describe the remote target accurately to himself so that he will recognize it later during his own judging. Or he may be required to describe the target publicly, such as to communicate enough to an unknown later judge to allow that judge to make a correct decision. Whoever serves as judge may have the task of selecting the picture which most closely responds to the subject's imagery, regardless of whether it's correct, or may instead be given the task of locating the correct picture on whatever basis, including psi, and regardless of level of correspondence. That means there's more room for more than one psychic/subject in the design, more than one psi task, more than one set of volitional factors. The original percipient serving as his or her own judge may have three peaks of volitional activity, during the period of responsible impressions, during the moment of initial exposure to the target set (a time of partial feedback) and during the time of final feedback. Even a remote judge will have volitional activity at the time of first experiencing impressions and targets, wanting to see some close correspondences which are potential true hits, as well as during the time of actual feedback. For somatic psi measures, the receiver may be aware of the task and thus be volitionally active during the time of being monitored, "wanting" to feel the sort of surge of somatic activity that might represent a true psychic influence. If feedback is given, there will be another peak of volitional activity at that time. In some somatic psi designs, as well as in others, the receiver may not be actively aware of being in a psi study or of having a task related to a target of some sort. In that case the majority of the receiver's volitions will be determined largely by whatever cover story or general justification has been offered by the researchers to account for the physiological monitoring. Such studies when successful raise the question of whether conscious volitional activity is necessary for psi, and whether in some real sense volitions need not be fully conscious. If I am in some way monitoring the environment, do I receive information that a loved one has just been stressed, which information is processed below my awareness, but still assisted by volitional activity, e.g., wanting safety for loved ones and wanting to know if they are unsafe? The latter might involve the notion of basic wants or needs functioning below our level of awareness, yet constantly there in the background. This is one of the more ambiguous areas in the definition of what does and does not constitute a volitional act.

Receivers may be encouraged to take a relatively relaxed, passive role, letting impressions come as they may, or might be encouraged to adopt a more active role, trying or striving to generate accurate impressions. Sometimes specific mental strategies may be encouraged, such as taking a guided imagery trip to the site of the target. Agents or senders as well may be encouraged to adopt a passive or active volitional strategy, either relaxing and experiencing the target to be sent naturally, or putting more effort into trying to send impressions, perhaps also taking a guided imagery trip to the receiver and delivering the message mentally "in person." These differences may be presented in instructions to subjects, or may emerge from the subject's own selection of strategy to accomplish the task. In each case, the subject must make a response, and the instructions may encourage the receiver to gain impressions and process them before making any response or, alternatively, may encourage an immediate, automatic response, say or do the first thing that comes to mind, without thinking about it. In such diversity of instructions regarding the task, certainly subjects are encouraged, in varying degrees, to want success. But additionally, to what extent are the very mental activities they are encouraged to engage in, in attempting to generate impressions, etc., not also at least in part acts of volition themselves? This is especially interesting with regard to the sender, where mental activities in some of the more active modes become very much like the mental activities of a PK subject "willing" a target to conform to his or her own intentions. We will pursue this again below.

The main point is that, for ESP studies, volitional activities are shaped in part by the subject's own interests and expectations, the means of recruitment, the experimental environment, the interaction with the researchers and the specific task-related instructions. They are often addressed only indirectly, in ways that can be subtle and not necessarily likely to be made clear in any kind of experimental writeup. They are probably generally not especially intense, and are certainly not measured in any systematic way.

### *Volition in PK Experiments*

Most of the considerations raised above generalize to PK studies. The task for the subject, however, is generally set as a volitional one We ask our subjects to exert influence, but without behavior of any sort. Rather, we ask them just to use mental influence—we speak of willing, wishing, intending the dice or RNG to behave in accordance with task instructions. Subjects are in essence asked to engage in volitional activity in and of itself, with no final intentionality vector sending a complex message to the cerebellum. We are asking them to send the analog of such a vector directly to the physical target system itself, complete with task-relevant patterned instructions on how the system is to behave. Sometimes our task instructions can be quite specific along these lines. They may even include advice on how to go about the wishing or willing (e.g., Debes & Morris, 1982; Morris, Nanko, & Phillips, 1982), in terms of imagery strategies, high and low levels of striving, and so on. Jahn and Dunne (1987) give the fullest description of the diversity of volitional strategies that their operators have employed, in the course of intending (their preferred term) during PK tasks. They include: preliminary meditation exercises; various visualization techniques, incorporating the target system in the imagery; transpersonal identification with the target; competitive strategies, such as seeking to outperform others, beat one's own record, defeat "the laws of chance" and so on; involvement volitionally with the different forms of available feedback; and attribution of anthropomorphic characteristics to the target, such that the target can then be exhorted, coaxed, threatened, or pleaded with. They note that such strategies appear to be idiosyncratic and not always used consistently. The only overall theme they note that seems informally correlated with success is the expressed feeling of attaining "'resonance" with the target, immersed in it, "going with its flow."

On the other hand, subjects once again may be unaware that they are in a psi study. As Stanford (e.g., 1977) describes it, there may be a need-relevant labile event hidden in the environment, serving as a target system in the mind of the researcher. There is an implicit task, in that the subject is presumed to be motivated to influence the target system to match his or her needs. Once again, as for ESP, we confront the question of whether or not volitions need to be conscious. If success occurs, then either volitions do not need to be conscious, or else volitions are conscious and are not necessary for PK activity. As Braude (1986) notes, given the diversity of volitional strategies employed, there is certainly no one-to-one correspondence between the experienced content of the volitional actions and the specifics of the task.

When subjects are aware of the target events, there may be two peaks of volitional activity, one at the apparent time of occurrence and one at the time of receiving feedback. The former may be the product of instructions as well as the subject's natural expections; the latter are generally just the product of the subject's natural response to receiving feedback, unless (a) the two events are very close in time or (b) the subject is aware of observational theory or has been asked by a researcher to focus on the final act of observation. Millar (1986) is an observational theorist who focuses especially on the psychological characteristics of the act of observation and regards them as important in the final fixing of the observed event.

Experimenter volitional activity may seem especially important within the general context of PK procedures, in that the experimenter and other observers have a very specific target system and easily defined psi task. Observational theory would posit that volitional activity during stages of the experiment providing knowledge of the target event outcome and the relevance of that outcome for the success of the experiment are relevant. There are many kinds of observation taken of events within the experimental system. Proponents of various forms of observational theory may want to take volitional activity at all or most of these observations or of only one of them into account, perhaps by suggesting that volitional activity during various stages of observations be intensified.

Proponents of the Intuitive Data Sorting model (IDS) such as May (e.g. May, Radin, Hubbard, Humphrey, & Utts, 1985) may also wish to focus on the timing of volitional activity. If realtime volitional activity is involved in psi functioning, then the IDS model would focus on volitional activity around the moment of deciding and implementing the decision determining the timing of the sampling of the task-relevant target system output. For a realtime PK model, later volitional activity, during the determination of the event itself, may be more important.

### *Some Implications for Research*

Volitional activity may or may not be relevant to psychic functioning. It seems likely that it is, at least in part, and we as researchers act as though it is. In our experimental research we erect tasks for our subjects that we presume they are motivated at some level to be involved in. We assume that, consciously or unconsciously, they want to interact with the target. But we pay little attention to the characteristics of the resultant volitional activity, especially in ESP research strategies. In our experimental descriptions, we often provide little detail about the general motivation of our subjects, the strategies by which they were recruited, the relevant components of the experimental environment, the interactions with the research team, the instructions to the subject and the volitionally salient features of the experimental system for the researchers and other involved observers. To do so with any thoroughness is at present impossible. We just do not know enough and we do not have effective ways of measuring volitional activity.

Yet volition may be an important component in our research whose neglected properties may contribute to the variance in our findings as well as their relatively low level of significance. Most of our studies give very little volitional guidance. The subject is generally expected to provide the bulk of the motivation. Most of our studies ask our participants to engage in fairly trivial tasks and at arbitrary times chosen mostly for researchers' convenience. We expect to see evidence of psi under those circumstances. Yet if it were that easy to call forth upon demand, would not we know it by now? We often achieve that level of motivation and do not find psychic events happening. Perhaps our subjects do not engage in enough volitional activity. On the other hand, not every crisis produces a crisis telepathy experience, and certainly disasters do occur which one would presume would be prevented if it were just a matter of having high volitional activity. There is even some evidence from PK research that extensive striving may produce poor results (e.g, Debes & Morris, 1982; Stanford, 1977).

There may be dimensions other than intensity that are more relevant, and need to be explored, perhaps in tandem with intensity. One could examine two extremes for instance.

1. *Low volitional tension.* Perhaps "trying" in a conventional way tends to interfere, or be a source of noise. Much of our usual volitional activity may be tied in to a biological implementational system, not at all linked to a psychic system, and simply serving to interfere with it. The notion of "passive volition" as psi conducive is consistent with this. Perhaps thus we should focus in some of our research on providing alternative volitional strategies not so directly tied in to our usual ones, and perhaps we should explore procedures more which have more covert forms of motivation in them that will be less likely to provoke effortful striving.

2. *Strong volitional states.* At the other extreme, perhaps we should explore enhanced volitional activities, in individuals trained to use them, but control them, such as those found in various mental disciplines and, increasingly, in sport psychology and high performance psychology in general. Such studies would need to incorporate tasks that were designed to be consistent with such states, compatible with the underlying range of motivations available to the subject.

In all of the above, it is necessary to focus on far more effective description of the volitional activities of our research participants. This means the taking of far more informal introspective material than we presently do and from a wider variety of participants within the experimental system. This can then give us a firmer basis for the potential development of scales that can be administered to look for correlation with success.

Another strategy, employed in some PK studies, is to assign different volitional strategies and see the effect upon results. In our two studies (Debes & Morris, 1982; and Morris, Nanko, & Phillips, 1982), for instance, we assigned subjects specific volitional strategies, in one case involving imagery content and in another level of striving, and found significant differences in PK results favoring one strategy over the other. This suggests the differences in strategy matter, that the volitional activities themselves mattered, directly or indirectly. By pursuing a systematic exploration of differences in assigned volitional characteristics, we may be able to build a gradual picture of which dimensions of volitional activity matter and which do not. Post-session interviews can be used to determine which strategies seemed most meaningful and implementable to subjects and which characteristics seemed not meaningful, not to matter. By assessing perceived success as opposed to actual success in post-session interviews or questionnaires, we may even be able to develop a dependent variable above and beyond psi success to use in assessing the salience of differentially assigned volitional characteristics. This would be a sizable task if done with thoroughness, but could as well help to restore the study of volition to the field of psychology in general.

Within this last context, it is important to note that psychology is turning increased attention to performance enhancement in sports and in business and thus is especially ripe for consideration of the role of volitional style in performance enhancement.It is occasionally said that parapsychology will advance when it starts to solve some problems for some other discipline. Performance psychology could use some help in defining and assessing characteristics of volitional activity. Perhaps this is one of our chances.

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## *DISCUSSION*

PALMER: Bob, I found your paper very similar to a paper that Marilyn Schlitz published a couple of years ago on what she referred to as ethnomethodology. What I found very encouraging about both papers is the need expressed to look at the phenomenology of various participants in our experiments. This is an extremely important point that has been very much neglected. I think it would even be appropriate to get into this in our research reports if there is enough space available.

I also have one minor criticism of both papers. I think it maybe gets in the way more than it helps to take a very complex vocabulary, particularly when it involves fuzzy concepts, and try to superimpose it on the phenomenology. In your case I found it awkward, in terms of your broader intentions, to try to distinguish the cause of the volition from things like intentions, motives, and analytical strategies. It might be more valuable to simply take the stream of consciousness at face value and see what we can learn from it, about what is going on in participants' minds, rather than try to impose this very refined structure upon it.

MORRIS: I agree with the thematic linkage with Marilyn's paper. I was trying to regard volition as the overarching or catchall term, with others being ordinary language terms which I defined by example. I think there are some meaningful differences in, for instance, the concept of intentionality as opposed to wishing and hoping, that I did not have time to get into. I think we can start to define some variables that may distinguish amongst them. Intentionality is, I think, much more linked to circumstances in which there is a fairly strong anticipation of success and of personal involvement. Wish or hope, on the other hand, can be extended to areas over which you do not have any control and, like world peace or becoming a billionaire may possibly never occur in your lifetime. However, I do agree with your notion of additionally focusing very much on the straight phenomenological experience of the individuals involved. And I think that is where people like Brenda Dunne and Bob Jahn have done a service, in terms of the descriptions of volitional strategies elicited from post session interviews. Such description after the fact can suggest different kinds of assignable volitional styles. We relied upon reading the informal folklore literature in parapsychology for some of the dimensions that we used to structure our differential instructions.

RAO: Bob, I am very glad to see the kind of emphasis that is being placed on volitional activity. This is something that interested me quite a bit. I am sure you know that I have a few papers with exactly that title "Psi and Volitional Activity." The strategy that I had attempted to employ is one of competition and cooperation where two individuals' volitions conflict with the desired outcome or they are blended together for the same outcome. I found at least a few cases where I was myself the subject extraordinarily relevant. The results have been quite strikingly good as far as I can make out from my own experience as an investigator. But the main problem, however, is always when you are working with groups of subjects, especially unselected subjects. We are not able to manipulate the desired effect as we would like to. In other words, our intentionality to make them act in a particular way does not seem always to work in spite of the fact that we have instructions to compete with each other or cooperate with each other. In practice this seems to be a function of a number of other variables including the personality, the style, the sex and so on. So there seems to be quite a number of variables which we have to keep in mind even when we have developed strategies for differential scoring.

MORRIS: I think that is a very good point. In one of our PK studies we administered post-session questionnaires to try to get at the extent to which people took us seriously. In one case we just asked them to reaffirm what strategy they had used and in another case we basically asked them a set of questions having to do with the feelings of competitiveness that they had had. And we found that our subjects who had been divided in advance according to their own questionnaire responses in the high and low competitive mode made different uses of the same instructions that we had given in a way that made sense. Everyone allowed themselves to get more relaxed, but the competitive people stayed competitive in the striving circumstance. I think it is possible for instance to try to incorporate post-session interviews with some specificity and also to get people to talk more about their experiences in such a way as to be able to explore them in more detail and perhaps differentiate between those who seemed to take the instructions strongly and those who did not. I think it may also be very important to work with performing individuals such as athletes, tying this in very much to the notion of sports psychology. Jim Perlstrom and Richard Broughton have done a very nice study along these lines, working with people who are used to the deployment of different kinds of volitional strategies. I recall an anecdote up at Syracuse University before I got there when their basketball center was brought in to interact with an RNG device. When he first came into the room with it he used his usual strategy on the basketball floor. He screamed at it, intimidated it, did everything but physically abuse it. His results were high, but after awhile started to decrease. He then shifted strategy, becoming more the finesse player and tried to sneak up on it in terms of his verbal behavior clearly playing different games with it. So I do agree with your point and I think it is a difficult task. It is analogous to the concept of cognition, to which an enormous amount of attention has been paid in the past. This may be just as rich and deep an area, but it will be extremely difficult to mine for some of the reasons you were suggesting.

STANFORD: First off I want to express my appreciation for a very interesting paper that I think can lead off into a lot of different directions. I was particularly intrigued by your suggestion of looking at how using an assigned volitional strategy might affect the individual's perception of success. Is that what you said?

MORRIS: Right, that is right, yes, exactly.

STANFORD: It might obviously be different for different individuals, but even if it is a generic effect it is really quite important that we not assume that perceptions of the success are the same with different strategies. I am probably not as much up on work on volition in psychology as you are but I know, for instance, there is evidence that the perceived values of outcomes can be affected by the mere act of making a choice. Ellen Langer at Harvard has shown that if people choose their lottery tickets they have a greater perceived value. They want to sell it for more money than if they did not get to choose it, but they received one nonetheless. But how does this change in perceptions of success come about? Through what kind of process internally is it mediated? Langer, for instance, proposes that there is a generalization of feelings of efficacy from normal situations. This is in her discussion of illusory correlation, which I am sure you are familiar with. There are other people who explain the results of the same kind of experiment as cognitive dissonance reduction, for instance. So one is going to have to get in there and look at some of those internal processes, which can produce some really meaty data.

MORRIS: It might be useful to compare parapsychology studies which provide a marvelous justification for asking people to engage in "pure intentionality" acts versus other situations in which the subjects do not think it is a parapsychology task, but they anticipate that there is some other meaningful consequence of whatever volitional activity they have been asked to do.

STANFORD: Something that you said that captured my attention was the implication a number of times during this conference that many of our tasks are just dreadfully dull for people. A subject is not motivated for the task if you have him come into the lab at 4 o'clock in the afternoon and say, "I want you to do this for me." With regard to the task per se possibly they are not that all motivated, but we also have a good bit of literature in psychology that has suggested that people must be terribly motivated and ready to exert volitional activity in that experimental situation. Martin Orne reached the conclusion that there may be nothing that you can ask subjects to do in experiments that they would not do. In some sense there is volitional activity there. He would have people sit for hours doing the most ridiculous task, then come in and tear it up in front of them and make them start over again and they would go on and on. That is volitional activity and in some sense there was motivation involved. But the question is what type of motivation, maybe motivation to impress, to please the experimenter, to advance science, maybe no real motivation for the task?

MORRIS: Yes, that is right and I think there may well be different qualitative aspects to the volitional activity of research participants as well. Oftentimes if somebody may say, "Well, OK, I am going to try." Then whatever he does, whatever he means by "trying" may be totally left up to him. He may draw some analogies with how he tries within other performance contexts. Certainly everybody who comes into a study has somehow been recruited and thus brings some level of motivation in with him. But we may need to increase the intensity of motivation. I know various of us are involved in that already, but also we may need to encourage people to explore different kinds of volitional styles. Some forms of ritual may be used simply to give some kind of structure to volitional activity and help organize it in some way independent of the actual experiential details.

MAY: As long as there is a lull, I would like to ask Rex something that occurred to me. Somewhat off the topic, but those poor subjects that got abused or at least my view of abused by what you just said, would they volunteer, hope, want or would come back a second time?

STANFORD: I would not be surprised that they would not come back a second time. What I am saying is that I think they would come back a second time.

MAY: Really?

STANFORD: Yes, in fact there has been work on the social psychology of the experiment in terms of trying to find out how people perceive the experiment as legitimate or not. Apparently people who volunteer for experiments (and that is an important desideratum) perhaps tend to think that whatever scientists do has got to be meaningful and worthwhile and so they do not mind sacrificing themselves for that sort of study.

MAY: They do not know scientists too well.

HONORTON: Obviously you must think so, too, since you repeated what they did.

STANFORD: What?

HONORTON: We have just been talking about it for the last 15 minutes. But in my recollection in Orne's studies some of the subjects would continue going. You give them a stack of papers and say cross out all the letters E and then come in, tear them up and give them another stack and some of them would keep going hour after hour after hour.

STANFORD: That's right. And turning knobs on things and then he turns them all back and they have to do the whole thing over again.

MORRIS: It might also depend on what else it is that they should have been doing otherwise.

STANFORD: That is true. If they had final exams it might have been wonderful.

# MIND AND METHOD

William G. Roll

## *Introduction*

We have missed something in our parapsychological research, something important. What we have missed is a subject matter. Other scientific disciplines, from astrophysics to quantum physics to physiology, have a subject matter, be it heavenly bodies, quantum bodies or living bodies. These objects can be described in ways that make sense and that are consistent with descriptions of other natural objects.

Each descriptive framework is reflected in a set of procedures—telescopes, cloud chambers, microscopes—that fits this framework and may help extend it. Parapsychology *uses* methods, sophisticated methods, but it does not *have* methods. And it does not have methods because it does not have a descriptive framework; it has no subject matter.

The methods that we use come from other disciplines, such as psychology, physiology, and physics. We do get results, however, at least on lucky days. All of us here would probably agree to this much. What do our results show? Our findings are determined by our methods and our methods are derived from the other sciences so what our findings show are deviations from the expected, from normal psychological, physiological, and physical effects. ESP, we agree, is not a known form of sense perception, and PK is not a known form of physical interaction with the environment. In other disciplines deviations from chance point to nature, to the normal. In parapsychology deviations from chance point away from nature, to the paranormal. We are in danger of becoming a deviant science, a science of the anomalous. Some of us are using these terms as synonymous with parapsychology and even speak about applied anomalous research instead of applied parapsychology. But the meaning of the noun science, *scientia*, knowledge, is canceled by the adjectives deviant and anomalous. So what we have is nothing, no science, and nothing to apply.

Dean Radin (1989) gave a more optimistic prognosis in his Presidential Address to the Parapsychological Association. Dean suggests that the gaps in our knowledge of psi will be filled by the sciences whose methods we are using. Psychology, biology, or physics will eventually show how target and response are connected in ESP and PK. According to this scenario parapsychologists are apprentices in these other disciplines. As we fill in the gaps in the description of psi, we will become fullfledged psychologists, biologists, or physicists and parapsychology will disappear from the scene.

I do not think parapsychology will be swallowed by any other science or combination of sciences. I also do not think that parapsychology is on its way out. But the self-image of the field is not too good these days and our corporate health could be improved.

There was an earlier, simpler time when we could say what parapsychology dealt with. It dealt with mind and *The Reach of the Mind*. To J. B. Rhine (1947) parapsychology had a distinct mission to trace mind across space and time and perhaps across death. This was his purpose, to demonstrate to the world that there is a dimension of human nature that goes beyond the limitations traditional science has imposed on our thinking. I doubt that any of us would be in this field were it not for Rhine's vision. Certainly we would not be meeting here in North Carolina. Let us join with Rhine and explore the possibility that mind is our subject matter and that a methodology exists or can be created to explore mind.

Rhine liked to re-examine his findings and theories. Let us take this attitude in tracing the features of mind. Rhine equated the physical world of everyday experience with the world of the science of physics. He saw mind in a different domain, not subject to physical limitations. But the image of the physical world has changed. Let us explore the concept of mind, body, and related terms with as few preconceptions as possible.

There are three sets of relationships I would like to discuss today. I want to explore the relation between self and other, between self and body, and between body and place. I shall suggest that psychic phenomena are expressions of a corporeal self that includes others and that exists in the world of space and time.

## *Mind, Body, Place, and Time*

*Self and Other.* "Mind is the element or complex of elements in an individual that feels, perceives, thinks, wills, and especially reasons." (Webster, 1977). Mind comes from the same root as memory and our mental faculties, the way we perceive, think and reason are also memory faculties. Mind and memory are constituents of the self. I experience myself in the present in terms of my history, of my past and I project myself into the future in terms of that same history Similarly I am perceived by others as they know my past, present, and future through their minds and memories. When my past is brought into my present, the remembered other is also brought into my present. The others are part of my mind, my self, and my mind is part of theirs.

*Self and Body.* The self is a corporeal self. I now exist in a body. When I look to my past, I trace an embodied existence and when I look to the future, I project this body ahead. If the self is many, its bodies are also many. These bodies are part of my body and my body is part of theirs.

*Body and Place.* If I review my past, present, and future, I always find my body in particular places and among particular objects. My body is grounded in place. If the bodies of others are part of my body, the places and things that surround their bodies also surround mine. My body is emplaced in many locations. This mind-body is an enduring corpus. Without endurance or persistence I would have no mind and no body. My memories of the past define my self and my mind in the present, and they project this present mind into the future. In an advanced state of Alzheimer's there is no past and no future; the mind is gone. The body, too, is a product of the past projected to the future. It is substantiality, weight, heft, comes from foods ingested in the past and metabolized in the present to carry the body forward into the future. If my embodied and emplaced self includes other emplaced body-selves, it endures in these bodies and places, it has several pasts, presents, and future.

*The Long Body.* The mind or self I am describing here is not some mysterious or paranormal entity. It is our ordinary, everyday self, our lived mind-body. This mind and body, however, is not the mind-body studied by present day psychology and physiology. The focus of these sciences is a small body abstracted from a wider field of experience. The larger lived mind-body includes other people and the places and times of these people. I suggest that this mind is the subject matter of parapsychology and that our methodology should be oriented to an exploration of its features, of its reach.

To clarify the distinction between the small body studied by present day psychology and physiology and the larger body of which it is part, I use the metaphor of the long body for the latter. This concept, which originates in the native American tradition, has been introduced to parapsychology by my colleague at West Georgia College, Christopher Aanstoos (1986). I shall now relate the three characteristics of mind outlined above to some of the findings in parapsychology. I shall then sketch some of the features of a research methodology that may aid in the investigation of mind.

## *Self and Other*

I have recently made a surprising discovery. The main features of mind outlined here can be found in the writings of Sigmund Freud. The idea that the self incorporates others is central to psychoanalysis. According to Freud the self, especially the ego and super-ego, is formed by identification with significant others, such as parents and siblings. Psychoanalysis regards the mind or self as something like a corporate or team spirit whose purpose is to insure the comfort and continuation of the self. The evidence for this view comes from clinical studies. When the mind is disturbed this has been traced to the conflicting needs and values of the internalized others that compose it.

Edward Casey (1987) notes that to psychoanalysis "mind is *ineluctably inter-subjective in origin and import*" (p. 243) because it is based on identification and because

> identification is always *with an other*, whether this other be parent, sibling, lover, friend, an ideal or even one's own mirror image. When Freud (1957, p. 77) spoke of a "new psychical action" by which every ego is formed, he meant the action of identification (mainly identification with one's parents); and the super-ego is entirely a product of identification. Even the id "inherits" identifications in the form of images and repressed memories, mixing these in with instinctual representatives. At every level, the human psyche is constituted by identifications. (p. 243–244)

Freud (1959) proposed a theory of how the notion of a separate self is formed:

> The objects presenting themselves, in so far as they are sources of pleasure, are absorbed by the ego into itself, "introjected" (according to an expression coined by Ferenczi); while, on the other hand, the ego thrusts forth upon the external world whatever within itself gives rise to pain (the mechanism of projection) . . . Thus at the very beginning, the external world, objects, and that which was hated were one and the same thing. When later on an object manifests itself as a source of pleasure, it becomes loved, but also incorporated into the ego . . . (pp. 78–79)

Norman Brown (1966) relates the processes of identification and incorporation to Freud's interest in telepathy. "Identification" is participation; self and not-self identified; an extrasensory link between self and not-self. Identification is action at a distance; or *telepathy*; the center of Freud's interest in the "occult." Freud (1964) said that "psychoanalysis by inserting the unconscious between what is physical and what was previously called 'psychical' has paved the way for the assumption of such processes as telepathy" (pp. 55).

Jan Ehrenwald (1971) explored the possibility of a telepathic relationship between the child and its parents, especially its mother. When the child is in the womb it is nurtured by the body of its mother. This continues after birth, but is now extended to the other needs that arise in the new and more complex environment. In this preverbal phase "signals are exchanged and mutual cuing occurs in a way which runs far ahead of the infant's capacity to make himself understood. At the same time the mother seems to 'understand' in a way which is difficult to account for in terms of the 'ordinary' means of communication." Telepathy, Ehrenwald suggests, would account for "the exchange of an infinite variety of primitive or protomessages between mother and child. At the same time it suggests that telepathy is in effect the embryological matrix of communication which is later destined to be superseded by speech." Telepathy ". . . follows the pattern of intrapsychic communication within one single, psychologically as yet undifferentiated personality structure."

According to this view, ESP may reflect connections within the same mind or self. ESP, if you will, is overhearing others in the mind talking when they are separated by space or time.

Psychoanalysis has shown that we can learn about the self when the self is disturbed. Like stars, atoms, and brains, the self may reveal its constituent parts and the forces that hold them together when it is disturbed.

J. Nickie Jackson (1986) has written a small volume in which she tells of a precognitive experience of her son's death and apparitional experiences following it. The psi experiences come at the end of the book. The text is mostly about her son's life, his death, and her grief. Glenn was injured in a football game and died from a staph infection two weeks later. The book begins in the hospital:

> Glenn's father, stepmother, and I were waiting anxiously in the surgeon's office for the final results of a life-saving operation, hoping and praying for a successful outcome.
>
> When the surgeon stepped quietly through the door his face reflected the dread in my heart. As he opened his mouth to speak I felt the blow of a sledgehammer strike my chest with a crushing,

> shattering blow that sent gut-splintering screams echoing around the room. I just screamed and screamed until there were no sounds left to utter. Great gulping sobs wrenched my whole body as wave after wave of anguish tore at my heart; my soul had never experienced such torment. Then shocked disbelief and numbness took over. I was like a zombie, like someone else had taken over my body and I was watching from a distance. I later learned that shock is a form of protective armor that nature seems to give us for a few days . . .
>
> After sleepless nights and long hours of waiting and watching, hoping and praying, and being afraid to hope, Glenn's bed was empty. Death came in the late afternoon of a bright October day. He was gone—this son I had held as a baby, who was so vitally alive, who had grown into a compassionate, caring young man. He was dead. My dreams for the future had been smashed. (pp. 1–2)

Glenn's death was the death of a part of his mother. A large segment of her life in the present and the future was erased and the meaning of the past diminished. A year before she said that she had woken up in terror from a dream that her son had died, but the circumstances were different from the actual event. His father told of a dream two months before that is more suggestive or precognition:

> "I dreamt that I saw Glenn lying injured on the football field," he said, "with little things like frogs pouring out of his mouth in a steady stream," and he looked up at me and said, "Don't worry, Dad, they don't hurt," I touched him and knew that he was dead. And I wept." "I awoke from the dream with a vague feeling of unease, like waiting for a phone call that never comes. Only this time the call did come—just two months later." (p. 82)

Glenn's mother, father, grandmother, and sister had apparitional experiences following the death though mostly without ESP elements, that is, without verifiable elements. A possible exception involved Glenn's nephew:

> One time when my seven-year-old grandson was visiting me, he went into Glenn's room and stayed for a long time. Feeling concerned, I went to check on him and found him sitting on the side of his bed with tears streaming down his little cheeks and clutching a picture of his uncle close to his chest. "Steven, are you all right?" I asked, and wrapped my arms around him. "Yes, Grandma Jack, I'm O. K. I just had a long talk with Uncle Glenn, and he told me all the things he used to do when he as a little boy."
>
> I was amazed at the experiences my grandson related because they

were from Glenn's childhood and had not been told to Steven by anyone else. (p. 79)

The psi and theta experiences surrounding Glenn's death show two features that parapsychologists have also noted. The experiences often concern significant others and they often relate to the death or injury of these others. (L. E. Rhine, 1957a, 1957b, 1961; Schouten, 1979, 1981; Stevenson, 1970). Schouten's studies are of special interest because they suggest that the prevalence of these features are not just the result of a tendency to report more such cases than cases involving individuals that are only remotely connected and to report incidents that are less critical to these individuals.

Clear cases of ESP may be rare in the lives of most, but if we restrict ourselves to times of death or injury they may be much more frequent. The reason, I suggest, is that crisis cases of ESP involving close family and friends thereby involve the self of the ESP percipient. A threat to the other is a threat to the self and the death of the other is a death of part of the self.

### *Self and Body*

My experience of myself involves my body. This is true not only of my present experiences; but also of my past and planned experiences. My existence is an embodied existence and it is embodied in many bodies. When people report apparent ESP experiences involving significant others they often describe bodily sensations.

A woman wrote me, "about one month before my dad died I was sitting at Denny's restaurant about twenty miles from where my dad was in the hospital (for cancer). It was around 11:00 p.m. and I felt a tremendous pain in my heart that took my breath away. I told my husband that my dad was having a heart attack—but it was too late to call. The next day I asked my mom if he was okay and she told me that he had a massive heart attack around 11:00 p.m. the night before."

Another woman told me when her son Kenny was a senior in high school, she was unable to sleep one night. "I couldn't sleep that night because my right knee was hurting so bad . . . The next day my husband called and told me that Kenny's knee was injured and he was in the hospital. Same knee, right knee. My knee never hurt before and it hasn't hurt since. I immediately knew why my knee hurt when I heard about his accident. It hurt most of all night."

A form of sympathetic pain that may be common and that may include a psi element is the "couvade syndrome," the bodily sensations some men experience during the pregnancy and labor of their partners.

In a study of this phenomenon, David Allan Rehorick (1987) describes apparent ESP sensing of his wife's condition.

> A description of swelling in her feet, experienced about one month from term, is illustrative. During a business trip, I experienced swelling in both feet, followed by stiffness in my hands and fingers. Within a week, the problem had intensified and I could not strap on sandals. I thought that my swelling was caused by hot and humid weather. I also wondered if the toxic chemicals which I had recently been using might have had some effect. When I returned home, I learned that Salley had experienced fluid build-up in her feet and ankles. I was surprised by the fact that the problem extended to her hands and fingers. (pp. 8–9)

Rehorick suggests that the reason men may fail to recognize the connections between the experiences of their wives and their own symptoms is that they may attribute their symptoms, for instance of swelling, headaches, or gastrointestinal upsets to more familiar causes such as tension (p.8) or that they may simply ignore unfamiliar localized pains (p.10). Rehorick attributes this to our tendency to turn away from our own body as a source of knowledge and perception.

### *Body and Place*

"To be embodied," Casey (1987) notes, "is *ipso facto* to assume a particular perspective and position . . . a *place* in which we are situated" (p. 182). A place is a container of objects and is identified in terms of these objects; to have a body is to be emplaced among objects; to have a body is to be emplaced among objects. If my body incorporates the bodies of those to who I am close, it is also emplaced among their objects. Norman Brown (1966) regards the unconscious as a container of objects to which the person is attached (object-cathexes). This attachment between person and object, whether another person or an inanimate thing provides a psychic link "the original telepathy" between them.

> The hidden psychic reality contained in the unconscious does not consist of fantasies, but of action at a distance, psychic streams, projects, in a direction: germs of *movement*; seeds of living thought. These seeds are Freud's "unconscious ideas," which are concrete ideas; that is to say ideas of things, and not simply of the words, or images inside the mind corresponding to the things outside. Concrete ideas are cathexes of things: "The Unconscious contains the thing-cathexes

of objects, the first and true object-cathexes"; the original telepathy. (p. 134)

How exactly does this relate to ESP? Casey (1987) states, "As embodied existence *takes place in place* . . . so our memory of what we experience in place is likewise place-specific" (p. 182). This leads to the observation that "*place is selective for memories*: that is to say a certain place will invite certain memories while discouraging others" (p. 182). The same situation holds for objects and memory.

Aanstoos (1986) suggests that our capacity for attunement with another person is also lived between our body and things. This attunement with things is reflected in the ESP practice of "psychometry" where the person connects with others through contact with objects they have touched.

This concept links "psyche," that is, the mental aspect of an object with "meter," or measurement, its physical aspect. William James (1909) proposed that memories may exist not only in association with human brains, but with other physical objects as well. According to James' theory a person's memories may persist after death in the objects with which the person was connected when living. An ESP subject may then obtain information about the person by contact with his or her object.

A number of rather impressive psychometry studies have been conducted over the years and the possibility of psychometric linkage has also come up in more conventional ESP tests (Roll, 1966, 1967, 1986; Pratt & Roll, 1968). In free response psychometry studies there may be a tendency to respond to traumatic events, such as accidents or sudden death, and also to frequent or recent events, tendencies we also find in other forms of ESP as well as in familiar types of memory.

### *Implications for Methodology*

ESP may be a form of memory in two respects. Firstly, ESP may be a form of remembering insofar as a person's ESP response may be composed of his or her memory images. Secondly, the act of connecting with a distant person or situation, may be an act of reconnecting with, of remembering a part of our long body. This suggests two types of research procedures: one involves an exploration of memory schemata to facilitate the emergence of ESP; the other involves procedures to facilitate connecting with or re-membering the long body.

*Exploring ESP Memories.* The common insensitivity to ESP, what we might call long body amnesia, seems to involve a radical form of forgetting. It is so radical that many people are unaware of their psi connections while others insist that ESP does not and cannot exist, that the human mind does not possess the cognitive schemata entailed by the postulate of ESP, in other words that it is impossible to bridge the spatial and temporal distances described in purported instances of ESP.

There is a form of forgetting that is familiar to most of us and that is so radical that it is all but impossible to penetrate; this is childhood amnesia. Most people are unable to recall events from the first years of their lives. Freud (1959) proposed that childhood amnesia is due to the repression of infantile sexuality brought about by feelings of shame instilled in the child as it learns the moral codes of adult society. Ernest G. Schachtel (1982) offers a hypothesis for childhood amnesia: "The categories (or schemata) of adult memory are not suitable receptacles for early childhood experiences and therefore not fit to preserve these experiences and enable their recall. The functional capacity of the conscious adult memory is usually limited to those types of experience which the adult consciously makes and is capable of making" (p. 192). Schachtel supposes that it is because adult memory schemata have no place for the child's wholehearted search for and indulgence in pleasure (p. 200) that childhood experiences cannot be reached by adult memory.

The child's world of sexual gratification and intimacy which, according to Freud and Schachtel, is closed territory to the adult, may be unreachable for other reasons. On the cognitive level this world shows distinct cognitive differences from the world of the adult (Anderson, 1985; Neisser, 1967; Piaget, 1952a, 1952b; Piaget & Inhelder, 1971). The unitary nature of the child's world may be especially inaccessible to the concepts of language and therefore to the world of the older child and the adult. Louisa Rhine (1961) mentions how she and J. B. Rhine

> . . . at first brushed off as just coincidence the fact that one of our daughters, then about three years old, seemed repeatedly to voice my own unspoken thought . . . eventually we did take notice and I began to keep a diary, recording the incidents. In time, as the entries accumulated, it was possible to notice some recurring characteristics in these little episodes. The first . . . was the ease and effortlessness of the apparent transfer. One typical example occurred when the child was playing contentedly on the floor after breakfast and I was starting to clear the table. One piece of buttered toast remained and I was tempted. Then I thought, "No, I'm gaining. I must not eat it."

> Just then the little voice, in true unflattering child-fashion, piped up: "Mama, you're fatter now than you've ever been, aren't you?"
>
> And then, back to her own pursuits again—no follow-up, just as there had been no introduction to her thought. For several reasons I was "really stirred," but she was entirely oblivious. The remark was evidently based on an impression received so easily and naturally that she was entirely unaware of its extraneous source, and also of the fact that it had no rational introduction or relation to anything that went before or after.
>
> The episode also illustrates another characteristic of telepathy often noticeable in the experiences of both adults and children. It is the obliqueness of the remarks. For in them the other person's idea seems to have been reconstructed and adapted to the child's viewpoint, instead of being repeated exactly. (pp. 137–138)

Rhine (1961) finds that such experiences may first be noticed when the child is very young, become frequent about the ages of three to four, then decrease and cease entirely when the child enters school. Experimental studies of ESP in this age group have also been relatively successful (e.g., Drucker, Drewes, & Rubin, 1977). The age when telepathy interactions seem most frequent thus correspond to Piaget's (1952a, 1952b) preoperational stage. This period, which is characterized by the child's tendency to deal with things as they appear concretely in the perceptual field, extends from two to seven, when the child has learned many of the rules of the adult world.

If adult cognitive schemata and abilities are inversely related to ESP, we might expect older children who are mentally retarded to show more evidence of ESP than children of the same age groups who have developed normally. Eloise Shields (1976), who worked as a psychologist in a school for severely retarded children and also in a school for unimpaired children, received many more reports of spontaneous cases of ESP and precognition from the staff of the former school than from the staff of the latter. In a series of tests with 75 children, ages 7 to 21, who had been diagnosed as having Down's syndrome or other forms of impairment and with mean IQs of 40–46, she obtained highly significant scores under telepathy conditions and suggestive results in clairvoyance tests. The experimental conditions in the telepathy trials did not conclusively rule out sensory cues from the speech therapist who acted as telepathic agent, but Shields is of the opinion that they did not contribute all the results of this series. Shields notes that these children, "are often primitive in personality and actions, and extremely dependent upon adults for their very survival . . . The superiority of the telepathy scores also reflects the high degree of rapport between the children and the speech therapist from whom the children may 'borrow' knowledge symbiotically in lieu of having knowledge themselves."

The unitary world of the child may be beyond adult thinking and language, or at least Western thinking, because language imposes sharp delineations into self and other, and sharp temporal and spatial categories. As the child learns to speak, it learns to experience the world in terms of these categories and to ignore events, feelings, and impressions that are not encompassed by them. At the same time there would be no means whereby the older child and the adult could consciously recapture and recall its earlier experiences.

There are exceptions to childhood amnesia. One such exception may be what Sheryl Wilson and Theodore Barber (1983) refer to as the "fantasy prone personality." These persons, who may constitute four percent of the population, show characteristics that are relevant to our discussion. They report an unusual capacity to recall early childhood memories, they are highly imaginative and spend a significant part of their days day-dreaming and fantasizing, and they believe themselves to be psychic and to have frequent ESP type experiences. A study of the childhood of some fantasy prone individuals suggests that they had a stressful childhood and that their world of imagination served as an escape (Roll, 1982).

The purported recall of childhood memories and of psychic experiences are rarely verified and it needs to be determined to which extent the memory and ESP claims are genuine. With respect to the latter it is perhaps suggestive that some psi sensitives ("mediums") whose ESP capacities have been empirically demonstrated (Roll, 1982) are probably fantasy prone. Perhaps the fantasy prone individuals avoided childhood amnesia because they retained their psychic schemata into adulthood, thus also retaining a mnemonic continuation with their earlier experiences.

The effort to retain or restore the world of psychic connections may entail allowing more rein to imagination. It may be the ESP subject's capacity to imagine that makes possible the arrangement of available memory images so that they match the distant situation. The play of imagination in ESP, however, creates a problem because of the difficulty of distinguishing imagination evoked by ESP from imagination evoked by other, more personal factors. The successful psi sensitive may be the rare individual who has an active imagination, who is able to experience the connections between things that usually seem distant, and who knows enough about the workings of his or her mind to identify the sources of its images.

*Exploring long body connections*: Performance in laboratory memory tests is usually poorer than in natural situations (Istomina, 1982; Keenan, MacWhinney, & Mayhew, 1982). Similarly results in laboratory tests for ESP rarely reach the quality of reported cases of ESP in real life situations.

Experimental methodology in parapsychology has largely been based on the dualistic image of a mind that can be detached from its body. In a test of ESP or PK the subject's body is placed in a room in a laboratory, while the subject's mind is instructed to go to the target wherever it might be in space or time. This mind, supposedly unbounded by physical limitations and governed only by its free will, would then be expected to go where directed. When results did not follow expectations, we speculated that the impediments were mental; the subject did not have sufficient belief, confidence, or motivation. And indeed some studies showed that these conditions do affect results. Nevertheless when all known psychological factors were taken into account, we have still been unable to replicate results at will or even to give a convincing account for our failure to do so.

If parapsychology deals with the long body, then that body, not the small body, should be the focus for research methodology. Conventional test procedures may impede rather than support the participation of the long body by the very parameters of the experimental design. The test of ten has little personal meaning to the subject; it typically takes place in an alien environment where the subject is surrounded by strangers with whom he or she shares no history. From the long body point of view, the body we attempt to engage in an ESP or PK test is often an amputated body.

If psi phenomena reflect relationships within a system that in part can be described in terms of the subject's personal history then that history must be engaged if consistent or meaningful results are to be expected. The researcher needs to determine the meaning of psi in the life of the subject and the extent to which the test responds to that lived meaning. If the subject comes to the test hoping to understand and perhaps develop his or her psi relationships, the researcher needs to address these aims.

Then, if results are obtained during initial testing it needs to be determined what effect these results have on the subject. Opening oneself to ESP impressions in relation to people with whom there can be no other intimacy may engender anxiety and may lead to a closing or distortion of the psychic connection. ESP entails a disclosure or sharing of one's personal history, of who one is and what one hopes to be. It is an act of intimacy that we cannot expect to be lightly given or accepted.

The subject in a psi test is not only the person who walks into the laboratory to be tested. The experimenter effect, where the subject's performance is affected by the experimenters, including assistants who may not even interact physically with the subject, suggest that experimenters and subjects have become members of the same long body. The meaning of the test to the experimenter then also becomes a focus for attention and gives rise to the same questions asked of the subjects.

According to the long body model, this also includes the physical setting of the test. By connecting with the laboratory and equipment the subject connects with the past and present users of the laboratory, an interaction which may affect the results of the test. The long body model implies a laboratory psi effect no less than it implies the experimenter effect and it implies a methodology that responds to the possibility that the psychometric aspects of the laboratory may affect results.

A psi study is not necessarily replicated by reproducing the manifest test conditions. The meaning that the earlier study had to the participants must be recaptured if similar results are to be expected in the later study. Like other living organisms the long body is a changing body and it may be necessary to change the test conditions to retain their meaning.

These conditions can fairly easily be met and have actually been met in several parapsychological studies. In the PK work of Kenneth Batcheldor (1966, 1983) a group met repeatedly to produce movements of objects and other large-scale PK effects. Batcheldor reported PK occurrences of several types under what appeared to be good conditions or observation. Brookes-Smith (1973) and Brookes-Smith and D. W. Hunt (1970), obtained similar effects under improved conditions of observation. Batcheldor's approach also bore fruit in the study by A. R. G. Owen (Owen & Sparrow, 1976) where an attempt was made to evoke "Philip," a ghost whose identity and history the group members themselves had invented.

In all studies a lighthearted state of arousal was maintained in the group coupled with a focused intensity aimed at PK effects. For the Philip group, there was a sense of "complete rapport" which ". . . was more than just a 'good friend' feeling; the group members have come to regard themselves as a family, and they behave together very like a closely knit family" (p. 77).

In an ESP group experiment conducted by Doris Koop (1986) and me, ESP exchanges within the group seemed to emerge following procedures that stimulated group cohesiveness and dissociated states. This special rapport also resulted, in apparent instances of ESP between them.

You may object that in experimental studies of ESP there is only a casual relationship between subject and experimenters. I suggest that this may be one reason why results are rarely sustained. Secondly, the purpose of the test rarely has the significance of the events to which people respond in natural situations. An ESP test should take account of the meaning the results may have for the subject. If ESP entails an opening or sharing of the self, the experimenter must be able to fill the role of the significant other. In this context it would be interesting to explore the relationship between Rhine and Pratt and their star subjects in the early studies at Duke. The psychoanalytic concept of transference might be relevant here. Nowadays we know that the experimenters are also subjects. But we are not, I feel, paying enough attention to the meaning the results may have for the researchers.

Parapsychology is still struggling with the issue of objectivity. If the researcher has a strong, personal investment in his or her studies, we tend to suspect them. On the other hand, if the researcher is remote and "objective," the subject may feel like an object that is cut off and used for the purpose of the test. Psi research calls for a combination of personal engagement and impersonal appraisal.

The experimental parapsychologist is usually skilled in what, with Bohm, we might call the explicate conduct and assessment of research. We are less knowledgeable about the implicate dimensions of this work. It is not only the results of a test that may—or may not contain evidence of psi. Psi, that is, the psyche, the minds of subjects and experimenters may be enfolded in all aspects of the test situation, including its material aspects and may contribute or detract from the results.

## *Conclusion*

Parapsychology, I suggest, has a distinct subject matter and its subject matter, as Rhine saw, is the mind. This mind reaches into and incorporates the other and it reaches into the material world and also enfolds that world. This image leads to empirical hypotheses and to the means to test them.

Parapsychology questions the distinctions of the world into separate realms of mind and matter, self and other, here and there, now and then. These divisions have had debilitating effects on human life. By extracting mind and meaning from the material world, matter is seen as a lifeless substance. The corporeal world has been endowed with the meaning of death—entropy with the exclusion of negentropy—and a death dealing technology has been created to hasten the destruction. The process is supported by the image of others as aliens, as threats to the self to be overcome rather than as parts to be integrated.

If we question these distinctions we also question the notion of an objective reality and of the scientist as an investigator whose explorations can be separated from his or her intentionality. The findings of parapsychology suggest that science is a dialectic that may change the world at the same time as it explores it. This, too, is said by other disciplines. Parapsychology goes further, however, because of its subject matter. If mind extends into the other and is enfolded in matter, then a clear distinction can no longer be made between private intentions and public events. The way I am, even by myself, the way I think and feel, might then have consequences for others. Also my unconscious feelings and problems might affect others in the direct and immediate way of psi, perhaps especially my unconscious mind might have such effects.

This scenario is not easy to contemplate. Perhaps this is why parapsychology meets such vehement opposition and perhaps this is why we ourselves have found it so hard to come to grips with our subject matter.

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## *DISCUSSION*

MORRIS: I found your whole paper really very interesting. I think you have gone considerably beyond what you presented at the PA conference in terms of deriving some of the implications of, for instance, the long body notion. I do have a minor disagreement with regard to the notion of anomaly. I think that a science, especially as defined within the terms of knowledge, quite frequently finds itself exploring anomaly. Anomaly represents circumstance where knowledge really is weak and needs to be expanded most. I think often science proceeds as it defines a problem and a problem can be defined in terms of a set of thematically related anomalies. When Rhine was dealing with the notion of mind he seemed to be studying it in terms of what appear to be anomalous interactions between self and environment. He evolved a set of methodologies to study them, but these seemed to deal just with certain characteristics of the notion of mind. The original societies for psychical research I think tended more to come at it from the idea of mind and wanting to explore evidence for its range of properties. Those societies have been at it for quite some time with certainly no more progress than formal structured parapsychology. I am wondering what you would see as the most important lines of departure coming from your concept of the long body with regards most especially to the traditions that have been used within the societies for psychical research, where they really were taking on the notion of mind very directly.

ROLL: First of all, the departure comes in seeing body and mind as indistinguishable or, if you will, as descriptions of different aspects of the same thing. So when you have mind, you have body. Second, a body is always emplaced so that where you have a body you have a place. Third, the self is a group or corporation in a very literal way composed of others. The psychic connection then is the self. There is no need for any further links, the missing link is the self. The self is the other, the self is corporeal, it is a group of bodies and these bodies are emplaced in space and time.

MORRIS: And what does that lead you to do that is different?

ROLL: It leads to a series of hypotheses focusing on this notion. It leads to a unified description of our phenomena including so called haunting cases, psychometry cases, and reincarnation cases, which are place-oriented as far as I can make out. And with that comes the hypothesis that if psi is going to work, if ESP is going to work, the experimenter has to be connected with the self of the person. There has to be the same kind of relationship with that individual as there is between individuals the person is close to. So it leads to a number of experimental designs several of which have actually been carried out by various parapsychologists. Jim Carpenter's group experiment reflects this type of thinking. The Batcheldor studies and the Brooke-Smith and Hunt studies in Great Britain, the Philip studies in Canada reflect this kind of procedure. So do some experiments that I did with Doris Koop involving group studies of ESP. So there are two ways you can go; you can go out in the field and you can study psi there and what you see is the long body, as far as I can make out, or you can create the long body in the laboratory.

BRAUD: I like your concept of the long body very much, as you probably know. It occurred to me that you might make use of this concept in dealing with the issue that Richard Broughton addressed: introducing need into experiments. The kinds of needs that Richard addressed were all needs in the service of individual survival. Now if what you say is correct, and I think it is, we also have other needs. Needs involving membership in the long body or connectedness with other people, connectedness with all of nature. I think we might use our psi experiments as opportunities for remembering and we could actually bring in some symbols or some additional tools to remind our participants of our membership in the long body. That very participation in the experiment would serve to fulfill that need, to add another element of motivation to the study. Chuck has done something along these lines by using superimposed images of the agent and percipient in his ganzfeld experiments. With some ingenuity we might be able to think of more similar ones.

ROLL: Bob has some speculations about meaning and psi, Debbie Weiner's Presidential Address to the PA was focused on this same realm and Richard's paper was very much also to the point. Everything that I know in the field with some confidence, such as the work on dissociation, the ganzfeld work and Rex's work all fit into the view of the larger connection that I call the long body and that I think is no more than the emplaced mind-body.

RAO: Bill, I would like to compliment you on your being so forthright and speaking about mind without any inhibitions. I have been tempted several times to do just the same, but then when I looked into the implications I began to lose courage. Precisely for the reason you gave in support of the use of the concept of mind I am afraid we may be left with no subject matter if we talk in terms of mind as the subject matter of parapsychology. It seems to me that mind is more an explanatory concept to account for certain phenomena with which we are dealing and it can not by and of itself be a subject matter. So I would like to make a distinction between parapsychology which proceeds without any theoretical presupposition about the ultimacy of the phenomenon we are dealing with and philosophical psychology that is concerned with what lies behind the phenomena themselves. While I agree with rational discussion of the meaning and the place of psi phenomena in the order of things, I believe we should not prejudge the phenomena.

ROLL: I understand your point of view. It is really very much the point of view of the field until now. At the same time I feel that, for myself, I need to say what psi is and it has to do with the self. Now I equate the self, the mind and the psyche. To me these are the same.

RAO: How do you define mind?

ROLL: The mind is the meaning of things to us, our intentionality, consciousness, the intentionality of a human being, the memory of a human being, the consciousness of a human being. So this I see as mind. The word self or psyche as far as I am concerned will do equally well. But then when I look at the self I always see it as corporeal, as embodied. So I see mind and body as the same, but as descriptive of different aspects of that one thing. When I try to convey the significance and the importance of this work I have to do it in terms of something that resonates with my mind and with the minds of those I am speaking to. We can now say something about this parapsychological subject matter. If we are going to say it all in one phrase it is the mind or the mind-body or the self. If we are going to describe it in detail then we describe it in terms of the findings of parapsychology.

STANFORD: Even if we may not like the way that Bill Roll has phrases for the specific concept of the long body, I hope we will not neglect the kinds of things that his construct is pointing toward and is trying to cover and help us to understand. It gives a broader view of the meaning to the individual of our psi experiments and I think that is really important. There are a number of observations that parapsychologists of the experimental and of the non-experimental kinds have made that are interesting anecdotal observations that seem to fit in with the kind of concerns that you have. William Braud and I were discussing at lunch one such observation which we have both made in our laboratories and which I imagine others have as well. It is rather a striking experience that quite often a subject comes into our laboratory for a ganzfeld session, let us say, and among the predominant imagery that he brings up during the session is a particular theme that the immediately previous subject has discussed. Now it is difficult to evaluate this scientifically and know what it means, but it certainly has a suggestive value to us who have seen it again and again. In like vein, psychoanalytically oriented parapsychologists have pointed out that, in the course of therapy when there seems to be some lack of communication between the client and the therapist, it is at that point that the client or the patient seems to intrude into their beingness, if you will, in the psi way by starting to dream about salient things that are happening in their lives. I think this is very closely related to what you are talking about. It is a shared extension of being which we invite people to participate in psi experiments. I am not wild about the concept of long body because it links it onto something material, but I look at it in a more existential kind of framework. But I think we are really talking about the same kind of phenomena. I do not know quite how to come to grips with them, but they seem to be potent factors and to represent needs in people who we deal with which we ought to acknowledge in some way.

# ARE WE MAKING PROGRESS?

SYBO A. SCHOUTEN

In research fields which are in an early phase of development, like parapsychology, the phenomena under study are mainly of a spontaneous character. Spontaneous phenomena occur in daily life, are not under control of the investigator and are observable without the use of special equipment. More advanced fields, like physics or biology, deal with properties of the phenomena on a more fundamental level. These properties are usually only observable in the laboratory under specific conditions created by the investigator.

Thus the research methodology changes with the development of a field. Research methods are always based on knowledge, or assumed knowledge about the phenomena to which they are applied. Research starts with systematical observation, which is based on knowledge of how to distinguish the phenomenon under study from other phenomena. Research then develops into experimentation. In experimentation research methods can be considered instruments which enable measurement of properties of the phenomenon. To be able to develop instruments for measuring requires considerable knowledge about elementary aspects of the phenomenon. Therefore in advanced fields the research methods reflect the progress in theoretical insight and the resulting technological achievements. In the beginning of the development of a field, however, such knowledge is limited. As a consequence the research methods can then only be based on assumptions made about the nature of the phenomenon. Such assumptions are strongly influenced by the outwardly observable characteristics of the phenomenon and by social factors. The development of research methodology in parapsychology illustrates this process.

A re-assessment of the research methodology in parapsychology, the topic of this Conference, involves basically the question whether the methodology applied leads to progress in the field. In view of the above two aspects of progress in methodology can be discerned. One concerns progress in the sense that the research methodology becomes better adapted to the knowledge or assumptions we have about the phenomena we study, in our case paranormal phenomena. Associated with this is that the research methodology applied becomes more and more specific for the field. This aspect will be discussed in the first part of this paper. The second aspect concerns the results of the methodology, the question whether the methodology is successful and leads to progress in knowledge. This question is clearly too extensive to be exhaustively discussed in this paper. However, some important aspects of it will be considered in the second section, especially those concerning the problem of what criteria could be applied to estimate degree of progress in a research area.

## *I. The Development of the Research Methodology in Parapsychology and Its Suitability for the Study of Paranormal Phenomena*

*The First Period: Observation and Description.* In the history of the development of research methodology in parapsychology roughly three periods can be distinguished. The first period starts in the last century with the beginning of scientific research in this field and lasts till the thirties of this century. In this period research is concentrated on the study of gifted subjects, persons who claim to be able to produce at will mental or physical psi phenomena. The methodology applied was mainly that of observation and description.

The aim of these studies was first to establish the genuineness of the phenomena produced, that is, to establish that the phenomena were not brought about by applying known perceptual or motoric abilities. Therefore much energy was devoted to ensure proper controls, which often lead to rather artificial test conditions in which the subject had to demonstrate his or her ability. Already from this period it becomes clear how strongly the methodology applied is influenced by the concepts the investigators have about the phenomena. Because paranormal phenomena were often *a priori* assumed to be of a non-materialistic, mental nature, the studies carried out were mainly directed at proving the truth of this assumption rather than being aimed at obtaining knowledge about the phenomena.

Only a few studies were carried out in which the effect of variables on the alleged psi abilities were systematically studied. An example is the Heymans, Brugmans, and Weinberg (1921) investigation, but it is noteworthy that even that study was carried out with one gifted subject (Schouten & Kelly, 1978). It might have contributed strongly to the impressive success of the study that the investigators put so much trouble in designing an experiment which created a test situation optimally adapted to the subject.

As a result of the research of the first period a consensus was growing among parapsychologists about some general conclusions. A negative one: that so many mediums, especially the ones who produced putative physical phenomena, turned out in the end to be frauds that it hardly seemed worthwhile to further invest time and money in them. On the other hand it became quite clear that paranormal abilities were most likely a human capacity and that the spiritistic hypothesis was not needed at all to explain the phenomena observed. It also became clear that the descriptive method mainly applied so far did not contribute much anymore to an increase in knowledge. Most seriously, this method could not produce the kind of evidence many parapsychologists looked for to convince the scientific community that psi phenomena exist.

What probably also contributed to the growing feeling that change was needed was an increasing discrepancy between psychical research and experimental psychology. The two fields started in the last century on a more or less equal footing. Both as regards the way of thinking and theorizing, compare for instance Freud and Myers, and in the research methodology applied, mainly observation and description, there was not much difference. But for various reasons the two fields grew apart. Psychical research developed slowly, partly for lack of resources and because of other social factors, but perhaps also because it remained somewhat fixated on the aim to prove the existence of psi. At the same time psychology developed rapidly into an experimental field with new approaches and research techniques.

*The Second Period: The Forced-Choice Technique.* The second period is characterized by the large scale application of the forced-choice research methodology. It starts with Rhine's introduction of the card guessing paradigm and its associated methods of statistical evaluation. In itself this procedure was not new. Probably the first card guessing experiment had been already carried out in 1894 by Richet (Richet, 1921). In a first experiment different subjects guessed a total of 433 times playing cards hidden in opaque envelopes, but without success. A second experiment applying the same procedure carried out with a gifted subject, a lady Richet had known a long time, had better results. In 14 days the subject made about 5 guesses a day and obtained 12 hits in 68 trials, a clearly significant result. However, the same experiment repeated with the same subject one year later consisting of 65 trials yielded only chance results. Hence Richet experienced already what we are so familiar with. Research with unselected subjects often fails to produce results and research with selected subjects often turns out to be unrepeatable as far as results are concerned.

Rhine's introduction of the forced-choice method can be partly seen as an attempt to catch up with the developments in experimental psychology. He felt a research method was needed which would be acceptable as regards evidential value to his colleagues in the other sciences and reports based on mere observations and verbal material were clearly not sufficient. Since psychology is involved with faculties which are under normal conditions possessed by all human individuals, Rhine probably reasoned that the likelihood of acceptance of ESP would increase if it could be demonstrated that psi was also a common human faculty. As a consequence, it was necessary to abandon the notion that psi abilities would be limited to gifted subjects. Whatever his reasons have been for defending so strongly the notion that all people have psi, the consequence was that research with gifted subjects became replaced by research with unselected subjects. Still it is noteworthy that some of the most impressive results from that period are based on studies with one or a few subjects. Examples are the Pearce-Pratt study with one subject (Rhine & Pratt, 1954) or the Pratt-Woodruff series, in which the high scoring was attributable to only five of the 32 subjects (Pratt & Woodruff, 1939).

Probably because of the controversial nature of research in parapsychology and the fierce debates raging about Rhine's results, little attention was devoted to the assumptions and rationale behind the forced-choice technique. One striking characteristic of this research method is that it appears so different from what happens in spontaneous ESP. It probably originated from the idea that subjects might get spontaneous impressions from the hidden contents of envelopes, a type of experiment sometimes carried out with gifted subjects or mediums. But in the application of the card-guessing technique the idea of obtaining impressions about the target was soon abandoned. It turned into a technique in which subjects in quick succession called out or pointed to cards. Hence one might ask why this methodology was considered so suitable to measure ESP. A common argument ran as follows. When subjects have to choose between a number of symbols or cards, there is no reason to prefer one symbol over the other. All symbols have an equal value to the subject and hence all symbols have an equal probability of being chosen. But if subjects have some psi ability then the probability of choosing the target increases slightly and this will result in a few extra hits in addition to what can be expected by chance. The total number of correct guesses might then lead to a statistically significant result.

Since the capacity for ESP in subjects has in itself nothing to do with the statistical evaluation, an implication of this justification for the method seems to be that the best strategy to optimize results is to use low probabilities for correct guessing. In that case only a few extra hits by ESP will increase the total number of hits beyond the significance levels. But reality soon taught that such is not the case and Rhine settled on the one in five probability which became the standard after the introduction of the Zener cards. Clearly something must be wrong with the above reasoning. One explanation for this lack of relationship between success in ESP scoring and the number of alternatives in the forced-choice methodology might be that calling habits exert a stronger influence on the subject's decision than the relatively weak psi signal. In addition, but this is an assumption perhaps the inhibitory effect of such response bias becomes stronger if the subject has to choose from a larger number of alternatives. But this reasoning violates one of the basic assumptions on which the forced-choice technique seems to rest, viz., the assumption that the probability for each alternative is equal and that there is no reason for the subject to prefer one over the other. However, it is by now well known that subjects do display strong response bias when guessing targets and this indicates that subjects are not neutral as regards their choice of the different alternatives. So the logical conclusion would be that reducing response bias would lead to an increase in ESP scoring. This was investigated by me in an extensive study carried out at the end of the sixties (Schouten, 1975). In order to be able to reduce response bias, I first carried out a number of experiments to study the properties of calling habits. Based on these data I developed a theory of why subjects display the response bias they show so abundantly in their calling patterns.

The main finding was that subjects have an incorrect concept of what random is and that by guessing according to that concept they produce the non-random patterns we observe. This theory was to a large extent confirmed in the final study, the aim of the investigation, in which subjects received training to reduce response bias. This was of course not done by teaching them random response sequences, but by teaching them a different concept of randomness than what they had before. In fact, the aim of the training was to teach subjects not to employ any concept or strategy when guessing targets.

The experiment succeeded quite well in the sense that all subjects learned to guess targets with significantly less zero order and sequential bias. Of the 34 subjects, 28 subjects managed after on average 3.5 sessions of training to produce response sequences which fell as regards response-bias within the significance levels. Since each response series involved 400 calls it can be concluded that there has been a real reduction in response bias and not merely a reproduction of specific prelearned sequences which statistically conform to criteria for randomness. This conclusion was supported by the finding that the speed of calling had increased and was higher for the latter, more random series. The calling speed for the random series turned out to be on average one guess in three seconds. This value does not indicate that subjects were consciously trying to construct random calling sequences. Hence I felt that in this experiment the reduction in response-bias has been real, and that in the last most random series in most guesses the alternatives had had an equal probability of being chosen.

The experiment was carried out with the aim of enhancing ESP scoring, but unfortunately the reduction in response bias did not result in higher ESP scores. To me that seemed to indicate that at least some of the assumptions on which the forced-choice method rested must be wrong. But the forced-choice method has more peculiar characteristics which give rise to doubt of its appropriateness as a research methodology for parapsychology.

After much discussion about Rhine's experimental results it became clear that merely demonstrating a significant excess in hits in an ESP forced-choice experiment did not yield much in terms of increase in knowledge, nor did it help to convince skeptics that ESP should be considered a proven phenomenon. So more and more studies appeared in which scoring in card guessing were compared under different conditions. But when the same subject was tested under different conditions the forced-choice method did not allow comparison with the number of ESP hits of that subject in the two conditions, but only the number of total hits which are made up of both "chance" hits and ESP hits.

Consequently only in the case of strong ESP effects or large numbers of subjects having some ESP ability might it be possible to find a difference between conditions. And even if such an effect is observed the size of the effect will probably not reflect the actual difference in ESP scoring. Hence it is no surprise that these studies often failed to have results and in so far as they did succeed the effects were nearly always small and often inconsistent.

A similar story can be told of the methodology applied in PK research. Here the study of macro-PK phenomena was replaced by the dice-throwing technique. At that time PK was still seen as exerting a mental "force" on the objects. This makes the PK dice-throwing technique the more surprising. There is no doubt that physically and neurologically it is impossible for human subjects to follow exactly the falling of a number of dice, and to predict during the fall how the dice end up when they come to rest. Consequently how could one expect a subject to know what force should be exerted and at what point, assuming that the subject would have been able to apply such a mental force. In fact, the dice-throwing paradigm for PK rested on the assumption that subjects possessed two magical sort of abilities: not only PK in the sense of exerting a mental influence on matter but also an ESP ability to know when, where, and to what extent to apply that mental "force."

Even if the assumptions of the forced-choice technique are correct, and assuming that most people are able to exert psi abilities, then still the forced-choice technique is so insensitive that it would be unrealistic to expect consistent results over experiments. The contribution of "chance" hits to the score, the total number of hits, is simply too high compared to the effect of ESP on the scores. In research in other fields the situation is different. In learning experiments scores reflect largely learning, in perception research scores reflect largely perceptual abilities and so on. With the forced-choice technique the scores reflect mainly randomness. In addition, the statistical techniques we apply are mainly developed for research in which the scores do largely or entirely reflect properties of the phenomenon under study. Thus in an analysis of variance the "noise" is reflected in the variance of the scores, but the scores themselves are representative for the phenomenon under study.

Although this discussion has only touched upon a few aspects of the forced-choice methodology it suffices to illustrate that many questions can be raised about its appropriateness for application in research in parapsychology. Especially in view of the way Rhine and his followers thought about ESP the method seemed not well suited. However, I know well that in the end it is never such considerations which decide whether a research method becomes widely used or not, but that other aspects of a more pragmatic nature are decisive.

Very attractive properties of the card guessing paradigm are its simplicity, the speed with which data can be collected and its cheapness. No complicated equipment or housing facilities are needed, a simple deck of cards suffices. But above all the most important consideration has probably been the fact that, despite the inherent improbability, the method seemed to work and nothing attracts more following than success.

*The Third Period: A Variety of Research Methods.* The sixties of this century can be viewed as a transition period, not only in a cultural sense for Western societies, but also for experimental parapsychology. It can be considered as the beginning of the third period in the development of research methodology in parapsychology.

Dissatisfaction with the progress in the field, a feeling that the possibilities of the nearly generally applied Rhinean methodology of forced-choice ESP and dice-throwing PK were exhausted and, above all, growing doubts about the success of the forced-choice paradigm as a research technique for studying ESP, created a strong interest in other, more promising methodologies for research.

The 1968 Parapsychology Foundation Conference, which was also attended by scientists of repute from other fields such as Karl Pribram, Henry Margenau, and W. Grey Walter, reflected this mood. At this conference the discussions, which were limited to ESP, concentrated mainly on three new techniques: free-response studies coupled with ASC induction as exemplified in the dream research, psychophysiological studies, and animal research. To a lesser extent this tendency also showed at the 1968 PA Convention held in Freiburg, Germany. Of the four PK studies reported only one was of the forced-choice type. As regards ESP studies the forced-choice method still dominated, but of the 18 already 7 employed other than forced-choice techniques. Within a few years this trend resulted in the nearly complete disappearance of the dice-throwing studies and in a strong reduction in the application of card-guessing in ESP research. What replaced the old methodology was a much wider variety in research methodologies, of which two techniques became strongly dominant. In ESP research this was the free-response approach, especially in combination with the ASC induction technique of the ganzfeld sensory isolation procedure. In PK research the RNG studies became the standard approach.

Whatever its further merits, it can be argued that, as regards its assumptions, the research methodology of the last 20 years is much better adapted to what we know or assume about ESP. Free-response ESP is more comparable to spontaneous ESP. It can be considered as a method to provoke spontaneous ESP under more or less controlled conditions. Not only does it better reflect ESP as it shows in daily life, also the theoretical background of the free-response technique as it is applied in research appears more plausible. Given the assumption that ESP exists and can be considered an ability, the noise-reduction model as applied by Braud and Honorton seems logical. At least this approach does not carry with it the inherent inconsistencies from which the forced-choice methods suffer.

Another important advantage of the free-response technique over the forced-choice method is that in principle the free-response method yields scores which are more representative of the degree of ESP transmission. That is because free-response scores are based on agreement in various aspects between mentation and target. On the other hand, there are still many problems not really solved at the moment which diminish the value of this method. Because an essential feature of the free-response technique is that the content of the subject's experience matches the target, the evaluation should be based on the degree of agreement between mentation and target. But we still lack sensitive evaluation techniques to do this and therefore the free-response experiment is often evaluated as if it had been an extremely time-consuming, guessing task.

Another problem which needs more attention is that hardly any studies have been carried out in which a direct comparison has been made between the effectiveness of free-response studies in comparison to other methods. The best we have are indications as provided by Honorton (1978, 1985, 1986, and also at this Conference), based on meta-analyses, that in terms of relative number of significant experiments free-response studies, especially the ganzfeld variety, are superior to forced-choice techniques. However, the most convincing evidence can only come from studies in which the scores from the same subjects obtained with the different techniques are directly compared and when it is found that the free-response condition does yield higher scores.

Two other promising new techniques were also extensively discussed at the 1968 Parapsychology Foundation Conference: psychophysiological research and animal studies. For these two approaches it also holds that the rationale for the techniques makes sense. Psychophysiological studies can either yield indications that certain bodily states are conducive to ESP or that psi effects which are still unconscious to the subject are detectable by reactions in the organism. Animal research rests on the rationale that if ESP is a property or ability associated with humans it is also very likely that it can be found in other biological organisms. In addition there is some evidence that animals sometimes succeed in feats which are difficult to explain even when accepting extreme sensorial sensitivity.

Hence it can be concluded that the present methodology applied in ESP research seems better founded and more appropriate to the phenomena than the forced-choice methods and in that respect we might say that progress has been made. However, this holds only insofar as ESP phenomena are assumed to be the result of a still unknown ESP process. The philosophy of the Parapsychology Laboratory in Utrecht has been that other models should also be considered in our search for the explanation of paranormal experiences. We know that various psychological factors must have an effect on ESP experiences. So it seems worthwhile to explore also models which do not assume an ESP process, but try to explain these experiences by applying psychological concepts such as, for instance, attribution. In such an approach research would for instance, be aimed at studying when and under what circumstances people have ostentative ESP experiences in their lives and what function these experiences have. I feel that this approach has been too much neglected by parapsychologists and in this respect more progress could be made. It has been more or less left to the critics, but they have made little contribution in this direction.

On the surface, the present-day RNG PK studies look like a modernized version of the dice-throwing technique. The difference, however, lies again in the degree of association between the type of phenomenon we think we are dealing with and the research methods applied. As shown above dice-throwing is a somewhat illogical test procedure when PK is considered as an ability to exert a mental "force" on material systems. The RNG studies, however, are based on an entirely different concept of PK and the methodology applied seems well-suited to that concept. The work of Schmidt and Walker have yielded the different versions of the observational theories. Others have presented novel theories like von Lucadou and Kornwach's "Model of Pragmatic Information." All these theories have in common that only random processes can yield PK effects. Thus as regards modern PK methodology we might even say that in this case theory came more or less first and that the research methodology was derived from it. In that aspect it appears that the research methods we apply now in PK are optimally suited to the phenomenon under study.

There is still a problem, though. Although the modern theoretical views on PK seems to many to reduce strongly the incompatibility between the concept of PK and modern physics, a view I do not share, it has increased the difference between PK in the lab and what can be considered spontaneous PK. Even if we reject all claims of the physical mediums, then we are still left with the problem of explaining the poltergeist phenomena. And, poltergeist phenomena are more suggestive of forces exerted on stable macro-objects than that they suggest OTs as an explanation. Hence as regards spontaneous PK one might argue that the present research methodology is rather a step back instead of an improvement. It appears that OTs are better suited to explain ESP phenomena than spontaneous PK and that therefore the micro-PK studies are rather to be considered as part of the research into ESP. Apart from descriptions of poltergeist cases there is not much research carried out anymore which might increase our knowledge about spontaneous PK phenomena.

There are of course more methods applied at present in research in parapsychology than the ones discussed above. For instance, there is research in the analyses of spontaneous cases where in my opinion also good methodological progress is made. But free-response and micro-RNG studies are by far the most popular, even to the extent that they tend to one-sidedness.

The overall conclusion seems to me that as regards the relationship between the phenomena, or theoretical concepts about the phenomena, and the research methodology good progress has been made in ESP research, with the exception of the "psychological" approach, but little in research is aimed at explaining spontaneous PK.

## *II. Does the Research Methodology Yield Results?*

The fact that the research methodology becomes better adapted to the phenomena under study does not automatically imply that the research will also become more successful. It seems, however, an essential condition which must be fulfilled before success can be expected. The evaluation of success of research depends n many factors, among others on how success is to be measured.

*By What Criteria Can We Judge Progress in Science?* Success or progress in science is a multi-dimensional concept. Its evaluation depends on the criteria one applies to judge progress by and of the level to which the current position is compared. Many fierce debates in parapsychology have actually been discussions in which implicitly a specific definition of progress was applied. One example is the frequent discussions on the repeatability issue. From these discussions it appears that authors sometimes base their opinion on the often not outspoken assumption that no progress can be made at all unless we have a repeatable experiment. A very specific concept of progress indeed. Another example is the critic's assertion that a hundred years of research in parapsychology have not yielded any results and that therefore the subject can be discarded. That judgment is mainly based on the perceived lack of control and predictability in parapsychology. Consequently, they apparently consider these criteria as conditions which have to be met before any degree of progress can be attributed. In its most simple form the concept of progress involves the following elements: starting point, the present position, the distance to the position one wants to reach and the speed with which the present position was reached. These elements constitute a scale and as with every scale it assumes a dimension along which the scale is to be used and a unit to express distances on the scale.

In science the question of which dimension one should apply to measure progress is rather unclear. When criteria of a practical nature are chosen then progress can be expressed in, for instance, the increase in number of research institutes, or funds allocated to research, or increase in applications from applied research as expressed in the number of patents awarded. With such criteria the problem of what unit to select to measure progress by is automatically resolved: the number of institutes, or dollars, or patents. However, such criteria are better suited to be applied to more developed sciences than to a science in its infancy such as parapsychology. In the special case of parapsychology another criterion could be acceptance by and integration into the other established sciences, which can be expressed in the number of university affiliated research institutes or professorships. Although all of the above mentioned criteria are important dimensions of progress, most people will first of all associate progress in science with increase in knowledge. But progress in knowledge is rather difficult to define. There is often no evident criterion to apply to the concept of knowledge, and no simple unit exists to express degree of knowledge in.

Hence what often happens is that progress in knowledge is expressed not in terms of increase, but in reaching a certain level of knowledge. Thus Lakatos, for instance, offers us a criterion that progress in science exists when the theoretical growth anticipates the empirical growth, i.e., when theory proves successful in predicting novel facts (Lakatos, 1978, p.112). Another example is the above-mentioned criterion of the repeatable experiment. It is clear that in parapsychology, as well as in many other sciences, considerable progress must have been made before such levels of knowledge are reached. In fact, in my opinion, the repeatable experiment, in the sense of repeatable results of experiments, in parapsychology will only be realized after we have obtained more or less full knowledge about ESP. It will be the result of our research efforts rather than a condition which has to be met before we are allowed to do research.

Criteria for progress as mentioned above, which are based on reaching a certain level of knowledge in the future, seem to me rather useless for assessing the present state of affairs. There is simply no way to predict when or whether a certain level of knowledge in the future will be acquired. The best one can do is to express the noble wish that such levels will be reached. To state that a field can only be considered a science after such levels are reached is in fact denying that field the chance to ever become a science. Because, especially in developing sciences, there is no unit to express degree of knowledge. Such levels, even if it was certain that they would be reached one day, are also useless as a point of reference as compared to the present situation. Even assuming that psychology would one day reach the level Lakatos requires, it is impossible to say, for instance, that psychology has now covered one-third of that way. The same holds for parapsychology. It is possible that one day a consensus will exist that a specific research method yields repeatable demonstrations of ESP, but there is no way to predict when that day will be and how far we are away from it. It might be around the corner, as some people believe in the case of the ganzfeld technique, or it may be centuries away or perhaps that day will never come.

Thus for various reasons it appears unsuitable to apply future levels of knowledge as a criterion of reference to express the present state of progress. Hence we are forced to evaluate progress by comparing the present situation with the situation some time in the past. However, that does not solve the problem that we lack a clear unit to express increase in knowledge. Suppose we accept as an item of knowledge that sheep score better in ESP tests than goats do. How can we compare this with for instance the finding in psychology, that male infants are more irritable and physically active than female infants (Gleitman, 1983, p.320)? Which of the two statements involves more knowledge or indicates more progress? Because the value of such bits of knowledge partly depends on the meaning individuals attach to them, and because that meaning depends on interest and various other "subjective" criteria, there is no meaningful and objective way to compare the value of different statements of knowledge. For a parapsychologist the first statement is of more importance than the second; for a psychologist working with children the reverse holds.

Another important aspect which makes it nearly impossible to compare the respective value of items of knowledge from different fields is the uncertainty concerning the validity of the findings. How certain can we be of the statement that in an ESP experiment sheep score better than goats? There are hardly any findings in parapsychology, nor in the social sciences, which are unchallenged and can be considered undisputable. In our field it is the rule and not the exception that positive findings become immediately criticized. In fact, it sometimes appears to me that this is one of the favorite pastimes for some people in the field. But to a lesser degree the same can be said of psychology. Therefore it seems not very useful to base a discussion about progress in a field on specific findings from that field and the value which should be attached to these findings. Hence unless we are willing to restrict the assessment of progress to the simple conclusion that we now know more about parapsychological phenomena than people did in the past, an assessment I am willing to endorse, we need some other methods of reference to indicate the degree of progress. To this end I propose to apply the following criteria to assess progress in parapsychology:

1. The extent to which research has been able to reject incorrect ideas about the phenomena.

2. Does the research have an effect on changing opinions in society about its subject matter?

3. How does the progress in the field compare with the progress in comparable fields?

4. Can the field be characterized as a cumulative science?

*The Rejection of Incorrect Ideas about Paranormal Phenomena.* The first criterion concerns the rejection of incorrect ideas or explanations. When the present situation is compared with the past we cannot say that research started with nothing and that now thanks to research we have obtained certain ideas about the phenomena. Science is often seen as a development in which no knowledge is gradually replaced by knowledge, but that is in general not true. When we are dealing with spontaneous and observable phenomena it is seldom the case that in the pre-scientific stage people did not have ideas or assumptions about the explanation of these phenomena. Such ideas exist prior to and in every stage of the development of research. The development of science is therefore better characterized by a gradual rejection of many incorrect ideas before proper explanations are found. Thus in the beginning of research the elimination of incorrect views on the phenomena plays a dominant role and can be considered a condition which must be fulfilled before real progress can be made. Indeed, there are many examples in the history of science that especially when a field started to develop, erroneous ideas or concepts for long periods of time effectively prevented the development of fruitful research. Therefore it makes sense in the case of a developing science to express progress in the degree to which the field has been successful in correcting and rejecting incorrect views on its subject matter.

In the case of research into paranormal phenomena we can certainly find examples of how research has gradually resulted in the rejection of once commonly held convictions about these phenomena. But one can never say that research alone brought these changes about; other developments in science and society have undoubtedly also contributed to them. For example, when research started, about 100 years ago, the spiritist hypothesis dominated. On this hypothesis ESP phenomena were mainly considered as an act of the deceased. Since experimental evidence does not support that hypothesis it has dropped out of the field. Another common notion about ESP was based on the telegraph model. A sender was supposed to take action as regards transmission of a "message" and only then could it happen that the percipient by telepathic means "received" that message. Despite the fact that we still employ agents and percipients in experimental settings, I think that few researchers support that model anymore. The principal role of the experient is now generally accepted, as is so for instance, exemplified in Stanford's PMIR model or in the ASC studies. Other examples can be found in changing convictions about the limits of ESP. Although theoretically we still toy with the idea of the omnipotence of psi the limited effects of ESP in reality are now well recognized.

Thus we know that it is not possible to pick the brain of another person by ESP, or to use ESP to find out what's going on inside the White House or the Kremlin. Also we can assure people that it is not possible to use PK to make other people do what we want. A few more examples of which I feel the results of our research will have an impact on at present are still widely held opinions. It is believed by many that mediums or paragnosts are either swindlers or people gifted with ESP who are able at will to obtain paranormal impressions. Research indicates (Boerenkamp, 1988) that both opinions are probably incorrect. Most mediums do believe in their "paranormal" abilities but from our point of view they are not able to demonstrate them. Mediums might have an occasional spontaneous ESP experience, but their success with people seem mainly based on normal psychological abilities. Another example is provided by paranormal healing. Although more research is needed in this area the findings so far suggest strongly that paranormal healing is effective, but not because of a PK influence or other unknown influences exerted by the healers (Attevelt, 1988).

Although parapsychologists might differ in their views and not everyone will endorse the opinions stated above, it can not be denied that parapsychology is a field in movement. Thanks to research, and often the lack of result of research, we learn the limits of the phenomena we study and are forced to adapt our opinions accordingly. The field is not stagnating and in this respect we can be considered to make progress.

*Do the Results of Our Research Affect Society?* Closely related to the criterion of the rejection of incorrect ideas is the question whether progress in the field is reflected in changing opinions and attitudes in society about these phenomena, especially changes in misconceptions or harmful practices. Often a relationship will appear between progress in a field of science and the extent to which people change their attitudes or behavior towards the phenomena in question. Medicine constitutes a good example in this respect. The progress in that field has gradually changed people's attitudes towards diseases and their opinions about what should be considered healthy and unhealthy ways of life. A field which makes no progress in knowledge will hardly be able to affect people's opinions. Therefore a second criterion to judge progress by is to consider the changes in attitudes and opinions about paranormal phenomena of people not directly related to research in the field.

In this respect I feel that our progress is less than it could have been, but it is difficult to judge what one could reasonably expect. In general there appears a time lag between the acceptance of new insights within a science and the time that this knowledge penetrates society and becomes commonly known and accepted. For instance, quantum physics dates from the twenties, but started to spread in society in the sixties and seventies. In other areas, however, as in the case of medicine, the dispersion of ideas seem to take place at a faster rate.

There are examples which indicate that changes in opinions within the field affect general opinions outside the field. One is the above mentioned rejection of the spiritistic hypothesis and the growing conviction that ESP is a form of human experience. On the other hand, it is clear from most of the popular and unscientific literature dealing with paranormal phenomena, the "paraporno" as Martin Johnson calls it, that many misconceptions are still very much alive.

As regards the "paraporno" I feel parapsychology has failed to take a more aggressive attitude towards this nonsense. Parapsychologists know best what the possibilities and impossibilities are as regards psi phenomena and hence it is more or less our responsibility to present this knowledge to society and to take action if views are presented which are at variance with what we know. However, I realize well that in some respects we have a more difficult task here than scientists normally have. One is the lack of resources in the field. The few people who do research in parapsychology work in general under rather adverse conditions and simply lack the time. Another negative condition is that parapsychology is one of the few branches in science which is systematically persecuted by organizations who are critical of research in this area and accordingly try to lower the status of its researchers. One consequence of this is that much time is wasted on rather useless debates, time which could have been used in more meaningful ways. Another is that the impact of what we say on parapsychological matters is less than it could be. That is a pity, because this effectively strengthens the position of the "paraporno" producers.

*The Progress in Psychology as a Reference for Comparison.* The above discussed criteria for progress remain restricted to the field itself and, although informative, do not say much about what value we should attach when, according to these criteria, progress or lack of progress is observed. The statement that a certain car carries a price tag of $10,000 tells something about that car but becomes really meaningful only when that value is compared with the price of comparable cars of other makes. Thus a meaningful evaluation of progress in parapsychology can only be made based on a comparison with the situation in other fields of science. It stands to reason to select for such a comparison a field of science which in most respects resembles parapsychology and, in my opinion, the best choice for this is the field of psychology. Hence as a third criterion to measure degree of progress I propose to compare the investment in resources and progress in parapsychology with those of psychology.

As explained above, it is difficult to compare two fields by comparing the relative value of the knowledge obtained in these fields. The best one can do in this respect is to form some global impressions. What we can do, however, is to compare more concrete issues which are related to the matter of progress and the validity of the applied research methodology. Such issues are, for instance, the above discussed aspects related to the impact research has on society, which in the case of psychology means mainly its usefulness. Or issues such as whether the fields have reached the stage that repeatable experiments are carried out, or that the foundations of a solid theoretical framework as a basis for the whole field are established. In addition, a fair comparison should also consider the differences in resources between the fields to be compared. In the following, therefore, I will discuss the state of affairs as regards the development of knowledge and some other important issues in the two fields. In addition I will try to give an estimate of how the resources in the two fields compare.

*Opinions of Psychologists About Progress in Research in Psychology.* It is outside the scope of this paper, and beyond my capacities, to provide a detailed discussion of all claimed findings in psychology, and the pro's and con's of the research arguments which support these findings. What can be said, however, is that in contrast to a science like physics most findings and developments remain disputable. Newton's laws are generally accepted, but psychology hardly knows any laws and few results of psychology go by unchallenged. It is striking that the most solid findings in psychology are those in perception research and psychophysiology, especially in areas which deal with the neurophysiological basis of perception and behavior. Psychology as a science of behavior and cognitive psychology seem to yield less convincing results whereas psychology as the layman views it, the study of inner experiences, seems hardly to have any consistent findings at all. These seem perhaps bold statements, but I think many examples can be found in which psychologists of repute offer explicitly or implicitly similar views.

Take for instance the not insignificant field of attitude-research.

Abelson (1988) discusses the problems concerning the findings in this area after decades of research and mentions as the most important:

1. The measured attitudes turn out to be poor predictors of behavior despite the presumed meaning of attitudes as predispositions toward behavior;
2. Respondents often conform to the demands of the questionnaire by concocting superficial attitudes on the spot. Such attitudes are extremely labile over time and have come to be called "nonattitudes";
3. Procedures which create changes in attitudes in laboratory research fail to do so outside the laboratory.

In fact, in my opinion this amounts to admitting that attitudes can not be reliably measured and that findings in this area have no relevance outside the laboratory.

Another example presents the continuing debate about the validity of clinical psychology and its practical applications. As recently as 1982 several publications were devoted to the question of whether a meta-analysis carried out on 375 studies of psychotherapy and counseling justified the conclusion of the authors that psychotherapy does work. (See among others Landman & Dawes, 1982.) And this concerns one of the oldest and most extensively researched areas in psychology. But similar remarks can be found for other disciplines as well. Pion and Lepsey (1984) state that "Many critics have argued that psychology has a less than impressive record in understanding and explaining human behavior" (p.743). That statement can undoubtedly be more strongly formulated when it concerns human experience. Or take Fishman and Neigher (1982). They state: "our discipline's own admission through writers such as Cronbach . . . Epstein . . . and Wachtel . . . is that the research accomplishments of psychology have been disappointingly small" (p.542). Wachtel (1980): "Nonetheless, the state of our field seems to me to leave much room for discontent" (p.399). Gibbs (1979) wrote,: "In perception and memory, in learning and development, in social influence and attitude change, one hears the same lament of trivial and irrelevant research" (p.127), and adds: "Those voicing laments and pleas include some of the most prominent names in modern psychology" (p.127). He continues these statements by citing many examples.

The generally felt doubt in psychology becomes already apparent in introductory books on psychology, such as, for instance Gleitman's *Basic Psychology* (Gleitman, 1983). Despite the fact that in this book a fairly rosy and optimistic picture is given about psychology's findings, it is striking how often one finds cautionary remarks offered and conflicting opinions presented. Already the style in which the field of psychology is presented is entirely different from for instance an introductory book on physics.

The above cited opinions from psychologists, and many more can be found, suffice to indicate that within the field of psychology serious doubts exist as to the progress it is making and as to the validity of most of its findings. In that respect psychology is not much different from parapsychology. Our field suffers from similar uncertainties as regards the validity of its findings. What is important, though, is to realize that we are not the only ones who suffer from this feeling of uncertainty. Therefore the tendency often seen in our field, to blame this uncertainty on our subject matter, or research methods, or to use it as a starting point to discuss the question of whether parapsychology can be a science at all, seems to me strongly exaggerated.

Physics was not built in a few centuries, and for various reasons physics might turn out to be an "easier" science to develop than psychology or parapsychology, if only for the reason that physics could start with a large number of phenomena which by nature are already consistent and repeatable, and which can be isolated, a type of phenomena the human sciences largely lack. That some psychologists feel the same is for instance voiced by Wachtel (1980): "Psychology is about the hardest discipline to do research in" (p.403) and further on: "To do really good research in psychology, research that really breaks new ground or gives definite answers to important questions (as opposed to research that simply makes it into journals) is exceedingly difficult" (page 403). If that can be said about psychology, it certainly holds for our field.

*Important Issues in Psychology and Parapsychology.* In addition to the criterion of research findings the two fields can be compared as regards aspects which are in a different way also indicative of the level of progress. Examples are such issues as repeatability or applicability of the findings. It is no coincidence that such aspects coincide largely with important criticisms leveled against parapsychology. In fact, I believe that a main reason for finding so many experimental psychologists among the fiercest critics of parapsychology is that parapsychology functions as a kind of mirror which magnifies strongly the weaknesses of psychology itself. For reasons of space I will only consider a few, especially those who are of relevance for the issue of the methodological approach.

The rather exhausted subject of repeatability, the topic of the 1983 Parapsychology Foundation Conference, is not only of great concern to parapsychologists. Although the opinions differ, I suppose that most parapsychologists agree that as yet we have not found the repeatable experiment with which to demonstrate psi, or, more accurately, to demonstrate specific effects from conditions on psi. I concur with that position. However, here again we are not such an exception as many seem to believe. Westland (1978) flatly states that "numerous literature studies of surveys (in psychology) have shown that reports of replications of "successful" research studies are rarely published" (p.98).

I have already mentioned Abelson (1988) who implicitly states that in attitude research the degree of uncertainty is so large that most findings must be considered as unreplicable. Also Fishman and Neigher (1982) speak about "single-study experiments with data that are unreplicated, under aggregated, and biased" (p.542). In general it is felt that replicability in the sense that it allows predictions to be made, or that it yields reliable applications, is very poor in psychology.

An area closely related to repeatability is the usefulness or application of research findings. Effective applications can only be based on solid and repeatable findings. Therefore lack of applications or doubts about them tells a lot about the degree of repeatability of the findings on which these applications are based. Here again psychologists themselves are aware of the dubious nature of many of their achievements. Helmreich (1983) complains about the limited influence of psychology on aspects of spaceflight: "One can . . . assign responsibility to the investigators for producing products of dubious utility" (p.447). Bouchard (1976) when discussing laboratory research is of the opinion that "Laboratory experiments . . . lend themselves to unjustified and often erroneous extrapolations" (p.364). This view does not create much confidence in the applications based on that research. In the same vein Chapanis (1976), when writing on Engineering Psychology, signals that: "Most laboratory experiments in psychology have only very limited relevance for the solution of practical problems" (page 730).

Fiske (1979), in an article adapted from a Presidential Address at an APA meeting expresses serious doubts about the whole area of personality research and even believes that it will never develop into a science (p.738). I have already mentioned the doubts which exist about the applicability of research in clinical psychology. This is also reflected in the cautionary statements which are made in the *Report of the President's Commission on Mental Health* in 1978, cited in Parloff (1979) where statements are found like: "Treatment by various types of psychotherapy is as yet of unestablished efficacy" (p.300) or "follow-up studies generally indicate that failure or success appears independent of the type of treatment received", etc. (p.300). As to other important aspects of research, Westland (1978) mentions among others the following crises in psychology: The Usefulness Crisis (is there any reason why the science of psychology should be considered relevant?); The Laboratory Crisis (is laboratory experimentation capable of producing results which are valid outside its walls?); The Science Crisis (is psychology a science?); The Professional Crisis (who or what is a psychologist?); The Publication Crisis (mainly studies are published which "turn out"). Nearly all of them apply to parapsychology as well, but it seems to me that fortunately we are more aware of it.

So it appears that in many respects the situation in psychology is rather similar to our situation. The difference seems to me more to be found in the differences in the nature of the subject matter of the two fields and the differences in size than in their respective levels of progress. Psychology deals mainly with phenomena which are experienced by all people and hence are taken for granted. People are not inclined to question the existence of phenomena they don't understand provided they experience them daily. The functioning of the brain, the riddle of the mind/body relationship, the miraculous capacities of memory, perception, language, etc., are all taken for granted because everybody experiences them. However, parapsychology is dealing with experiences which are relatively rare and of a spontaneous character, and therefore they are less easily accepted. Nearly all of the problems discussed above which trouble psychology apply to parapsychology as well. Of course, that is little comfort to us. I am certainly not suggesting that our situation looks better because psychology is not the hard science as is implied in the sometimes arrogant attitude towards our research displayed by psychologists. On the contrary, it is a regrettable situation, because in many respects our progress depends on the progress in psychology.

*How Do the Resources in the Two Fields Compare?* It is clear that both fields have serious problems as regards their striving to become a progressing science. Without further study it is also clear that the two fields must differ considerably in resources. Critics sometimes love to argue that 100 years of research in parapsychology has failed to produce a reliable demonstration of ESP. Since a simple experiment can never constitute a reliable demonstration, they mean in fact that 100 years of research have failed to yield the knowledge to enable us to control the phenomena and to demonstrate ESP at will. This is correct, but as was discussed above this applies to many findings in psychology as well. Moreover, the "100 years" sounds very suggestive, but does not take into account on how much research capacity this supposed "failure" is based.

In order to compare the two fields as regards resources a rough comparison suffices. In this comparison I will restrict myself to the human resources, thereby assuming that the research facilities for individual researchers would be more or less equal for the two fields. This is of course not true; psychology has in this respect a clear advantage over us. As regards human resources in psychology the latest data I found are from Stapp et al. (1985) who in 1983 and 1984 polled the entire American population of psychologists. The size of the population turned out to exceed 100,000. The investigators managed to obtain an 82% response rate which resulted in 81,500 responses which could be used for the evaluation. Of these psychologists 74,417 were employed and according to table 8 of the publication, 34,022 of these were involved in research activities. Hence we can assume that the human research investment in psychology in the United States for one year can be set at about 34,000.

If we consider research in parapsychology the picture appears somewhat different. I have not taken the trouble to count for each year how many persons in the United States might have been involved in research. But it seems to me that if the last 100 years is considered, for most of this period it have never been more than perhaps 5 to 10 persons. But to stay on the safe side I will put the figure at 50 a year, which is clearly exaggerated because I don't think that any year can be found that so many people were involved in research in this field. In that case the 100 years of research in parapsychology would amount to a total of 5000 research-years. That implies that the entire investment in parapsychological research in the United States is equivalent to less than two months research in psychology in 1983. If we include foreign countries the picture becomes even worse, because perhaps apart from Britain and Holland the situation in the United States can be considered to be rather favorable for research in parapsychology. Thus in terms of resources we can just as well turn the critics' argument around and ask: "What did two months of research in psychology yield to justify further investment of such huge resources?"

I realize that this comparison is over-simplified, since in the 100 years of our research we profit from developments in other sciences which will not be possible to such an extent in a two-month period. Since both psychology and parapsychology are extremely difficult research areas, for reasons I won't discuss here, the critic's opinion about the results of our 100 years of research and the dissatisfaction which is often noticeable within our field seems to me rather a consequence of a wrong estimation of how fast research in these areas can proceed, than a realistic evaluation.

*Is Parapsychology a Cumulative Science?* A fourth criterion for establishing progress lies in the type of relationship between the different items of knowledge which are obtained in a field of science. One can roughly discriminate between two types of collections of items of knowledge. One is a set of items of knowledge with little or no relationship among the different elements of the set. The other is a structure of knowledge with strong interrelationships which can be said to be cumulative in nature. That is, new theories or findings encompass facts or theories which were until then unrelated and these new theories lead to new findings which again result in expanding the scope and explanatory power of the field. Thus research fields can be judged and compared as to the nature of their body of knowledge. This criterion seems to me one of the utmost importance for the evaluation of a field of science.

A science which is characterized by the first type of knowledge can be said to progress, but only in the sense that each time items of knowledge are added. I will call this a collecting science. The value of this process of information gathering is unclear because little is and can be done with the increasing amount of information. It is as if one collects rocks of different sizes and colors and keeps them lying around in the backyard. A science which displays progress in a cumulative sense, which will be called a cumulative science, is clearly much more successful. Such a science not only collects the rocks, but puts them together and constructs a house with them. The difference between a collecting and cumulative science becomes in many ways apparent. An important one is the way the direction of research is established. A cumulative science, like for instance physics, is characterized by a steady and logical development of research methodology and research topics. On the other hand a collecting science like the social sciences, which is only able to add more items of knowledge to the already existing collection without integrating them, is characterized by fads and fashions. The application of new technologies, mainly introduced from other sciences, and new subjects which become fashionable follow each other one after the other, but with little consistency from past to present.

The above characterization of two different types of sciences is of course rather abstract and does not do justice to the great variety which exists within the different sciences. Thus although I feel that as a whole the social sciences are characterized by a rather meaningless collection of tidbits of knowledge it is undeniable that certain specializations within the field, especially those who are closely related to the beta sciences, grow more and more into the direction of a cumulative science.

Nevertheless, I will not discuss it further here for the simple reason that I consider both parapsychology as well as psychology still collecting sciences. Both are characterized by changing fashions in research. Each

PA Convention demonstrates the variety in research methods and subjects of investigation. As regards psychology it suffices to compare two volumes 20 years apart of one of the popular journals for publications of research data. The two volumes will yield an impressive amount of publications. In nearly all these publications, which cover a wide variety of subjects, significant effects or correlations are reported. But it is likely that somebody who is not familiar with the development in psychology will have a hard job to tell only from the contents which volume is the older one. And to add some comments from within psychology which support my views in this matter: in Fishman and Neigher (1984) it is observed that the present situation in psychology: "encourages large numbers of . . . irrelevant . . . experiments . . . The result is that our information wheels spin very fast but make little progress toward cumulative scientific knowledge" (p.542). Dr. Wachtel (1980) comments: "it certainly seems that (to put it kindly) our studies in psychology tend to be of . . . enduring interest. A good 1950s study in the area of personality, for example, could, I contend, get published readily today . . . as an interesting new finding. Our rate of obsolescence is rather low" (p.399). Indeed, a very sympathetic way of phrasing.

If we compare the progress in research in parapsychology in its 100 years of existence with a comparable two-month progress in psychology, and if we take into account what psychologists themselves think about their progress obtained in more than 100 years of research in psychology, then I think there is no reason to feel that we are doing worse. From this I conclude that the rate of our progress, however slow it may look to some, is in itself no reason to express doubt about the research methodology we apply.

*Conclusions.* According to the criteria applied we can conclude that our field is progressing, although slowly. The research methodology appears to become better adapted to the phenomena we study. The field is certainly not stagnating as regards the rejection of incorrect models or the introduction of new theories and approaches. Considering the differences in investment of resources our progress seems at least comparable to those of psychology. Hence the degree of progress in the field appears not a sufficient cause to change dramatically the research methodology currently applied in parapsychology. However, like psychologists I feel that we also have reason to express disappointment with our achievements. But from the situation in psychology and in the other social sciences it follows that there are probably common reasons why in all these fields progress is so slow. Hence it is likely that our possibilities to increase progress and to find better research approaches by trying to make improvements within our field are rather limited. We will remain a small research field with little resources and hence our possibilities for improvement of the situation will remain largely dependent on progress in other fields of science.

That does not excuse us from trying to strive at better and more efficient research procedures. The history of parapsychology has shown that such improvements are possible. I am of the opinion that both as regards research in spontaneous experiences as well as in experimental research improvements have been made. In general, parapsychological experiments show a constant improvement for instance as regards the elimination of sources of error. We should be glad that so many people outside the field are taking the trouble to criticize our work because that gives us the opportunity for further improvement. And further improvement in research methodology seems to me possible.

*Suggestions for Improvement of Research.* First I would like to propose to invest more research into the relationship between psychological variables and spontaneous ESP. As I have argued elsewhere (Schouten, 1984) we should replace the metaphysical, proof-oriented approach in parapsychology by a pragmatic approach. The latter involves that: (a) we should strive to explain paranormal phenomena or experiences and (b) that in these attempts we should keep an open mind for the possible effects of both parapsychological and psychological processes. Parapsychological experiences cannot be seen isolated from the rest of the personality of the experient and consequently psychological processes must play a role in ESP experiences. Therefore I feel that more research should be devoted to the study of spontaneous paranormal experiences, what function they have and how they fit into the experient's life.

I have no doubt that some ostentative spontaneous ESP experiences are in reality coincidences to which the experient for psychological reasons attributes a paranormal character. It is necessary that we learn to distinguish such experiences. Then we might be able to better understand under which psychological conditions experiences occur which might be classified as "real" ESP experiences. If such research leads to the conclusion that nearly all spontaneous ESP experiences are not suggestive of ESP, but are satisfactorily explained by a psychological attribution process then we would have made a great step forward. That would imply that we found the explanation for the main phenomenon we study, spontaneous ESP. However, from what I have learned about the subject it is by no means certain that psychological explanations suffice. In that case experimental research will certainly benefit from a better understanding of the relationship between psychological conditions and the occurrence of ESP experiences.

When the experimental research in parapsychology of the last decades is considered it is apparent that relatively much research is either of the RNG-PK or of the ganzfeld type. Together these two research approaches may well take up more than half of the research effort in the field. This dominance might have turned attention away from other approaches which could also be promising. In this respect I can mention that the 1968 PF Conference discussed possibilities of psychophysiological and animal research. There has been research in these areas but very little compared to the possibilities these approaches offer.

The third area from which we might strongly benefit is a more careful analysis of why certain experiments or approaches are successful. Meta-analysis as discussed by Honorton at this Conference is an important step in this direction. Another suggestion is to study more extensively which subjects contributed to these successes. It might well be that many series of successful studies from the same group of investigators are based on the contributions of only a few subjects. It is important to find this out because if that turns out to be the case it might well be that we had better depart from the Rhinean approach of working with unselected subjects.

Another direction which might lead to improved results is the more careful study of variables which play an important and perhaps vital role in the experimental procedures we apply. In free-response studies which constitute the bulk of ESP research there are important variables, as for instance the judging procedure or the statistical assessment, for which we still lack optimal procedures. We can not properly estimate the effect of an independent variable in a free-response study if, for instance, the judging or the statistical evaluation is also influenced by the conditions.

We do know that subjects sometimes differ strongly in both the direction of scoring or type of effect in ESP or PK research. Jahn and Dunne (1988) speak of a sort of personal imprint, a strong correlation between specific patterns of results and individual operators (p.144). If that is the case, we might improve the efficiency of our research by applying a different operationalization and statistical evaluation for each individual subject, instead of lumping the scores of all subjects together. This technique seems especially suited for process-oriented research. The principle of the technique is to include in each experiment one or two calibration conditions. Suppose two conditions are to be compared in an experiment. If the two conditions are not mutually exclusive, the calibration condition is chosen in such a way that it includes both the two conditions. For example, if in an RNG-PK experiment the OT model is compared with the IDS model, the calibration condition could be made up of a pre-recorded true RNG sequence and the subject can choose the entry point in this sequence. The result is displayed to the subject. Hence both OT and IDS apply to the calibration condition. For each individual subject the calibration condition can be used to establish the most extreme chance expectation deviating operationalization (number of 1 or 0, variance, runs, decline, etc.). This operationalization is then applied to the experimental conditions. The results of the experimental conditions can then be rank-ordered based on that operationalization according to the extent they deviate from chance.

If one of the conditions receive systematically higher rank-order numbers that might indicate the superiority of that condition. With mutually exclusive conditions a similar procedure can be followed. The above is only a rough indication, most experiments will lend themselves to more refined applications of the principle. Another possibility is that with such procedures it also becomes possible to use an external criterion to discard subjects from the database, so that the final evaluation can be based only on subjects who might have had some ESP effect on the data.

No doubt others will have more valuable suggestions for improvement of research methodology. However, considering the amount of time research takes it is to be feared that most of these suggestions won't be followed. It is the lack of research opportunities in the field, the lack of money and positions, which put the greatest constraint on our progress. The development in psychology shows that abundant resources are no guarantee of success, but without resources faster progress can hardly be expected. I hope that in this respect the future will have more promise than the present.

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## *DISCUSSION*

HONORTON: Well, I want to congratulate you, Sybo, I think this is a very important point that you are making. It is one that I have made myself over the years, particularly with regard to the issue of replicability. We have to look at our own accomplishments in relation to what is going on in other areas rather than looking at parapsychology as though it existed in a vacuum. And when we do that we see that, although we certainly are not doing as well as we want to, we are doing much better than some of the more pessimistic assessments have suggested. So I think that is a very important thing for us to keep in mind. I also think that in terms of accumulation, while I agree with you in general, we are still basically in a collecting phase. There is very clear evidence that there is some cumulativeness that is contrary to what some of the critics, like Hyman for example, say. New researchers who come into the field are not constantly reinventing the wheel. Mostly what they are reinventing is the terminology, so that they do not have to be identified with the excess baggage associated with that. But very much so there is the ability to build on previous research and that is the foundation of the idea of cumulativeness.

SCHOUTEN: Well, I agree there is some accumulation. It is not a black and white scheme. But what I meant by accumulative is that a certain field of science reaches a theoretical basis including laws and relationships which are predictable. These then expound new theories which engulf the previous ones. So you know there is progress, in such a science there is no question at all about it. If you look at a journal volume of 30 years ago, they have a different level of knowledge compared to now, a much more restricted level. I think that is at present not so much the case in psychology and parapsychology.

MAY: Sybo, I also want to add my congratulations. I thought it was an excellent talk. But we physicists have done a number on you and I think we parapsychologists can learn from how we have done that number on you. It is actually a scientific myth that science builds upon; we are told in school that it builds upon this pyramid. There was an excellent series on public broadcasting a while back in this country by John Burke. He made the point over and over again that we believe that all of science has been aimed at the moving present but then if you examine the history of science you can't support that. I got out of the field of nuclear physics research in the early 70s and from time to time I wander over to the library, pick up the most current journal, the one I used to publish in, and discover that I have been away a week. They are still doing the same stuff. But I think what that is is a characteristic of how one gains knowledge in general. You go through periods of plateaus. The period from 1895 through the next 35 years has been known world-wide as the 30 years that shook physics, because at that time there was exponential growth. Another but related comment that I wanted to make is that there are laws of numbers that are derived simply from calculus that state that the rate of change of knowledge depends upon the number of people investigating it. And if you look at psychology and parapsychology where that appears not to be true, all that means is that we are still in the flat part of the curve and eventually we are going to take off.

SCHOUTEN: Yes, I agree with that. I think that is true. In fact you might turn it around and say, well, let us not get too many people in parapsychology because it would not help much. That is not true either. But it is surprising to me that, if one looks in the psychological literature and you see those really big fields, how many people are researching clinical psychology and how little comes out of it in the sense of real solid knowledge you can apply. It is very, very disappointing.

MAY: I did not add what I thought parapsychologists can learn from physicists is that we have got good public relations. We have convinced everybody we are making that sort of progress and I think parapsychologists can use a little of that.

SCHOUTEN: A question which I raised for myself was how is it possible that clinical psychology, working a 100 years and with an investment of billions per year and not yielding any solid result, is still supported vitally? And why is it possible that parapsychology, a small field, is not supported? I think that is a real issue. I think parapsychology is looking in entirely the wrong direction when it looks for answers. I think the reason partly is that clinical psychology, although it is not making progress, is dealing with things that are alive in society. If we go into research which deals with people having problems with psychic experiences—and not everybody would like to do that (personally, me neither)—I think that the moment you establish institutes for that and do research, you show that here is a service we present to society, I am convinced you get money. Same about healing.

MAY: In other words, we should be paying more attention to survival before bodily death.

SCHOUTEN: Certainly.

MORRIS: I would like to follow up very specifically on one of your points about the way of measuring progress namely the potential contribution of parapsychology to the real world problems of people. A certain kind of progress in parapsychology may be what we might within some frames of reference define as negative progress i.e., helping people understand more of what is not psychic, but looks like it, ways in which people may be mislead. In the last six months you have had your final two doctoral candidates graduate at Utrecht, both dealing with groups of practitioners in society. They did theses which on the one hand did not find particular evidence for psychic functioning, but on the other hand provided a fair amount of specific information about what else may be going on there. Within your own criteria this would be regarded as a service, as one of the positive contributions of parapsychology and I would agree with that. My question to you though is can you reflect for us what impact those two studies appear to be having these days in Holland?

SCHOUTEN: Well, that depends on whether we follow it up or not. I think that depends on what is done with it. In itself, carrying out a study would not bring much money. It is service you provide. I mean it is a fact that so many people need clinical psychology or think they need clinical psychology, that is what brings the money in. Now in our case in Holland at least we established an institute for counseling. It takes some time, of course, but it turns out that works rather well; there is quite an interest. This institute was supervised by us at the university. There is a real need. Psychologists and other service providing organizations are sending people over, because where else do they have to go? The usual situation is that they cannot go anywhere, there are only cranks. Now there is this institute. When the situation is that there is a known procedure, that people are referred to this bureau and so on and if the benefits of it are recognized, it is also very easy to send in proposals and get support for it.

STANFORD: Well I certainly concur with most of the comments that have been made about the value of your paper, Sybo. There was one thing in particular that concerned me a bit. I certainly agree that in some respects, perhaps especially in our textbooks, psychology has been vastly over-sold. But at the same time you are advocating that we maybe do a little bit of over-sell or try a little bit harder to sell parapsychology. So I think we probably need in some respects to sell all of these areas. But I do feel that you may have done a bit of an injustice in one area of psychology. I know this is a parapsychology conference, but when I feel that there has been an injustice done I feel that I have to comment on it. I know a fair amount about the attitude research area. There were some very serious problems there up until the late 60s, when it began to be recognized that the problems existed. And many remedies have been found for these problems. We know now very concretely about the kinds of things that moderate the attitude behavior correlation. We also know that many of the problems of studying the relationship of attitudes to behavior were due to several methodological problems in the way attitudes and especially behavior were measured. We really do have some very good progress in that area. In fact, I would say that what comes out of it has some relevance to parapsychology and can encourage us as well. This is that those doing attitude research had really failed to look at and empirically examine some of their fundamental assumptions about what they were doing. Once they did so and started to do research in that framework, they began to make some meaningful progress in that area and attitude-behavior research has come alive again and, in my opinion, very justifiably so. I think that message applies to parapsychology. I think it is one of the reasons we have for enthusiasm and optimism today. We have started as never before to question the underlying assumptions of our methodology. I do not know of any area of science where that has happened that it did not ultimately lead to some degree of progress. I think we can fully expect that here.

SCHOUTEN: I am glad you are optimistic about attitude research and have more or less a wait-and-see attitude. I am impressed by the techniques developed for it, scaling techniques and so on. I think they did a very good job there. Whether it really will work in the sense that you can predict and measure, that remains to be seen, but it is not a black and white thing. I know you can take polls and make fairly accurate predictions in some areas.

BRAUD: Whenever I hear comments about our lack of knowledge or lack of advancing or accumulating knowledge I am reminded of an analogy that I will share with you. Consider the physics of trajectories. A very young child is able to throw and catch a ball or a stone with tremendous accuracy. That child has a knowledge of the physics of trajectories. It has taken physics literally centuries to encapsulate that knowledge in formulas so that this knowledge can be communicated to other people. There is a kind of informal or tacit knowledge that we can acquire through our own experience very early. Then there are the more formal quantitative aspects that take quite a while to develop. I think that in the fields of astronomy—because the heavens were there for our inspection very early—or in psychology or in parapsychology the subject matter is very familiar to us. I think we learned a great deal very early and that knowledge is so familiar to us that we hardly consider it to be knowledge. We consider it too common and we forget it. Perhaps the curve describing knowledge in parapsychology and in psychology is logarithmic rather than exponential. Perhaps it showed an early acceleration and it is now leveling off, and we are learning some of the more subtle things that were inaccessible before. I think that is a much more optimistic way to view the concepts, the things that we possess. In terms of methods and theories, I agree that we are perhaps on an exponential curve or a linear curve.

SCHOUTEN: Well, I certainly do not disagree. I think the knowledge people personally have is different from the knowledge that concerns what we call science. But I am not talking about that. I mean I am not talking about personal knowledge, I am talking about science. If what you say is true, it applies to psychology and parapsychology too, but unfortunately it does not help either of those fields.

BRAUD: The point is we do a lot of predicting of human behavior. We base our lives upon accurate predictions. Those predictions are based on personal knowledge that has not yet been systematized. It is just so familiar and common that we do not consider it worthy of the name of science.

SCHOUTEN: I am not going to fight about words.

PALMER: It seems to me that when we are trying to assess how much progress we have made in parapsychology we need to be very much aware that we have given ourselves a very big challenge. Psi is a very difficult nut to crack, and I think the reason is because it is closely linked to very complex mental or psychological processes. This is the same problem that afflicts the softer areas of psychology, and they are making about the same rate of progress as we are. To tease that all apart boils down to a trial-and-error process, which is what we have been doing. And that, simply by the nature of the beast, takes time. So in a way it is unfair to compare progress in our field and in the soft areas of psychology to a field such as chemistry or certain areas of physics, where the problem is much less complicated to begin with. In some ways maybe we are too hung up on the question of whether we are making progress. Maybe we are making as much progress as we should expect given the great complexity of our subject matter. We are going to need to keep the faith and let the process run its course. I believe this progress will be exponential. We are on the lower part of the curve right now, and that is where we should expect to be.

SCHOUTEN: An important aspect is how hard the task is that you are dealing with. I would like to argue that parapsychology is one of the hardest fields to do research in. If you deal with physical processes at least in the beginning you start with processes which are repeatable in nature. That is a real blessing. The sun rises each day. When you deal with psychology you deal with much more complex phenomena because so many variables are active at the same time, you can't control them all. But if I carry out a study in learning or perception at least I know that my subjects are learning and I know that my subjects are perceiving. All I have to do is to think of a clever experiment and I can do it and publish it. But the bad thing is in parapsychology I do not even know that. I do not know whether there is ESP in what my subjects do. I can design a clever experiment and it ends up in the wastebasket because just nothing happened. So I think parapsychology is one of the most difficult fields to deal with and that should also be taken into account. Also I feel that we are very strongly dependent on the progress in psychology, for instance. And my personal feeling is that we will not make the sort of progress we would like to make unless psychology itself is progressing faster. It might be that in the end ESP turns out to be a physical process, certainly not the kind of thing I would like to exclude, but for the time being I do not know. For the time being, it is clear that the human factor plays an important role in the whole thing.

MORRIS: This is really addressing itself both to these issues and to a theme that I think has run through much of the conference. We are studying very complex, open systems. Much of what has been said has really been about expanding our definition of what constitutes the system of the experiment to one which takes into account a whole host of variables attending an experimental situation. Part of what we have also touched on is that you can take each such system even though it is open and embed it within a larger system. Each researcher in some sense is his own system. So is each lab, the parapsychological community, society as a whole. These are all dynamic systems which are greatly affected by feedback into the system as a result of its own activities. And I think it is analogous to the problems that economic forecasters have in that if they do their business they must make a statement of some sort, an announcement of some knowledge or guidance. Once that guidance and knowledge is taken into account by individuals active in the system that economic system changes and has different properties than the system upon which the original prediction was based. If we regard ourselves as trying to be socially useful and we interact with institutions of the sort that are likely to acknowledge progress and foster more, we will find ourselves in an extremely complex dynamic situation that is very hard to anticipate.

# AFTERNOON GENERAL DISCUSSION
## DAY TWO

MAY: I want to return to a really pragmatic situation. We are worrying about methodologies and all these complex issues, yet maybe this is not correct. It seems to me we are ignoring, not beyond just lip service, two very important methodological issues that address any kind of process-oriented research you would like to try to do. One that I myself have looked over, that Chuck pointed out earlier today, is that if it turns out to be true that in free-response experiments there is a great deal of target dependencies and you do not acknowledge that in your research, you have to solve that problem first; because, if that is true, you will have chaos in terms of understanding any of the other process-oriented research. The second one, which is one of my favorite themes, is that you can do experiments in what we will call a precognitive methodology and, if that is true and I certainly believe that it is, again you have a problem of method, of process-oriented research. You have all these complex conditions that you are doing experiments upon. If you really are simply by-passing all that by some precognitive mechanism, you have another difficulty on your hands in interpreting the process-orientation aspect of your experiment. And these are two fundamental methodological issues that I believe have to be solved if we are going to make some really significant progress in process-oriented research.

HARTWELL: It can not be denied that we are making progress. Never before have we used such space-age microphones, clear mark of progress. Sybo, I was grateful for your breaking the field's history into the three sections that you did. It seems to me that we did just as you described. We began long ago casting about and looking out there in a very phenomenological way. Then came the period we credit the Rhines with, where we tried to frame hypotheses very tightly and that was done by-and-large in the context of forced-choice experiments. Then we went to free-response experiments where the goal was in the main to try to cast a broader net, not to press things into such a tight box. It seems to me that the real progress that we have made there was brought forth in Dr. Utts' paper yesterday and in the discussion which followed. The common theme of the questions addressed her was "could I answer the following?" and her response was always "yes, if you construct your bit list in that way." And so the nexus is in bringing together the broad net that one wishes to have (the more life-like situation in which we wish to place the subject), and the scientific need to tightly frame a hypothesis. The idiom in which we couch that today is "how you construct your bit list." Ed May came right back saying these were the parameters that guided us in constructing the bit list for a particular experiment. And it seems to me that that really does constitute progress. There is some way in which the present state we are at does try to bring the best from our historical phases and see us approach scientifically the sort of real world situation that gave birth to it. So I thought Dr. Utts' contribution was excellent and summarized exactly where we are at in some progressive way.

HONORTON: Most of the methodology that we are using today did not exist in 1968 when the Parapsychology Foundation had its earlier conference in methodology. The free-response work was just getting started, the random number generator work really had not quite yet gotten started. I do not think anyone 20 years ago forecast the extent to which computers would be everywhere, cheap, easy to access and would provide ways of controlling certain aspects of experiments that had not been possible before that time, in addition to doing analyses and looking at pattern recognition possibilities and so on. If you look at the books on parapsychology up through the mid-60s, let us say, you can take a book in the early 60s, a book in the 50s and the 40s and the 30s and you could pretty much interchange them. There really was not much methodological movement at all going on. That is no longer true. I am in the process now of reviewing the Edge, et al. book which was published three or four years ago, finally getting reviewed in JASPR, and that book in my opinion is badly out of date in a number of respects. The meta-analytic perspective had not yet come into its own, there were lots of changes that had occurred in that short period. I would like to reinforce an aspect of Schouten's paper that did not get mentioned orally and that has to do with the importance of bringing spontaneous cases back into the picture a little bit more directly. After all the free-response research had as its primary impetus the idea of developing a more naturalistic experimental approach. One that was more compatible to the way in which the phenomenon seemed to occur in everyday situations. And I suspect that the more we can model experiments on trends that we can find in the collections of spontaneous cases, the more progress we are likely to make. The spontaneous case material I suspect is likely to provide better predictors of laboratory success than many of the other kinds of predictors that we have used up until now.

ROLL: There is something that we have not covered here and that is the suggestion made a good while ago by Charley Tart that the state-specific approach should be part of parapsychological methodology. That is, the researchers should be able to enter the parapsychological realm in a personal, immediate way as a means of gaining insight into the processes that they are exploring in their experiments. I feel that some respect should be paid to that idea and that is it something that it is still worthwhile talking about in a conference dealing with methodology.

BRAUD: Let's open the issue even wider and rather than saying state-specific just extend that to subjective. We can learn a great deal from our own subjective experiences, whether or not we enter altered states. And that was really the thrust of my analogy, that we probably are carrying around in us right now a great deal of tacit knowledge that we are not sharing with others or perhaps only privately. If we look inside, I think we will find a lot of lawfulness and a lot of useful information that is being ignored. At the Mind Science Foundation, we typically participate in our own experiments as subjects, influencers, or agents to get a subjective feeling for what is happening. Those experiences drive our research. Well, yes, I think it is a very good idea. In its loose form, we can do that today; in the more strict form that Charley suggested I think that might be difficult. I am always wrestling with the idea of how to verify things that are not immediately evident to the senses. A lot of parapsychology has to do with that. If it is sensory information, that is rather easy to verify, but in entering this state several researchers come up with content. How do they put that content into words once they have left the state and how do they communicate that to those who are interested only in sensory experiences?

RAO: Yes, I think we have made progress. The proceedings of this conference is a clear testimony of the many-faceted progress that the field has registered in the last several years. This is something that we all can be very proud of. But at the same time I do not think it helps very much, except perhaps it gives us some additional motivation, to say that psychology has not made any better progress, that we are doing better than psychology with fewer people. It is probably comforting to think we are doing better. Perhaps we are. But we should not ignore the fact that today we are too few. The funds that are available for our research are too meager. As Ed said the question is survival. So it is not going to help just to say that we are doing better than psychology. It is not going to help us to say that our effect is comparable to at least some meager effects in some soft areas of psychology. The fact of the matter is psychology is flourishing. There are more people studying psychology, there are more funds psychologists are able to attract and parapsychology is not making commensurate progress in terms of attracting people, attracting funds in proportion to the practical and philosophical significance of the phenomena we are studying and relative to the high standards of our research. So I think we must ask "Why?" Why is it that after 60 years of continuous experimental inquiry into the field, we are still struggling to survive? I hope the discussions that we have had on methodology here during the last two days have given us some insights into the new directions we must take, new approaches we must make so that whatever success we have achieved would meet with necessary encouragement and necessary support. Without that I think the field is going to be in very bad shape.

HONORTON: I agree with you, Ram, but I think there is one very important sense in which this comparison is more than just morale boosting for us. That is that there has been a very strong tendency for a very long time for people to suggest that the slow progress in parapsychology was due to the intractability of our subject matter. And it is not.

STANFORD: I would like to address two topics briefly. One of them relates back I think to the question that Bill Roll asked. I am not really going to comment on state-dependent science, which I do not think of as a conceptually viable endeavor for reasons that I will not go into here. But I have a feeling that the individuals who are having considerable success as investigators in psi experiments are persons who have personally experienced these phenomena. I can't prove that. I do know, though, that many years ago Charley Tart and Burke Smith did a survey of parapsychologists asking their opinions about the extent to which they had seemingly been able to elicit the phenomena. They found a definite correlation between people's reports that they had had personal experiences and whether they had been able to elicit the phenomena in their own experiments. I have not seen any evidence that that situation is any different nowadays. I frankly believe that some of our most successful investigators are those who have had a number of personal experiences that may be psi; obviously we can't evaluate an individual experience. William Braud was talking about implicit kinds of knowledge that the investigator might have. I quite agree with that. One way we might boost the yield in this field is if individuals who have not been having success in getting psi could find a way to open themselves up to have some experiences themselves. It might bring some yield. I do not know if this is plausible or not. I mentioned this on a number of occasions, but it is something we might want to think a bit more about. Finally, I think it was Sybo who said we really need to make some advances in certain areas of psychology that can help us in parapsychology. I alluded to some of those in my talk and more explicitly in my paper. Now there are some glowing opportunities for parapsychologists to make contributions to psychology. There are various reasons to do this. One is because we need those substantive contributions. We need those methodological contributions to benefit our own research. But I would suggest that there is another reason we need those things. It is for public relations within the scientific community. I know from experience with my colleagues in psychology who have no special interest in parapsychology that they tend to be open to what you have to say, to respect you and listen to you if they know that you are able to deal in the realm with which they are familiar and that you can make contributions and work there. They can no longer push you off as someone who is a bit too strange to consider seriously. So there are solid reasons why we need to get to work and make some contributions outside of parapsychology. I believe we can do it at the same time as we are doing our psi experiments in many, many instances.

EDGE: It would be interesting to hear from sociologists now.

MCCLENON: I am in a strange position because I have attended a lot of these conferences here in America, I watch parapsychologists. I have also seen psychical researchers at conferences in Taiwan and in Japan and in the People's Republic of China gather together and talk to each other. People from different cultures seem to be on different wavelengths. The people in Asia seem to be, to a degree, still in the observational stage. But at the same time they use computers and videotapes and they perceive of themselves as making a lot of progress. It would seem that they *are* making progress because, they getting very, very robust results and they perceive themselves as progressing. They appear different from us in that they have a greater perception of the importance of social networks and the importance of belief. William Braud noted this is something we know we *should* do, but they are seemingly better at doing it. Our response to this is that they are naive and their subjects are fooling them. But ironically the skeptics say the same thing about us in many cases. So it seems to me that maybe we are congratulating ourselves a bit too much. There does appear to be progress, but it seems that the methodology itself restricts the theorizing. I wonder if some of the panel might address that question.

MAY: In a sense that is partially what I wanted to say a moment ago.

I think it is a testimony to the enriching aspect of our discipline that there is room for psychologists and engineers and physicists and medical people and our attention. This is addressed to something that Bill was talking about a little earlier. I am frankly not interested from the nose inward. That is too hard a problem for me to solve. I have to leave it to you guys to solve that one. There is a lot that can be done once you have a stable receiver of psi (and fortunately we are lucky enough at SRI to have that) to begin asking about what aspect of nature in the physics realm allows information to get from point A to point B. So here is a case where the methodologies you have been hearing about primarily are not restrictive in terms of the physics modeling that might go on. So I do not think we should restrict ourselves only to the psychological methodologies or the psychological models. They are important to do, no question, but I think the field has grown to the point to where there is more room for physics and more room for physiology, more room for biology and the more traditional, harder kinds of sciences which are really easier.

MORRIS: Well, this really follows up on both points. I think one of the main things I heard you say there, Jim, was that it would be instructive to look at some of the possibilities of things that they may be doing a bit better than we are. A focus in part was on some of the social network aspects, some of the ways of really sustaining and maintaining enthusiasm. I think this goes along with having us reflect more on just what does go on within the system of our own labs, of our own research endeavors. At Edinburgh part of what we are trying to do is to survey some of the different labs to try to pull from people some of the deeper lore of what goes on in things such as target selection and target usage, free-response judging procedures and so on, and yet even that is pretty shallow. We intend to study psi training procedures and are ourselves subjects in some of the early informal work. We do try to pay much more attention to ourselves as system components and get ourselves to the point at which we are more inclined to regard ourselves as active participants in the subject matter we are dealing with. But that is going to be hard.

PALMER: This is pure speculation, but I have a feeling that somewhere down the line we are going to come up with an idea, a concept, a theory, whatever you want to call it, that is going to be radically different from anything we have now and is going to lead to a breakthrough that will make the current levels of effect-size and replicability seem rather trivial. The point I want to make is that I think it is very important that we be open to new ways, perhaps even radically new ways, of looking at our phenomena, provided that these can be logically developed and articulated and that they are testable. I am coming increasingly to believe that the observational theories are wrong, but I think it is very good they exist, because they have allowed us to look at our phenomena in new ways. I think Rex Stanford's Conformance Behavior Model has served a very similar function. And as I said in my talk, I think taking experimenter psi seriously is another example. I also believe that systematic development in areas that look promising now is important and should continue; that is really the bedrock of our science. But at the same time we should not allow ourselves to get into a conceptual rut. There may still be some very exciting avenues out there that we do not know about at this time, and we need to be open to them.

ROLL: In relation to some of the remarks made by several people here, including Sybo, and connecting them with the concept of the significance of the goal-directed aspects of psi, I feel that one realm of methodology is in the realm of applied parapsychology. Improving the condition of clients, society, somebody in disease who is having problems, that general purpose pulls you ahead and enriches your work, enriches your attitude and in this case would enrich the field. So what I would like to see is further consideration about the possibility not only of psychic healing, but also the realm of counseling parapsychology or clinical parapsychology or whatever we call it. This is an interdisciplinary realm, this is a realm where our skills, our knowledge have to meld with the fields of medicine, psychology, psychiatry, neuroscience in particular. It is a field where we might do some good, make some significant discoveries and help to place our subject matter firmly in this world.

## CLOSING REMARKS

HOYT EDGE: I would be remiss if I did not begin these concluding remarks with a note of appreciation to the Colys for all they have done in setting up this conference. My toast to you last night during the reception was more detailed, but to make sure our thanks becomes an official part of the Proceedings, let me once again express the appreciation of all the participants, as well as the observers.

Friday afternoon I had a tour of the building of the Institute for Parapsychology, and we visited the attic. Among all of the boxes of data from experiments over the many years, there was a collection of old Cox macro-PK machines. These were marvelous and unique creations. Also, there was an apparatus built for an automated gerbil experiment. It looked like a miniature ferris wheel that the gerbils rode, cycling around until it came time for them to participate in the experiment. I came away from the attic, not only with a fuller sense of history but in relationship to the theme of this conference, with the conviction that psi methodology has changed.

There is no doubt that over the last twenty years, we have become much more sophisticated technologically in our experimentation. However, Ed May has pointed out to us today that technology is a mixed blessing. Sophisticated technology allows us to explore more sophisticated systems in psi research; computers allow greater ease and complexity in statistical analysis, as well as automated control of the experiments. On the other hand, Ed warned us that we face the danger of using technology without fully understanding its limitations, especially when we use "off the shelf" equipment. As Ed pointed out, technology is not always the answer in psi research.

However, when we get down to it, the most sophisticated instrument that we use or can use in psi experiments is not a product of technology at all; rather, it is the human organism. William Braud gave us good reason to pursue research in the distant mental influence on living systems, including, among others, the effect on autonomic nervous system activity. His research has shown that the effects of distant mental influence are relatively reliable and robust. The implications of this research are far reaching, especially since the effects are able to be specifically directed on particular aspects of the living systems, and the influence is bi-directional.

As a parenthesis, let me say that I have been impressed with William's research for a long time, because I feel it is an example of the development of a specific research program. It may be that we do not have an over-arching explanation for psi, but we do have sufficient conceptualization to drive sophisticated, progressive and successful research programs. To me, this is an indication of our maturing as a science.

My further concluding remarks should not be taken as a summary of the Proceedings. I would fail to capture the subtlety of papers and have to leave out a wealth of material if I attempted such a summary. Rather, I plan to focus on those areas where ideas from papers seem to cluster. There was convergence of ideas and emphases in the various papers, and it is these idea clusters which I would like to summarize. Let me list six of these ideas, or idea clusters, and briefly talk about them.

*Idea 1. We can learn things from other fields.*

Victor Adamenko suggested that we may be able to understand precognition better by using conceptualizations from the Russian physicist Kozyrev. My physics is insufficient to understand everything that Victor talked about, but he argued that Kozyrev's notion that time has physical properties can become a way that we can bring our understanding of precognition into physics. He talked about an experiment done by Kozyrev in astrophysics which displayed these principles.

In the past, Chuck Honorton has taken meta-analysis and applied it to Ganzfeld research. In the paper at this conference, he summarized his use of meta-analysis on published precognition studies. I will refer a bit later to this analysis, but at this point, it is important to see that Chuck was able to take a methodology employed in other sciences and apply it creatively and helpfully in parapsychology.

Rex Stanford argued that a major reason we have not made more progress in some areas of Internal Attention States research, especially hypnosis research, is that psychologists know so little about what is really happening when a person is e.g. hypnotized. Without an advance in understanding Internal Attention States in psychology, parapsychology will probably not be able to advance much farther in our own internal attention research. The lack of progress in other areas of science, in other words, bodes ill for parapsychology. Yet, Stanford pointed out that there is material in psychology from which we can learn. For instance, there is good data on the phenomenon of self-handicapping, when someone engages in behavior which secures less than optimal performance. Self-handicapping may be displayed in parapsychology, for instance, by people who fear the implications of having psi ability. Rex urged us to learn about the problem of self-handicapping and how the literature in psychology tells us how it can be mediated.

*Idea 2: Other fields may learn a few things about methodology from parapsychology.*

Although we are heavily dependent on research in other fields of science for our methodology, it may be that progress we have already made or may make in the future could benefit other fields. For instance, Jessica Utts has made good progress in proposing statistical techniques that could help solve problems in other areas of science. These statistics have focused on the problems involved in judging free response material in parapsychology. Two traditional techniques have been used, atomistic analysis and holistic analysis. The problem traditionally involved in atomistic analysis is that there are many features of targets which cannot be categorized on the atomistic list used to evaluate the targets. Further, gestalt features of a response are often more informative that atomistic features. To solve these problems, Jessica has used the statistics of fuzzy sets, which incorporate vague information into the analyses. The main problem involved in holistic analysis, on the other hand, stems from the use of decoys. Often they are too close to the target and therefore a judge cannot discriminate the target picture from the decoy. In order to assure that there is a qualitative difference among the targets in the pool, Jessica has used cluster analysis to separate all of the potential targets into distinct groups. Once a target is randomly chosen, decoys from other groups are then chosen as decoys to insure the qualitative distinctness of the pictures in the judging pool.

Bob Morris pointed out that parapsychologists are imprecise in describing the volitional activities of subjects. One reason for this is that the concept of volition is theoretically vague and some contemporary philosophers even argue that volition is an unuseful notion. Yet it seems fundamental to psi research. Parapsychologists tend to assume that volition is present in the experiment but often we don't focus on it, and we don't realize all of the ways in which volition can be present in the experimental situation. Along with Richard Broughton, Bob urges us to pay more attention to the notion of volition. Furthermore, he suggests that if we can understand better the role of volition in the performance of psi tasks, we may contribute to the general psychological literature in this area, especially since performance psychology—such as sports psychology—takes volition to be central.

*Idea 3: Results in parapsychology have been more robust than we may have thought.*

Chuck Honorton's paper on the meta-analysis of precognition showed us that the overall results are consistent and robust. The effect size across the precognition experiments is as good as in many areas of psychology and in medical testing of drugs. It seems to me that Chuck's work in meta-analysis should put to rest once and for all the critic's objection that there is no repeatability in parapsychology. The rate of replication may not be as high as in the physical sciences, but it appears to be as good as in most areas of psychology.

Although parapsychology has not made the progress that it had hoped—in that we have not answered all of the questions that we wanted to, nor have we moved as quickly as we had hoped to—Sybo Schouten pointed out that progress is better measured by looking at other areas of science dealing with human nature and comparing our progress with theirs. His analysis showed that they are also slow in development, and when you take into account the relatively small levels of funding and the minimal numbers of researchers in parapsychology compared to psychology, our relative progress should be considered on a par with psychology's. Schouten mentioned three additional criteria that we can use in measuring our progress. The first is that we have progressed by rejecting some old ideas. The second measure of progress is whether we have changed opinions in society about our field. We could use more progress in this area, but at least there has been some change. The third additional criterion of progress is that science moves from a collecting phase, one in which data are merely collected, to a cumulative stage, where data is not merely collected but where one piece of data builds on the next. Although parapsychology is still a collecting science rather than a cumulative science, in general the same can be said for psychology.

*Idea 4: Parapsychology works in a system.*

This idea is admittedly vague, but what I have in mind is the general idea that our phenomena are understandable only in terms of the relationship within some sort of system. For instance, John Palmer discussed the experimenter effect and urged us not to ignore this effect not give up on our understanding it; rather, we should attempt to test it directly. In the past, insofar as we have attempted to get at the problem, we have wanted to test whether or nor it is the experimenter or the subject who has contributed to psi. Palmer suggested we should expect both experimenter *and* subject to contribute, although one may contribute more in a particular experimental condition. The aim of our experimentation should be to see how much is contributed by each rather than to attempt to eliminate the contribution of one or the other.

Based upon a psychoanalytic interpretation of self, Bill Roll employed the concept of a "larger self," as well as the idea of a "long body." Employing some philosophical arguments and using the data of spontaneous cases, Roll argued that we ought to understand ourselves as emplaced as well as embodied, not only in the material body but in the environment with which we come into contact. Roll suggested that if we engage this "long body," which is a system incorporating both participant and agent in ESP tests, we should get more robust results.

Ramakrishna Rao recommended that we employ the "fusion" hypothesis rather than the "exclusion" hypothesis. Traditionally, parapsychologists have been concerned with separating psi from all other modalities, thinking that if the involvement of sensory modalities in the experiment has been eliminated, what is left would be psi. Rao pointed out that psi may work *in tandem with* sensory modalities, and research, at least in its initial phases, might be more successful if we employed conditions which allowed sensory modalities to work also. Then we might be able to find out how much psi enhances the reception of information.

Finally, Richard Broughton suggested that in serving the needs of persons, psi may blend in seamlessly with normal activities.

*Idea 5: We should pay more attention to individual differences.*

In his paper on Internal Attention States, Rex Stanford pointed out that individual differences are especially important. If we understand how internal attention states work, we can take advantage of these individual differences in our psi tests. Sybo Schouten further argued that unless we take into account individual differences, our experimentation with unselected subjects may not be productive.

As an aside, I want to mention here that in general, although there were some dissenters, the participants seemed to be in a good deal of agreement in recommending that we work with selected subjects. Even some who have not worked with selected subjects became more convinced that they should do so, at least until we know more about the effect of individual differences on psi testing.

*Idea 6: We need to go back and ask for fundamental definitions.*

At the Parapsychological Association Convention in Montreal, the Edinburgh group gave several papers showing their interest in defining more precisely some basic terms in parapsychology, such as what a target is. Bob Morris presented a paper here in which he was concerned with understanding the notion of volition better. Indeed, there has been a general emphasis—by Broughton, Morris, and Braud, among others—that we need to focus more on needs/volition in the experimental situation. One of the problems in understanding how volition works in psi tasks is that our experimental write-up has been sloppy in describing in any precise way how volition may have played a part in the experiment.

Finally, Richard Broughton suggested that we should go back to the very notion of psi and ask what its purpose is. He suggested that it must be a product of evolutionary development and should therefore have survival value. That value may not be a defensive value, but in some sense it should be need-serving. Perhaps psi serves us in a general need for well-being. If this is so, Richard suggested that we should make sure that psi tasks are challenging, and we should pay more attention to the research environment.

Let me conclude these remarks by making a general statement. This could be a time when parapsychologists are downcast and pessimistic. We are losing the lab in Utrecht; there are problems in Germany; the effects of the negative NRC Report are yet unknown. There are other signs, however, that are much more positive. In the first place, the Parapsychological Association commissioned a response to the NRC Report. This excellent rejoinder, written by Palmer, Honorton, and Utts—three of our panelists—is receiving some public play in the media. The New York Academy of Sciences has asked Chuck to write a report about the controversy. The excellent article in *Brain and Behavioral Sciences* has given a boost to the field. Also, the Office of Technological Assessment held a conference last month in Washington to evaluate psi research. The panel included several parapsychologists and several skeptics, including the two who wrote the parapsychology section of the NRC Report. In this kind of forum, in which one had a chance to ask penetrating questions and respond, parapsychology was able to demonstrate how strong and robust the field is. In sort, we came out smelling like roses.

In keeping with this general trend, I sense a real optimism at this conference. Of course, the optimism is tempered by experience, but nevertheless there is optimism and there is pride.

The conference ended twenty years ago with a great deal of optimism. Although we have not made all the progress that we had hoped in those twenty years, we can conclude this conference by being positive about what we *have* done. Although our attitude is wiser and more realistic, based on the work of these last twenty years, nevertheless we can be optimistic about and proud of our field.

LISETTE COLY: Well, I am aghast to have to follow that very stirring speech, Hoyt, but the Parapsychology Foundation would like to thank you for your participation in this conference. Of course, our panelists certainly deserve our thanks for their stimulating papers and their lively discussions. The observers—and some of you have also traveled a long way to attend this conference—have contributed so much to making these meeting a success. We thank you. The Foundation is particularly grateful to our very good friend and capable moderator, Dr. Hoyt Edge, and I think he deserves a round of applause. A conference with discussion periods such as ours can easily unravel, but he always kept us seemingly effortlessly in order, with a smile, and we are very grateful to him.

I am going to digress for a moment to share with you on behalf of the staff of Parapsychology Foundation and the family of Eileen Garrett our deep appreciation of the warm welcome we have received here in Durham. In a sense it has been a feeling of coming home, despite the fact that we do not visit nearly enough. I hope we shall be able to remedy that in the future. Thank you all.

As we close this conference we would like to leave you with one more reference to a quotation in that conference held 20 years ago which we feel is worth repeating as we continue our various methodological approaches to the questions raised by psi. There was an exchange between Jan Ehrenwald and George Owen in which Jan Ehrenwald said, "I think the experimental design has to be flexible enough to accommodate different qualities of the psi function. We must not try to force it into the straightjacket of our preconceived experimental design." George Owen responded, "I agree heartily with that. We do not want to be like the man who went to South Africa to look for gold and when he came back he said it was a total failure, 'I found very little gold; only a few diamonds.' "

As researchers and academicians we continue our mining operations in parapsychology. I hope that soon we will find both gold and diamonds! Until then, Ladies and Gentlemen, the 37th Annual International Conference of the Parapsychology Foundation is adjourned.