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

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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 sci-

ences. 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. Pro-

fessor 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.