THE TWO FACES OF PSI: PSI REVEALED AND PSI OBSCURED

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> He who wants to have right without wrong, order without disorder, does not understand the principles of heaven and earth. He does not know how things hang together.
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> —Chuang Tzu, *Great and Small*

Introduction

There are those who maintain that psychical research has made little or no real progress since its formal organization in 1882. On the other hand, there are those who argue that considerable progress has been made. Both positions are correct and both are incomplete. From one viewpoint, the past century has witnessed the slow but steady revelation of many of psi's secrets. From another viewpoint, the essential nature of psi remains as obscure as ever. Psi has always shown us two complementary faces and, like the particle and the wave that is light, which face we see depends upon the nature of our observation.

Progress in Replicability

The phenomena of electricity and magnetism were experienced at least as early as the period of the ancient Greeks, when Thales of Miletus (*circa* 600 B.C.) wrote of the power of amber (Gk. *elektron*) to attract light objects when rubbed. However, it was not until the beginning of the 17th century A.D. that the scientific study of these phenomena was begun. This study proceeded through several stages which are characteristic of scientific progress in general. First came the exploration of qualitative principles by such investigators as Gilbert, von Guericke, DuFay, Haukesbee and Franklin. Next came the discovery of quantitative laws of static charge by Priestley, Coulomb and Cavendish, and of current electricity by Galvani, Volta, Oersted, Ampere and Ohm. In the mid-19th century came the

theoretical elaboration and integration of electricity and magnetism by Faraday and, a bit later, the quantitative mathematical formulation of the electromagnetic field by Maxwell. Then came the applications: the electromagnetic waves of Hertz, Lodge and Marconi, and the wizardry of Edison, Tesla and Steinmetz.

It is not unreasonable to suggest that the scientific study of psi may be following a time course similar to the one just described. Psychic phenomena were familiar to the ancients. The scientific study of these phenomena came much later. According to an optimistic assessment, the basic phenomena had been identified by the end of the 19th century, chiefly in the contexts of mesmerism and Spiritualism. By the midpoint of the 20th century, the most important qualitative principles had become familiar, and the new science of parapsychology is now well into the beginning of its quantitative phase, with its own Faraday and its Maxwell just around the corner. One to three centuries from now, parapsychology will have attained a degree of sophistication in data base, theory and applications similar to that enjoyed today by the science of electromagnetism. So, at least, goes the optimistic view.

There are indications that this optimistic view is correct. Psychical researchers are now able to point proudly to several important milestones in the acceptance of their field as a serious and legitimate discipline. Professional societies devoted to psychical research have grown in number and in membership. One such society celebrated its centenary anniversary last year. Research centers now exist in which psychical research is carried out on a full time basis. Several scholarly journals devoted exclusively to psychical research are being published. The three best known of these journals have just published their seventy-sixth, fifty-first and forty-sixth volumes, respectively. Serious monographs devoted to psychical research continue to be published each year, with the quality and level of sophistication of their contents increasing progressively. A major handbook was recently published and a leading scientific publishing house is about to release the fourth in an ongoing series of biennial reviews of recent advances in parapsychological research. International, national and regional parapsychological conferences are held each year. A major professional parapsychological association has become affiliated with America's oldest and largest scientific association. Parapsychological presentations have become commonplace at the annual scientific conferences of physicists, engineers, psychologists, psychiatrists, anthropologists and others. Full credit courses in parapsychology are being offered at an increasing number of leading colleges and universities. It is now

possible to earn master's and doctoral degrees in parapsychology. Government grants have been awarded for research on parapsychological topics. Psychical research has received serious media attention. There have been advances in theory construction and theory testing and in the search for possible practical applications of psychic functioning. Parapsychology's progress has become great enough (and apparently threatening enough) to motivate the recent publication of a number of journals and monographs critical of the field and the recent formation of several organizations devoted to criticism of psychical research.

And then there is the issue of replicability. It is unlikely that progress of the kind mentioned above could have occurred without some measure of replicability of the findings of psychical research. More specifically, and contrary to the claims of parapsychology's detractors, we can point to actual instances of replication success in selected areas of psi research.

Some Assessments of Replicability

Honorton (1975), in a superb review of the early ESP card-guessing experiments, has shown that by 1940, nearly one million experimental trials had been reported under conditions which precluded sensory leakage (GESP designs in which subjects and targets were in different buildings; clairvoyance and precognition designs in which cuing was not possible). Independently significant ($p < .05$) results were obtained in 27 out of 33 such experiments reported between 1934 and 1939. Additionally, Honorton showed that there is no truth to the claim that most of the pro-ESP evidence occurred in the Duke University studies while most independent replications by other investigators were nonconfirmatory. In a survey of all English-language studies providing statistical treatment of the data, 1934–1939 inclusive, 33 of the total of 50 studies were non-Duke experiments. A majority (61 percent) of these non-Duke studies reported significant results and this proportion of positive outcomes did not differ significantly from the success rate for the 17 Duke studies.

Turning to the more recent free-response GESP experiments, reviews are available for the effectiveness of remote viewing and for psychic functioning during conditions of internally directed attention (meditation, hypnosis, relaxation and sensory restriction). Hansen, Schlitz and Tart (1984) tabulated the published studies in which a "remote viewing" design was employed—i.e., studies using a free-

response GESP procedure with geographical locations as targets. They were able to find 28 remote viewing experiments, published between 1975 and 1982. Of these 28 studies, 15 yielded significant evidence of a psi effect, while 13 were "unsuccessful." These same authors were able to locate 18 unpublished remote viewing experiments. Of these 18 studies, eight were successful and ten unsuccessful. Thus, the success of remote viewing does not appear to be due to a reporting bias (vast numbers of unsuccessful experiments going unreported).

Several reviews of the psi-effectiveness of the sensory restriction or "Ganzfeld" technique have been published (Blackmore, 1980; Honorton, 1977, 1978; Hyman, 1982; Sargent, 1981). The most recent and most complete summary of the Ganzfeld database may be found in Honorton (1982). According to Honorton: "Between 1974–1982 a total of 48 ESP ganzfeld studies have been reported by investigators in 14 different laboratories. This work comprises 1782 individual ganzfeld sessions with approximately 1000 subject-receivers. Statistically significant ($p < .05$) overall ESP effects have been reported in 23 of these studies and by investigators in 10 different laboratories. This is a success rate of 48%, compared to the expected chance rate of 5%" (p. 63). This conclusion, of course, applies to published studies. Blackmore (1980) was able to locate 19 Ganzfeld studies which had been completed, but remained unpublished as of 1980. Of these 19 unpublished studies, seven (37 percent) were described as having significant outcomes, indicating (as in the case of the remote viewing work) that selective reporting is not a major contributor to the apparent success of the Ganzfeld procedure.

The replication rates for the psi-effectiveness of three additional internal attention conditions have been reported by Honorton (1977). Of 16 studies involving psi tasks during or following *meditation,* nine yielded independently significant evidence for psi. Of 42 studies of psi following *hypnotic induction,* 22 were significant. Of 13 experimental studies of psi during *induced relaxation,* ten were statistically significant. Results for a final internal attention, *nocturnal dreaming,* have been summarized by Van de Castle (1977). At the time of his review, 16 formal dream telepathy studies had been published by the Maimonides investigators and by dream researchers elsewhere. Of these 16 studies, nine yielded significant evidence for psi, while seven were unsuccessful.

May, Humphrey and Hubbard (1980) have provided the most complete tabulation of experiments designed to explore psychokinetic (PK) influences upon electronic random event generators (REGs).

Surveying the PK-REG literature published in the three major U.S. psi periodicals for the period of 1970 through 1979, these authors were able to find 48 papers reporting the results of 214 individual experiments. For 74 of those experiments, statistically significant results were claimed.

Marilyn Schlitz and I have just completed a survey of "bio-PK" experiments. These are experiments in which a person attempts to psychokinetically influence a living target system. We were able to find 149 experiments in which persons attempted to psychically influence nonhuman organic target systems such as plants, bacteria, fungus, cell cultures, protozoa, insects, fish, small animals, etc. Of these 149 experiments, 79 yielded evidence for a significant bio-PK effect.[1] It is recognized that these studies vary greatly in design, procedure, target system, method of analysis, etc. Nonetheless, all are conceptual replications which have tested the hypothesis that persons can mentally and at a distance influence the behavioral or physiological activity of living organisms. Taken as a whole, these bio-PK studies have been quite successful and indicate a promising area for future psi research.

A final area which should be mentioned is research on the correlation between psi scoring and the absence of psychological defensiveness as measured by the Defense Mechanism Test (DMT). This area is of interest in the assessment of replicability because, although only a relatively small number of studies have been conducted, it is quite unlikely that any unreported studies exist which are unknown to those in the DMT area. This is because the technique for evaluating the DMT is relatively complex and is known to only a handful of psychologists. It is likely that anyone seeking to use the DMT would have communicated with the psychologists in Sweden and in the Netherlands who are familiar with the test and intended DMT-psi studies would therefore be known to the DMT investigators. Ten complete studies of the correlation between DMT-defined absence of defensiveness and psi scoring have been published (Haraldsson and Johnson, 1979; Johnson and Haraldsson, 1983). Of these ten studies, seven have found a significant correlation in the predicted direction and three have not.

The replication rates for the areas described above are summarized in Table 1. This is certainly not an exhaustive review, but merely represents the repeatability records for certain selected areas which are of interest to the present writer. The numbers are derived from published reviews and in many cases have not been updated. "Significant outcomes" are those defined as such by the respective

TABLE 1

Replicability Rates for Psi Experiments in Selected Areas

Area	Total	Number of Significant Studies	Percentage of Studies	Reviewer
Early ESP Card Guessing	33	27	82%	Honorton (1975)
Remote-Viewing	28	15	54%	Hansen, Schiltz and Tart (1984)
Ganzfeld	48	23	48%	Honorton (1982)
Meditation	16	9	56%	Honorton (1977)
Hypnosis	42	22	52%	Honorton (1977)
Relaxation	13	10	77%	Honorton (1977)
Nocturnal Dreaming	16	9	56%	Van de Castle (1977)
REG-PK	214	74	35%	May, Humphrey and Hubbard (1980)
Defense Mechanism Test	10	7	70%	Haraldsson and Johnson (1979) Johnson and Haraldsson (1983)
Bio-PK	149	79	53%	Schiltz & Braud (1983)

reviewers. These definitions are in most cases based upon evaluations made by the individual experimenters—i.e., very little "methodological screening" has been done. The writer is quite aware of the problems involved in the use of such a "box score" method of assessing replicability. Many of these issues are treated in detail by other contributors to this volume and need not be elaborated here. I would like, however, to discuss three issues at this point.

Three Replicability Issues

The first issue is the role of possible *unreported* studies in assessments such as those made above. In some cases, such as the DMT research, it is unlikely that unreported studies exist which are unknown to the investigators. In other cases, such as remote viewing and Ganzfeld research, attempts have been made to assess the actual number of successful and unsuccessful replications by means of surveys (Blackmore, 1980; Hansen, Schlitz and Tart, 1984). What is of interest about the results of such surveys is that the proportion of successful outcomes of the unreported studies does not differ greatly from the proportion of published successes, and the inclusion of unpublished findings changes the "box score verdict" relatively little.

An alternative approach to this issue has been suggested by Palmer

(1977) and elaborated by Sargent (1981). This is the "directional analysis of significance procedure" (DASP), in which one assesses the number of confirming *vs* negating instances of the effect of some variable upon psi, analyzing published *significant* outcomes only. By chance alone, equal numbers of significant confirming and negating findings should occur. Sargent maintains that the DASP is unaffected by suppression (nonpublication) of null data. While this is true, the DASP would be influenced by suppression of *significant* negative findings. Sargent argues that such suppression is quite unlikely. Sargent applies the DASP to eight selected research areas in parapsychology (e.g., attitude, extraversion, neuroticism, etc.) and finds striking evidence for real directional effects in each area.

The second issue involves the *quality* of the designs of experiments contributing to box score assessments. Studies vary greatly in degree of methodological adequacy. A quite legitimate question is whether psi hypotheses are supported and high replicability rates are obtained only for the less adequate studies? Hyman (1982) has argued that this is indeed the case for the Ganzfeld-psi database. However, Blackmore (1980) did not find this to be the case, and Honorton (1982) has pointed to errors in Hyman's assessment and has provided convincing quantitative evidence that the psi-Ganzfeld replicability rate is *not* influenced importantly by the presence or absence of methodological flaws such as multiple analyses, potential sensory cues or "inadequate" target randomization.

A third, and most perplexing, issue concerns the specification of *exactly what* is being replicated in the areas assessed above. Is it the effect upon psi of the ostensible independent variables, or are we actually witnessing replication of the ability to elicit psi by particular laboratories or particular investigators? We may approach an answer to this question only when a large number of conceptual replications, each including appropriate control or contrast conditions (inclusions which unfortunately are lacking in most extant studies), have been conducted by a wide variety of labs and experimenters. Even then, possible psi-mediated experimenter effects will be difficult, or perhaps impossible, to rule out. However, although we cannot be certain of the precise locus or source of the psi effects in our present data, by comparing the number of significant experimental outcomes with the number expected by chance (*viz.*, 1 out of 20 in the studies assessed above), we *can* be certain of the presence of some form of psi in our experiments. Is this not a most useful form of replication at this stage in the development of our discipline?

Obstacles to Replication

We have seen that our psi experiments often succeed. However, almost equally often our psi experiments fail. These failures suggest a less optimistic half of the story which we must now examine. Balancing the indications of academic and scientific acceptance mentioned earlier are an equal number of indicators of *non*acceptance. The Parapsychological Association is an affiliate, not a full, member of the American Association for the Advancement of Science. Mainstream scientific journals continue to reject our parapsychological papers. Psychical research organizations and full-time psychical researchers are few in number and are poorly funded. Educational, training and employment opportunities in parapsychology are limited. Our understanding and control of psi remain frustratingly incomplete. What has gone wrong?

Many suggestions may be offered for psychical research's lack of progress. Perhaps the alleged psi processes that we think we are studying are not real after all, but are an amalgam of illusion, delusion, artifact, faulty observation and faulty interpretation. I think most of us would reject this possibility. Perhaps the psychic process is a delicate one which functions well only when a complex, poorly understood and rarely occurring set of predisposing conditions happens to obtain. Perhaps we are using inappropriate methods of investigation. Perhaps a group of conspirators has been consciously and cleverly sabotaging our progress. Perhaps certain implications of the reality of psi are threatening to us and motivate us to sabotage unconsciously our own work. Perhaps there is something intrinsic to psi itself which defies our understanding and control of the process. There may be elements of truth in each of these suggestions.

Evidence that Psi is Being Obscured

Are there any indications that the verification, replication and understanding of psi are systematically and, perhaps, actively, being obscured? Consider the following observations.

In the field and in everyday life:

- The aversion of ghosts and poltergeists to the presence of scientific investigators is well known.
- The frequency, magnitude and degree of impressiveness of physical phenomena in the seance room tend to be inversely proportional to the room's level of illumination and directly proportional to the laxity of controls imposed upon the medium.

- In the more modern experiments of investigators such as Batcheldor (1966) and Brookes-Smith (1973, 1975), table taps and levitations are most likely to occur under conditions of dim illumination and uncertainty as to whether the observed phenomena are paranormal or deliberately induced by the unknown "Joker" of the sitting. Additionally, the light-hearted, active, jovial, even boisterous mood which is produced in order to encourage paranormal events would seem, also, to discourage sober observation.
- In spontaneous cases, when psi does emerge into the light of day, it surrounds itself with so many uncertainties, unknowns and uncontrolled factors that its presence can be inferred only with great difficulty and never unequivocally.
- Many cases of ostensible psychic phenomena have been reported by witnesses who were in some altered state of consciousness when the phenomena occurred; is this psi facilitation or malobservation?
- Uncertainty may be contributed by "interesting misses" or distortions that accompany apparently veridical "hits."
- Uncertainty about the reality of a psychic experience may be contributed by the passage of time.
- Psychic functioning is an essentially unconscious process.

In the laboratory:

- Transplanting psi from its accustomed habitat to the rarefied atmosphere of the laboratory typically results in a reduction in its yield; some of this reduction could be attributed to the degraded levels of meaning, importance and significance of the laboratory tasks used to access psi.
- The detection, permanent registration and accurate documentation of psychic events may be impeded by faulty protocols, curious procedural mistakes, curious equipment breakdowns, flaws in "sound experiments," psychic events occurring outside of the protocol.
- Psi may hide in the apparently trivial nature of many of its manifestations—e.g., bent silverware, guessing which side of a white-and-green card is uppermost when the card is concealed inside an opaque envelope.
- The degree to which psi manifests in an experiment seems to be inversely proportional to the analytical sophistication of that experiment. The more information about the nature of psi a given experiment is likely to yield, the less will be the likelihood

of psi emerging in that experiment. "Loose" demonstrations seem to yield better psi results than "tight" demonstrations, which in turn seem to yield better psi results than analytical experiments. By "psi results" I mean just that, *not* artifacts that may be confused with psi. A "loose" demonstration is a protocol devoted to simply demonstrating the existence of some psi phenomenon, but one which contains sufficient flaws or loopholes so that the presence of psi cannot be determined unequivocally. A "tight" demonstration is one in which all loopholes or confounds have been eliminated absolutely; we may conclude with certainty that psi is present, but the design does not allow us to conclude any more than that. An "analytical experiment" provides information not only about the presence of psi, but also about its nature, about how psi interacts with other variables. The analytical experiment is the means by which process-oriented research is accomplished.

- Related to the above point is a phenomenon which might be termed the "overcontrol effect." Sometimes an investigator is so concerned with methodological minutiae, so careful and so critical of every possible procedural detail that an experiment finally emerges which is the epitome of soundness, elegance and sophistication, but which is totally lacking in any psi effects whatsoever.
- Given that psi has survived the various "filters" mentioned above and has manifested in abundance in some particular context, its interpretation is still obscured by a number of remaining uncertainties: (a) its type (i.e., whether the effect is telepathic, clairvoyant, precognitive or psychokinetic), (b) its source (i.e., whether the effect originates in the ostensible subject, the experimenter or someone else) and (c) its precise locus (i.e., whether psi is affecting the ostensible target or some other link in a complex chain of possibilities and whether psi is being modulated by the intended variables or by some other factor or factors).
- Despite the difficulties mentioned above, rather impressive, relatively interpretable results will emerge in the laboratory. These sorts of findings may not persist, but may succumb to the strangely ubiquitous decline effect.

We should pause for a moment to insert some important clarifications. It is true that the observations mentioned above do not always hold true. Sometimes dramatic physical phenomena *do* occur

under conditions of brilliant illumination and faultless control. Sometimes highly significant psi effects *do* occur in extraordinarily well-controlled analytical experiments. But I believe most of us would acknowledge that these delightful occurrences are *exceptions.* Unfortunately, the *rules,* the more typical cases, are more accurately depicted by the observations mentioned above.

Why do I bother to list these observations at all? Could they not be merely pesky accidents and inconveniences that have somehow persisted throughout a century-long concerted attempt to eradicate them? This might be so, were it not the case that these various tendencies occur too frequently and too conveniently to be accidental, temporary nuisances. Rather, they appear to suggest a nontrivial *pattern*—a pattern which interferes with the revelation of the nature of psi and with its control and replication, a pattern which results in psi remaining cloaked in mystery.

The "self-obscuring" character of psi has not gone unnoticed. In his 1909 essay, "The final impressions of a psychical researcher," William James remarked: ". . . I confess that at times I have been tempted to believe that the Creator has eternally intended this department of nature to remain *baffling,* to prompt our curiosities and hopes and suspicions all in equal measure, so that, although ghosts and clairvoyances, and raps and messages from spirits, are always seeming to exist and can never be fully explained away, they also can never be susceptible of full corroboration" (James, 1909). Over half a century later, Jule Eisenbud suggested that psi's resistance to verification attempts may not be accidental or temporary, but rather an "inherent, categorical limitation . . . something in the very nature of the beast" (Eisenbud, 1963). More recently, John Randall (1978) wrote of psi's "evasiveness," its "camera shyness," its general tendency to cover its own tracks.

We have examined some of the ways in which psi "covers its tracks" in the field and in the laboratory, in what philosopher of science Hans Reichenbach (1938) has termed the "context of discovery." Tactics that look suspiciously like self-obscuring ones continue into the "context of justification," the arena in which we attempt to communicate our work to fellow scientists and to the public. Here we enter the fascinating realm of the psychology and sociology of science.

The stratagems employed by the critics of parapsychology in their efforts to prevent awareness and acceptance of paranormal findings are not of primary concern here. Harry Collins and Trevor Pinch, two University of Bath sociologists who specialize in the sociology of science, have provided an excellent review of some of the favorite

methods of the critics, including the following: ignoring the findings, blank refusal to believe, using the symbolic hardware of philosophy (especially the argument of parsimony), association of parapsychology's findings with unscientific beliefs, accusations of triviality, attacks on methodological precepts, unfavorable comparisons with canonical versions of scientific method (stressing the absence of repeatable experiments and adequate theory), suggesting fraud, various *ad hominem* arguments, magnifying anecdotal evidence, denying orthodox publications and diluting orthodox publications when they do occur (Collins and Pinch, 1979). Here, we shall mention some of the ways in which our own psychical researchers may retard rather than further the progress of our discipline.

- We are wary of "too impressive" results. Upon confronting truly impressive data in our laboratories, our first inclination is to distrust them. Investigators sometimes do not report exceedingly significant findings for fear of their colleagues' reactions to those results. On the other hand, an abundance of minimally significant probabilities (i.e., .05, .01, .001) are reported.
- Data sometimes have curious ways of going into hiding. They may disappear after being left on a train in Waterloo Station or be hidden "under a cloud of statistical confusion," as in John Coover's early experiments at Stanford University (Pratt and Rhine, 1961).
- We abandon promising lines of research.
- Some psychical researchers make such extravagant claims and engage in such unusual activities that this detracts from their more careful, more conservative work and from the field as a whole.
- On the other hand, extreme solicitousness of one's scientific reputation is not without its own psi-obscuring aspects. An overly cautious attitude may actually repel personal psi experiences along with the knowledge and understanding that accompany those experiences. Excessive conservatism may prevent encounters with groups or individuals who may possess useful information about the psi process, information which is typically ignored because of its source. In addition, overly conservative statements to the public or to the scientific community may dissuade others from the same sorts of potentially profitable interactions.
- Psychical investigators have sometimes engaged in fraud, which has called into question all of their own work and, sometimes, entire areas of similar investigations as well.

- On the other hand, excessive concern about fraud and overreaction to this issue may promote certain psychological strategies and defenses which are themselves psi-antagonistic. Out goes the baby with the bathwater.
- In communicating our findings to others, we tend to restrict ourselves to "reputable" conferences and publications. Usually these limitations quite admirably accomplish the purpose for which they were instituted. But with limitations comes the risk of rejecting valid information. Not only might we deprive ourselves of new knowledge, but by isolating ourselves from "less reputable" investigators we also deprive them of information and methodological and interpretive skills which they might learn from us and use to increase their own credibility. We safeguard our scientific reputations by avoiding such company, but by withholding our thoughts, findings and suggestions we actually encourage the spread of inadequately documented findings and prevent the integration of knowledge which is our professed goal.
- The psi-antagonistic publication policies of mainstream scientific journals have already been mentioned. At the other end of the continuum is the wholesale publication of sensationalistic and less carefully researched articles, periodicals and books by the mass distribution publishing houses. The result is the dilution of accurate information about the paranormal, an obscuring of psi and its nature. It is almost as if the publication of each volume which clarifies the psi process is matched by the publication of several volumes which serve to obscure psi, which effectively cover up any tracks that may have been left by the original publication.
- Similarly, within the scientific community (and even within parapsychology itself) an advance in our understanding of psi is followed by an attempt to obscure that advance. Within our own field, that obscuring attempt, frequently in the guise of a clarifying effort, may take the form of a methodological critique of some successful experiment or series of experiments. Such critiques have a tendency to magnify minor flaws in the original work, exaggerating these flaws so greatly that the value of the original contribution is not properly appreciated.
- The recent emergence of critical organizations such as the Committee for the Scientific Investigation of Claims of the Paranormal is a clear indication that there exist social forces devoted not only to the obscuring of our understanding of psi,

but also, unfortunately, to the prevention of even the very conduct of psi research.

Again, certain clarifications may be in order. I do not wish to underplay the importance of some of the activities mentioned above. Such activities may be quite useful and may contribute importantly to the scientific enterprise. I do wish to point out, however, that these same activities can actually be self-defeating if carried too far or if engaged in with improper spirit.

Motives for Mystery

From a consideration of the observations mentioned above, a pattern emerges which suggests the existence of some process which functions to impede replication efforts and minimize unambiguous knowledge of the existence and, especially, the nature of psi. If such an obscuring process does in fact exist, what might be its function? Which sorts of motives might mystery serve?

Intellectual resistances. The most obvious motives served by mystery are intellectual ones. It is easy to understand the readiness with which one who has a great investment in a particular scientific world view will oppose and attempt to keep hidden any evidence deemed incompatible with that world view.[2] Cloaking psi in mystery would serve well the critic of psychical research by extinguishing any embarrassing light which might be shed upon deficiencies of his world view by troublesome facts. But would mere intellectual opposition sufficiently account for the often virulent and irrational manner in which this opposition is expressed? And what about the similarly strange behavior of parapsychology's friends and even, as so masterfully related by Inglis (1977), of the early psychical researchers themselves? Nor has the opposition of our friends been restricted to the past, as an examination of recent issues of our journals will reveal.

Emotional resistances. A powerful response suggests a powerful motive, one which is likely to be accompanied by a variety of formidable defenses. Can there be any doubt that a major motive for obscuring evidence of psi is a reluctance to consider some of the more frightening implications of this evidence? If psi is a reality, then in a very real sense thoughts become things—things which may supply useful information and produce beneficial effects, to be sure, but which may also provide knowledge of which one might prefer to remain ignorant and produce outcomes which one might prefer to avoid. The acknowledgement of psi may be accompanied by a

recrudescence of fears, not only from our individual childhoods, but from the childhood of our species as well, of insubstantial, but potentially malevolent ghosties, ghoulies, long-legged beasties and all manner of things that go bump in the night. Those who have reduced their anxieties about such things through a strategem of denial would prefer to avoid reminders, however gentle and indirect. But perhaps even more frightening than the harm that might be done unto us is the possible harm we might psychically do unto others. Possibilities range from relatively minor mischief to the fulfillment of the death wish (Eisenbud, 1972). Even the possibility of positive influence may be frightening since with this possibility comes a burden of responsibility, a necessity for exercising good judgment regarding the best possible use of psi abilities.

Related to the fear of possible unwanted influence is the fear of possible unwanted knowledge. The actuality of psi obviously implies the revelation of secrets, one's own and those of others. Motives and mechanisms which result in the retention of secrets would be expected to interfere with any process, such as psi, which threatens to reveal those secrets. The greater the anticipated negative reactions of others to the revelation of one's secrets, the greater would be the expected opposition to psi and the greater the motivation for making a secret of psi itself.

Charles Tart has discussed similar motives for opposing psi in his recent article (Tart, 1982) on the "social masking" and "primal conflict repression" theories of psi inhibition. According to the first theory, ordinary social intercourse involves a great deal of masking of our true thoughts, feelings and motivations, and strong psi functioning, unless it could be systematically blocked, would have great potential for drastically disrupting this social balance. According to the second theory, we have learned to repress our psychic functioning because of negative experiences and emotional conflicts we may have experienced in early childhood as a result of the information conveyed through psychic channels. Tart suggests that it is psychologically "safer" to deny the reality of psi than to take the chance of reactivating primal conflicts.

The implications of psi's reality threaten our very notions of individuality. Where and when do "I" end and "you" begin? Is there really an "I" separate from a "you"? To most Westerners, it has become second nature to think in terms of a universe occupied by separate entities and peopled by distinct, discrete individuals. To return to our first nature and entertain a different world view has momentous implications for all human endeavors. K. R. Rao (1981)

has expressed a related idea. He suggested that if death is dissolution of individuality and the return to undifferentiated being, then the fear of death which is so universally experienced may be an assertion of our unwillingness to return to a state that is devoid of personal boundaries. The fear of death may amount to an abhorrence of total psychic openness. The craving for individuality may be the root cause of our loss of psychic potential.

Once upon a time, open trafficking in psi could have disastrous consequences for both practitioners and investigators. These consequences varied in severity from raised eyebrows of acquaintances, through loss of one's scientific reputation, to social ostracism and even finding oneself consumed on a fiery faggot. An adaptive response to such dangers was to go underground, to keep one's interest in psi well hidden. Is it possible that the once adaptive strategy of hiding psi became overgeneralized, somehow extending to the very evidence for psi?

It is conceivable that some of our experiments evidence little psi because the involved subjects or experimenters have too much psi. Subjects or investigators may peer psychically into the realm of future possibilities, perform a kind of cost/benefit analysis of the most likely outcomes and abort any experiments or research programs which promise to yield a net negative outcome. Psychically sabotaging ultimately unfavorable experiments may be an adaptive method of minimizing further probing which a subject might find uncomfortable or of avoiding experiments with noxious aspects.

We have considered cases in which evidence for and knowledge about psi may be kept hidden in order to avoid certain negative consequences of psi's existence. However, mystery *per se* may be positively valued; the very existence of unknowns may be positively rewarding. To those for whom the experience and investigation of anomalies is pleasant, the completely replicable psi experiment and the completely understood psi process would actually be disappointments. Unconscious psi-obscuring strategies might be mobilized in the service of such a mystery-maintaining motive.

Mystery might also be maintained in the service of "martyr" and "frontier" motivations. However reluctant they might be to admit it, a certain proportion of psychical researchers may enjoy being in an "unaccepted" field such as psychical research. With their small band of noble comrades, they battle the entrenched citadel of establishment science. They gallantly blaze difficult but exciting trails toward new realities. Such outsiders would be devastated were their mission actually to succeed and their cause be incorporated into a

new orthodoxy. Psychological and psychic stratagems would certainly be mobilized to prevent such a possibility.

Maintaining functions. Rao (Vaughan, 1976) has suggested that perhaps we have developed defenses against psi in order to maintain our more conventional cognitive systems and that psi provides wrong information as often as it provides correct information, so that our conventional cognitive systems are not abandoned in its favor (Rao, 1979). Thus, inaccurate psi information *or absent psi information* could serve a balancing or maintaining role with respect to our conventional cognitive processes.

If Nature has programmed psi as an essentially unconscious process and if conscious awareness is not only unnecessary, but perhaps even antagonistic to the process (Braud, 1978), one begins to wonder about the wisdom of attempts to make people aware of their psychic functioning. Carrying this reasoning one step further, might not increments in our conscious, formal (i.e., scientific) knowledge of the psi process lead to attempts to consciously modulate this essentially tacit process and increase the likelihood that the process might be disrupted? Stated somewhat differently, could the integrity of the psi process be maintained to the extent that it remains obscure? From this viewpoint, critics of the paranormal who oppose its formal study become friends of the psi process, rather than its enemies.

Control functions. Could the low frequency and sporadic nature of psi occurrences serve some type of control function? Might meanings and messages be encoded not only into psi events themselves (see Honegger, 1980), but also into the more general context or matrix in which those events are embedded? Bluntly stated, is there a set of messages to be conveyed by the observations that (a) psi is real, but (b) psi cannot be controlled or replicated at will? Space does not allow a consideration of what some of those messages might be, but the intrigued reader might attempt the exercise. Opposition to these messages or implications, conscious or unconscious, would be expected to generate defenses against psi evidence itself.

An observation: The temporal distribution of spontaneous psi episodes and laboratory psi episodes greatly resembles the distribution of reinforcing events which one finds in a so-called "variable interval" reinforcement schedule, an intermittent schedule of reinforcement which generates extremely stable and persistent behavior (see, for example, Ferster and Skinner, 1957). For one who values paranormal occurrences, experiencing them according to what approximates a variable interval schedule almost guarantees persistence and dedication to their study. The power of this schedule may account, at least in

part, for the continuing interest in subject matter which is hidden a great percentage of the time.

Another observation: Obscured psi brings to mind two interesting metaphors. These are the metaphor of the hurdles and the metaphor of the flowers. The hurdle metaphor suggests that the obscurity of psi serves a kind of "screening" function, a test of one's persistence and dedication. The flower metaphor suggests that the obscurity of psi may function as a reminder that psi occurrences are phenomena, indications or symptoms of something greater. Flowers are beautiful and impressive and easily attract attention. But flowers are ephemeral; they wither and die, leaving behind them a more solid and more persistent system of plant and roots, without which there could be no flowers.

If psi replications are in fact being impeded by some of the factors discussed above, then by becoming more aware of those impediments, we might be able to minimize or eliminate them and thereby increase the likelihood of success of our replication efforts. Here are some specific recommendations which we might find useful in such an endeavor.

Counteracting Intellectual Resistance

Seeing is believing, but in many instances believing may be the source of seeing. A first step in getting psi out of hiding is to believe in its existence. If one's world view leaves no room for psi, psi is not likely to appear; if it does appear, it is not likely to be noticed or welcomed. Fortunately, world views can be changed to accommodate psi. We all like to think we believe in psi, but there are degrees of belief, and perhaps a very thoroughgoing belief is necessary if defenses against psi phenomena are to be eliminated completely. An intellectual training program might be recommended in which one immerses oneself in literature and discussions favorable to a world view which includes psi. Included in such a program would be dramatic and impressive evidential accounts of psi, as well as presentations of philosophical and scientific systems which stress the interconnectedness of apparently isolated events, the relativity of space and time, the interaction of observer and "reality" and the limitations of a purely rational, linear approach to reality. Observing one's intellectual processes, especially one's reactions to evidence which is incongruent with one's world view, could supply useful information. The practice of meditation could provide interesting insights into the nature of mental processes, as well as familiarize the practitioner with nonverbal, nonlinear modes of knowing.

Counteracting Emotional Resistance

Attempts might be made to become more aware of our emotional reactions to psi, especially any resistances we might have to psi and its evidence. We might explore the specific ways in which psi-phobia manifests in us. It is extremely unlikely that this phobia and its defenses are isolated from other aspects of our personality. Therefore, only the most thoroughgoing analysis would be expected to uncover the many subtle ways in which defenses against psi influence our thinking, feeling and behavior. In the absence of such a comprehensive self-examination, we might at least explore our feelings about psi, our motives for seeking and for hiding evidence for psi. We might consider what we would personally gain or lose if psi were to become an established reality. We might examine our attitudes toward individuality, personal responsibility and the retention and revelation of secrets. Perhaps this sort of self-work could be done in the context of a small group which meets regularly for this very purpose. Systematic psi testing of group members could be carried out periodically in order to ascertain whether the group work were indeed effective in disinhibiting psi. More difficult to accomplish, but certainly worth the attempt, might be the elimination of social punishment for the practice and study of psi.

Specific Research Recommendations

Here, in no particular order, are a number of specific research tactics which might be expected to minimize some of the various motives for concealing psi which were discussed earlier in this paper.

1. Studies might be designed in which it is difficult for any one subject to assume total responsibility for any observed psi manifestations. One obvious possibility would be the testing of subjects in a group situation. Another possibility is a study in which a subject is asked to relay or modulate the psychic influence of another subject. Still another possibility would be the use of rituals (e.g., certain induction procedures) or devices (e.g., automatisms) which might be held responsible for successful outcomes, freeing the subject from feelings of sole responsibility. In general, experimenters might attempt to learn the skill of convincing their subjects (and themselves) that they are worthy human beings regardless of their specific accomplishments or failures. Batcheldor (1966) and Brookes-Smith (1973, 1975) have made interesting observations and recommendations concerning responsibility in their discussions of "ownership resistance."

2. Observing the outcomes of one's psychic attempts may encourage a feeling of responsibility for those outcomes and inhibit further positive outcomes (Batcheldor's "witness inhibition"). Witness inhibition might be minimized by reducing immediate, trial-by-trial feedback to the subject. Delayed feedback might be less disruptive.

3. If feedback is provided in psi tasks, perhaps a more encouraging mode of presentation of that feedback could be devised. Feedback for hits only, or feedback in which hit indications are amplified or augmented, while miss indications are made less noticeable, could be tried. Experimenters might attempt to train their subjects to be less emotionally reactive to hit and miss indications.

4. The psi-obscuring consequences of feelings of responsibility may be minimized by increasing reliance upon unconscious psi tasks. Early psychical researchers emphasized conscious psychic experiences because the latter were frequently so dramatic that they were easily noticed and because early researchers did not have the tools necessary to detect unconscious psi. Today, we do have those tools, in the form of methods of measuring unconscious physiological reactions and methods for assessing unconscious psi-mediated behavioral reactions (e.g., the psi-mediated instrumental response procedures described by Stanford, 1974). Thus, we can now study psi without the need to rely upon conscious verbal reports and in a manner which allows psi to operate in ways more closely approximating those in which it functions in everyday life.

5. Our experiments should contain no aversive components. Potential subjects could psychically detect experiments having aversive aspects and avoid participating in such experiments. Subjects who do participate in aversive experiments may hide any evidence of psi which would result in subsequent exposure to those experiments either by themselves or by others. An experimenter who conducts an aversive experiment might psychically sabotage that experiment in order to avoid inflicting that experiment upon additional subjects. Note that some aversive components of experiments may be extremely subtle (e.g., boredom factors, idiosyncratic negative reactions to task stimuli, etc.), and extra effort would have to be devoted to their detection. Additional care might be taken in pre-experimental interactions with subjects in order to make them feel more comfortable in the experimental setting. We might even be more mindful of the precise language we use in our instructions to our subjects (e.g., do we "tell" them or "ask" them to do such and such, do we ask them to "try to influence the target" or simply to "influence the target," etc.)

6. We should be cautious in our use of deception in experiments. Such deception may involve misinforming or keeping subjects ignorant about certain aspects of an experiment, giving them false feedback, etc. As Tart (1977) has pointed out, using deception is a very tricky business. Subjects may discern such deception psychically and feel less trusting (and perhaps even resentful) about the experiment and experimenter. Deception may induce psi-antagonstic reactions and even counter-deception. Even the use of "control" groups or conditions may be problematical when dealing with psychic subjects. As Tart has suggested, we may have to re-think even some of our most fundamental assumptions about analytical experiments and process-oriented research, in view of the unique psychic characteristics of our subjects.

7. The ideal subjects for psi experiments may be persons who are not threatened by being "probed," who are comfortable with themselves and who have come to terms with psi. Experimenters involved in the Stanford Research Institute remote viewing experiments have mentioned to me that they seek out just these kinds of people as subjects. Perhaps this is one of the factors which contribute to the success of that program. Another success-contributing factor may be the SRI investigators' emphasis on letting their subjects know that it is OK to be psychic, that they have "permission" to behave psychically during the experimental sessions.

8. Psi experiments might be kept simple. Such experiments may be more likely to succeed than complex, multivariate experiments which threaten to reveal too much of psi's mystery.

9. Attempts could be made to make psi experiments more personally meaningful to our subjects. If psi functions in everyday life contexts in the service of needs, then laboratory experiments could be made to more closely resemble everyday life situations through the introduction of factors of need, meaning or importance. For example, a PMIR experiment could be designed in which the correct response provides the subject with an opportunity to satisfy some real need, gain useful information or solve some meaningful problem. Psychokinesis could be explored in the context of psychic healing analog experiments in which the desired PK outcome would benefit a living target system.

10. In addition to noting the outcomes of experimental protocols, investigators might attend more carefully to "messages" which may be contained in overall patterns of results of experiments, in peculiar malfunctions and mistakes that occur during experiments and in

possible psi manifestations which occur outside of the protocols and which would therefore not ordinarily be counted.

11. Attempts could be made to design studies in which psi's self-obscuring aspects could be directly investigated. Some possibilities include investigations of the "spreading thin" hypothesis (see Braud, 1978a) and studies in which the evidential value of various observations is directly manipulated (e.g., Bierman, 1979).

12. Increased attention might be paid to spontaneous case reports. In these cases, because of their reduced evidential value, psi may be less reluctant to reveal its secrets.

13. We might devote more attention to experiments in which the aim is the *blocking* of psi influence. If we can demonstrate empirically, to ourselves and to our subjects, that the psychic process can be controlled, can be turned off when desired, this would be of great value in reducing the threat of psi. Physical barriers and shields have been found to be ineffective. However, psychological and psychic blocking or shielding techniques have not been explored. Such experiments would seem to be of the highest priority.

These and similar research tactics may be useful in decreasing resistances and increasing the incidence of psi manifestations in our laboratories, the rate of successful replications and our knowledge of the psi process. All of this, of course, assumes that the major psi-obscuring factors reside in subjects and investigators. On the other hand, if the psi-obscuring factor is in some strange way inherent in the psi process itself, our attempts to influence the psychology of our research participants may be only minimally advantageous.

The Way of Applications

Three general strategies are available to those who seek to increase the acceptability of psychical research. These are The Way of the Replicable Experiment, The Way of Theory and The Way of Applications. It has been suggested that Science will pay serious attention to the findings of psychical research only when those findings are such that they may be repeated reliably by any investigator. Many of us have sought to develop an effective recipe for The Replicable Experiment, but the truly repeatable experiment continues to elude us. Several candidates have been proposed, but these have invariably failed to live up to their promise. The latest candidates were described in the first part of this paper. Time will tell whether these experimental methods will maintain their high replicability

records or whether they will join the ranks of earlier methods which seemed so promising, but whose lights soon faded.

It has also been suggested that scientific acceptability of psi findings will be achieved via The Way of Theory. Science abhors isolated, free-floating facts almost as much as Nature abhors a vacuum. Nature rushes in to fill its vacuums and Science hastens to fill the gaps among unconnected facts. Science fills its gaps with theories which tie together many facts that were not previously considered to be connected. Without an adequate theory to "explain" them, some argue, psychic occurrences will remain simply oddities, "damned facts" (Fort, 1919) whose lack of interrelationship with familiar processes embarrass conventional scientific investigators without provoking them to further explorations. Thus, some of us have attempted to devise models and theories of psi which might make our findings more palatable (see Rao, 1978). Unfortunately, we know so little about psi that our theories have been singularly unsatisfying.

The Way of Application has been relatively unexplored. Many argue that it is premature to consider the possible applications of psi, that too little is known and that psi effects are too small and too unpredictable to be of practical consequence. But perhaps these critics are wrong. Perhaps more attention, thought and research effort should be devoted to The Way of Applications. If uses could be found for psi, interest in the psi process would increase. If it could be shown that psi *works*, even if we do not know *how* it works, even the critics would find themselves paying increased attention to psi. Applications research may well be a royal road to acceptability. This may be so for a number of reasons. First, and most obviously, the study of useful applications of psi would attract to the field an increased number of investigators, increased visibility and increased financial and logistical support. Secondly, if psi is more likely to occur in response to a real *need* for its occurrence, then practical applications would provide this need factor, a factor which is conspicuously absent in typical non-applications laboratory studies. Thirdly, increased experience with positive applications of psi would be expected to reduce some of the negative emotional resistances which may be contributing to our obscuring of psi. What we use in positive ways, we fear less, and a type of "desensitization" could occur which might generalize and reduce our overall emotional resistance to psi. Finally, and of most relevance to the theme of the second part of this paper, applications research can still preserve much of psi's mystery. For something to be useful does not require

that it be completely understood. Thus, applications research and practice may be a useful compromise strategy by which we can satisfy our Mastery motive, while still providing Mystery a place to play.

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NOTES

1. These figures are based upon reports of the "success" of bio-PK experiments *as defined by the individual investigators.* Due to insufficient detail in some reports, it was sometimes difficult to isolate and properly evaluate individual experiments in papers describing more than one experiment. For a more detailed analysis, the reader is encouraged to consult the literature itself.

2. We should not ignore a possible psi-obscuring motive of a psi researcher who is also a "radical dualist," i.e., one who maintains that mind cannot be reduced to merely a state of the brain or of any other physical system. As Beloff has pointed out (Beloff, J. "Parapsychology and Radical Dualism." Paper presented at the 26th Annual Parapsychological Association Convention, Madison, N.J., August, 1983), the existence of psi may provide evidence for the radical dualist view that mind is a distinct, nonphysical, autonomous domain of nature. However, with regard to demonstrating the existence of psi, the radical dualist finds himself in a quandary. If too little evidence for psi is found, there is inadequate support for his position. On the other hand, if too much evidence is found, and especially if psi is shown to be reliably correlated with certain physical variables, then this may embarrassingly suggest that psi (and hence mind) is physical after all. Thus, the radical dualist may be motivated to find just enough evidence that psi is real, but not so much as to suggest that psi is a lawful, predictable, physicalistic process.

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DISCUSSION

WALKER: I am particularly impressed by Table No. 1. I think this is a marvelous thing for Dr. Braud to have prepared for us and to present here. It is most appropriate. My question with regard to it has to do with the point that was raised earlier concerning the belief in the law of small numbers. What if we were to go through all these experiments and add a correction as to how many we should expect to fail to replicate, considering the overall level of the signal that we are looking for—magnitude of the effect and the size of the test? I would not be surprised if we were to find that the numbers overall went up very high. To put it another way, when we take into consideration this law of small numbers, should we not expect to have seen just the numbers or very nearly those numbers not replicated as indicated in Dr. Braud's table?

A second comment is that I do not feel that theory is in a bad state. The work Rex Stanford and William Braud have done shows signs of theories actually working. I would say the same in regard to my own efforts at theory. What has been bad is that there have been critics who have been assumed to be knowledgeable with regard to the subject and their criticisms have been taken at face value. For example, Martin Gardner's very biased and self-serving criticism has been accepted as valid by some who should have been more knowledgeable. Even though the Gardner material has a lot of favorable comments, it is not to be taken at face value and I could say the same with regard to many other critics. I have not in the past taken it upon myself to attack these critics. I think that this will change

shortly and I hope I will be able to show you that theory is really in a much better state than some have implied.

HONORTON: My comment is in reference to the replicability table. A brief reference to Dr. Walker's question, what would happen if you attempted to apply a correction here for small n's? In several of these cases we have a very large number of samples relative to the others—for example, in the RNG, REG PK area. I don't know what the average sample size there is, but it certainly is an order of magnitude or more greater than it would be in the Ganzfeld or meditation or remote viewing work, but we see there one of the lowest overall success rates. On the other hand, we see a fairly high one for the early card guessing studies which also tended to involve fairly large numbers of trials. I know that nothing was intended here other than a very rough overview. Some of these areas have undergone more and others less critical scrutiny and replication estimates go up and down in relation to the degree of critical examination.

BELOFF: I have one general comment and one rather specific query. For a long time now, I have been fascinated by this phrase you have introduced—the self-obscuring aspects of psi—since you sent me an earlier paper. Here you have elaborated on it a great deal, so I am beginning to see more clearly what you have in mind by this phrase which I think surely rings a bell with all of us. Anyone who is engaged in parapsychology knows the dreadful perversity of the phenomena, which are always slipping out of one's reach. Now, in this paper there are two different kinds of self-obscuring aspects that you elaborate on. There are the familiar psychological blockages such as Batcheldor and his followers have emphasized. With this sort of thing I think we can all fairly easily come to grips and see the relevance. But over and above that it strikes me that in a lot of what you say you are almost driven to personify psi as if it were some kind of a curious entity that wanted to elude us. For example, you say that a very comprehensive multivariate experimental design might fail just because it's trying to pin down the components of psi in too thorough and systematic a way. It is a ghastly thought, because it seems to bring one into the realm of superstition. Maybe William James was on the same track when he talked about the Creator putting this field forever outside our reach, as if there were somehow hidden powers that just don't want us to make too much progress with psi. It is a daring idea, but I just would be interested in hearing you comment on it. So much for the general point.

One quite quick specific query—at one point you raise the question of the wording of instructions to the subject. But you don't follow

this up with any sort of guidance. Is one better off saying to the subject "try and influence this REG" or should one say "influence it" and hope that more definite commands will produce better results? I just don't know which of those alternatives one is better advised to try and follow.

BRAUD: I have three responses to your first comment. One is that this is simply a useful way of speaking—talking about psi self-obscuring aspects. Secondly, even if subjects are resisting psi for psychological reasons, if the participants are defending themselves against psi, they may be using their own psi abilities to do this. So, in a very real sense psi is obscuring itself. And, thirdly, there may well exist that ghastly possibility that you mentioned, that there may be something intrinsic in the process which prevents its complete understanding. I know Dr. Rao has thought about this and has mentioned it occasionally. A number of other people have also. If that is the case then it would suggest different approaches, different strategies to deal with it. But even then it is not totally hopeless, because a certain amount of revelation that psi would be willing to give would be expected and we could change the balance of our experiments to include more of a certain type and less of another type.

About the wording of instructions, this is a very theoretical kind of paper and I am not offering any detailed suggestions in any of these categories. I do think that we should be very careful of subtleties in the laboratory. I think Rex Stanford will discuss this at length in his presentation—everything ranging from the furnishings of the lab to the body language of the experimenters. There is no evidence on the role of instructions, whether it is better to ask subjects to influence the random number generator or to tell them to try to influence it, but I suspect the former would be more successful. This is an eminently testable idea.

SCHECHTER: I want to mention a concern that I felt as I was listening. Eliminating some of the obscuring factors by focusing on our participants' belief systems and attitudes is not the same as focusing on our own belief systems and attitudes. I agree that hyping up our own belief systems is going to make it far more likely that we will "see" psi—and this requires a great trust in our own perceptive and interpretive abilities and accuracies. My experience of seeing something is not a truth test in any objective sense; my experience is influenced by my beliefs. Even in the first part of what you presented today, you seemed to be assuming that the psi phenomena are there in spite of the obstacles that make it hard to be sure. This has a

feeling of pulling one's self up by one's own bootstraps. I agree we often get in our own way by being overcautious, but isn't it equally risky to say "Yes, it was there even if I didn't see it"? We need to balance very carefully the belief that may lead us to see things that we simply cannot verify without reference to our own belief systems, with being so cautious that we are going to refuse to allow our beliefs to have any impact. What are your thoughts on how we can do this without having it backfire on us?

BRAUD: I would not recommend that we substitute, but that we simply add the kinds of things I discussed as an adjunct to what we already do. It is true that I began by assuming that psi is real. Now I have seen enough, experienced enough to know that. Given that that is the case, how do I explain the absence of psi? Why does it not occur more frequently? A pattern emerges. It is very much the bundle of sticks idea. If you look at any one of these notions, then it doesn't hold very much evidential value. But all of them together are extremely suggestive of a pattern. So given that pattern there are some techniques we might use to modulate that effect. We would suggest as a beginning simply using the techniques that we are already using, but prepare ourselves or our subjects in different ways. Let the experimental design handle the truth assessing part of the problem. Let that be the fail safe device.

STANFORD: I am delighted by many of the things you say in your paper about what I would call the intra-psychic aspects of psi manifestation from the standpoint of the experimenter—what is going on in the experimenter's head. I am also especially interested in your remarks about how important it is that psi serves some real needs in a study. But the secondary sense in which I am pleased is that your paper is such an excellent complement to the subjects that I am going to be addressing this afternoon. You are dealing with something that fills out a vacuum I left and I will be filling out some things that you haven't mentioned, but these taken together will provide some very positive steps that might be taken toward enhancing replicability. My paper has a more social psychological orientation while yours is focused firmly upon the individual. I want to comment more specifically on one matter. You talk about attempting to make psi experiments more personally meaningful to our subjects. I happen to have known well a person who practiced as a psychic for many years. This individual has told me on many occasions, that there was rarely any success at all when the individual for whom the psychic was trying to give a reading did not have some kind of real need. If however, there were some kind of real need that could be fulfilled,

the chances of a real psi hit were extremely high. I suspect we could get similar remarks from a lot of psychics and I think we need to pay more attention to this. Perhaps we might ask what needs can our experiments serve from the perspective not only of ourselves, but of our subjects.

BRAUD: Let me make a comment about need. There are two possibilities here. One is that need is truly efficacious, need really has an effect on the presence of psi or the degree or magnitude of psi. The other possibility is that it doesn't really, but that it is a belief system that we happen to entertain. In either case it doesn't matter in terms of a practical outcome whether need is truly important, whether the people concerned believe this is important. If we present need the scores should increase, take it away and the scores should go down.

HONORTON: I think you have gone into some things that really have not been discussed much at all before, except after hours at meetings such as this. I am a little concerned though with your list of ways in which psi is self-obscuring. Particularly the first one, that we are wary of results that are too impressive, suggesting that we get an abundance of minimally significant results. In doing my Ganzfeld review for the Cambridge PA conference I was very impressed by the fact that half the studies that were significant at all were significant at the .002 level or lower, which, given that the average Ganzfeld study has 37 trials, is really quite impressive. I haven't done similar analyses in other areas. For a number of years we have speculated on whether there might be a tendency for psi to emerge in such a fashion as to just give us enough to meet our statistical needs; but at least in this particular area that doesn't seem to be the case. We will have to wait for Professor Hyman to finish his revision of his review of the Ganzfeld before going into this in detail, but my analysis indicates that the statistically significant studies are a little bit better in methodological quality than the nonsignificant ones. I don't know whether this generalizes to other areas or not.

Finally, I have a comment in relation to the discussion you were having with John Beloff in terms of instructions to subjects. Until we have done some systematic research, it might be that the best interim advice we could offer would be if you can tell subjects to influence the target and do it in a believable way, then that is probably going to be helpful. But if you can't do it in a believable way—if you are doing it because you think that it will help him, but you yourself are not optimistic, then you will make the subject more anxious and apprehensive, rather than facilitate his ability to get results. One

thing that is very important in dealing with instructions to subjects is that you really must have some sensitivity to the individual and tailor the instructions to the needs of the individual subject and the individual experimenter.

BRAUD: I am especially pleased with that last comment. It is very true if we instruct our subjects and we ourselves feel different we are actually increasing the conflict in those people and perhaps working to our disadvantage. Tailoring is also important. What I try to do as a matter of course is not have a stereotyped spiel that I use with our participants, but actually chat with them and rather informally attempt to find their level. Why are they there? What would be the best kind of approach to use in discussing the experiment with that person? And then tailoring the kind of remarks I would make to that person. It was a very useful comment.

HONORTON: I wonder if this is something that is at all unfamiliar to anyone here. You talk with the participant in advance to find out where he is coming from and attempt to tailor the whole situation as best you can and within the constraints of your experimental design to that individual.

BRAUD: Exactly. What we have instituted gradually is a two session procedure in which, if someone finds out about us, there is an initial screening that goes on via telephone and then there is a first appointment that is made and in that appointment we usually don't do very much in terms of an experiment. It is a "getting to know one another" session, an orientation meeting. That eliminates a lot of the first time effect and the anxiety. It also familiarizes the subject with the range of experiments and familiarizes us with that person. Then we can call that person to participate in an experiment which might fit. If the person is interested in healing, for example, then a bio-PK experiment would be useful. If someone has precognitive dreams, then there would be another set of experiments.

STANFORD: Chuck Honorton asked a question as to whether there is anybody here who is unfamiliar with this matter of seeking out where an individual is coming from, psychologically speaking, and tailoring one's remarks to him. I suspect that nobody here who does psi research is unfamiliar with that, but I equally strongly suspect that almost all experimental psychologists are unfamiliar with it. Even undergraduates learn very quickly that we must standardize all our instructions, so I think that these remarks may be very helpful to the very audience that might get out there and try to replicate some of our research.

BRAUD: Chuck Honorton made some earlier remarks to which I would like to return. One is that these conclusions that I am reaching

are global impressions. I, of course, haven't done counts of statistical levels to see whether in fact there is an abundance of .01s or .05s or .001s for that matter. That is simply an impression from looking over the literature, an impression that a number of us have had. Secondly, I have actually seen this effect at conferences in which people present dramatic results and there is tremendous resistance. I even know of cases where individuals in very well established laboratories have not presented results because they were simply too significant. They just didn't want to present these because the results seemed to be too good. It would be an interesting research project to go through the literature and do counts of the various probabilities to see if there is an excessive number of these compromise P's, minimally significant ones. That might be quite edifying.

BERGER: I want to address the question of instructional set and preface my comments with a personal opinion. When I design experiments of any sort, I don't feel comfortable asking a subject to perform a task that I cannot myself perform. This may or may not be a widespread attitude in this field, but I think it's important that at least some of the experimenters who are directly contacting the subjects be able to themselves perform the task successfully in order to both assist the subject in determining strategies as well as foster the belief system that the task is a possible one. It is quite common for an experimental subject, when given the instructional set for the experiment to ask the question, "But *how* am I supposed to do it?" Although most of us in our laboratories are proficient to some degree on some of our experiments, we haven't arrived at a recipe for successful strategies for any given experimental task. What we do have is our own gut feelings from our subjective experiences which are derived from our own interaction with the task. When subjects ask us how to do it, we can then tell them, "Well, this seems to work for us and you can try this, but try different strategies and see which one works for you."

The main point I want to make is that even if we did derive a recipe for success in a given experimental task, it is quite possible that the given strategy will be limited to a given subpopulation. If the instructional set interacts with a personality factor, say competitiveness, and in a randomly selected population half of the subjects are competitive and half are not, each may cancel out the effects of the other. If the experimenter is not aware that it is the competitiveness that is accounting for the variance between subjects, the experimenter would conclude that no effect was present. So I think that we must search for instructional sets that seem to work, and go the step further to define the populations on which they work.

BRAUD: I appreciate all those comments, especially that last one. I have been talking as though they were a main effect of everything that I discussed. But, of course, there are interaction effects as well. It may well be that one set of instructions would work for one experimenter and the other set with the other. There should be some tailoring there as well.

HALL: I like very much your emphasis on the subject and the meaning of things to the subject, which seems to me to be the weakest link in trying to understand the place of psi. We will have to understand that with regard to individual subjects and then in broader subject populations like sheep-goat differences in groups of people. I wanted to say something about Louisa Rhine's observation of the high incidence of psi in dreams and then something about the nature of dreams. With people in psychotherapy and psychoanalysis dreams relate to the needs of the person, but they usually do not necessarily relate to the conscious needs of the person at the time that the dream occurs. They may do that, but the broadest statement would be that the dreams are related to the ongoing life process. Now, it may be that the dream is getting a person ready, in a precognitive way, for a problem that is coming up, rather than dealing with the problems a person would list at that time. If that sort of thing is reliable, how could that be translated in some kind of experimental design? It would be necessary, for example, to take an organism less than human where one could more clearly define what is good and bad in the future. It might be an organism that would easily involve human feelings, like pet dogs and cats, or it might be possible to take a population of human subjects such as prima gravida women about to give birth for the first time. There would be a great deal of archetypal meaning and there should be some strong motivation to know such things as the sex of the child, the health of the unborn child and things of that sort. I am trying to look for some way to touch this dream level and link it into the motivational problems that we have.

BRAUD: Yes; it would be interesting, if I understand what you are saying, to use the dream processes as incentives and rewards in some way. Ideally, if we could be sure that person X had a particular dream on Monday evening, we could then arrange an experiment on Tuesday in which the reward to that subject would be an interpretation of that dream. I don't have any ideas immediately about how dreams could be used, but since they are containers of so much motivation it is a very useful place to look.