

WHEN OMNISCIENCE IS WRONG: PSI ERRORS
AS A CLUE TO THE BOUNDARY
OF PSYCHIC FUNCTIONING

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Background: The Question of Boundaries

In the mid-1970s, parapsychology experienced something of a paradigm shift when two excellent reviews of experimenter effects in psi research were published (Kennedy and Taddonio, 1976; White, 1976a, b). Both reviews raised the possibility that scoring differences related to experimenters, randomizers, data checkers and others were not necessarily a result of subjects' conscious or psi-mediated reaction, as had previously been thought, but rather were a result of psi on the part of laboratory personnel.

The implications of such an idea for psi research are profound. In certain types of experiments—say, a test of PK on a novel target system—it is not important to know who is producing the PK effect if we want to know only if PK can affect the system. Further, in research with psychically talented “special subjects” the consistency of their high scores across numerous experimenters, randomizers and so on, grants credence to the idea that in these cases the subject is, indeed, the source of the psi effect.¹ But the vast majority of experiments in parapsychology are aimed at the discovery of predictors of high-scoring subjects or the development of test methods that would produce high scores in average subject volunteers. For this type of research the problem of the psi-based experimenter effect is particularly critical. It is necessary to know whether a correlation between, say, extraversion and PK scores represents a genuine relationship between personality and psychokinetic ability or whether it is merely a paranormally produced expression of

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the experimenter's expectation that such a relationship would occur. If experimenter psi influence cannot be ruled out, the latter interpretation remains a possibility.

Two suppositions regarding the range of psi functioning, also developed in the mid-1970s, form the foundation of the psi-based experimenter effect. The first of these, supported empirically in research by Stanford and others, is that an individual can use psi without being consciously aware that he or she is doing so. "Nonintentional psi," as it came to be known, was tested by experiments in which the decision as to whether a subject would experience a pleasant or boring condition was based on a random event about which the subject had no knowledge. Thus, even though subjects were not aware of the random event nor of the relationship between its outcome and their future activity, they seemed to produce a psi effect so as to experience the pleasant condition.

The second supposition, proposed on theoretical grounds by Schmidt (1975) and Walker (1975) and supported by various literature reviews (Kennedy, 1978, 1979c; Stanford, 1977), was that psi operated efficiently to achieve the desired result and was not diminished by the complexity of the task at hand. (This concept has been referred to as psi's "independence of task complexity," its efficiency, or its "goal-oriented"² nature and I will use these terms interchangeably.) Given psi's independence of task complexity, the experimenter could not be ruled out as a potential psi source on the grounds that in order to produce a given result—say, the significant correlation in the above example—he or she would not only have to use PK on the random event generator (REG), but would also have to use ESP to find out a subject's personality score in order to know how much to bias the REG. This two-step task is a more complex one than the subject's one-step (PK only) task, but that does not mean that the experimenter could not have accomplished it.

These findings suggest an unfathomable reach for psi processes. If psi can operate without conscious awareness, then we cannot necessarily identify the source of a given psi effect on the basis of some behavior (e.g., sitting in front of a computer terminal and "wishing" for hits). Potential sources for a given effect, then, include the experimenter, a randomizer, or anyone who cannot be excluded on the grounds that their psi would have to go beyond the limits of known psi functioning in order to produce the effect.

But what are those limits? If psi is goal oriented, we cannot say that the task is too difficult—that it involves too many steps or the acquisition of too many bits of information—for psi to accomplish. If psi is not

diminished by distance, then we cannot say that an individual is too far away from the laboratory to produce the effect. And if psi is independent of time, the problem becomes even broader. For example, the "observational theories" of Schmidt (1975) and Walker (1975), which posit that psi is independent of space and time, hold that anyone who observes the outcome of a random event can influence that event, no matter when the observation takes place. These theories engender the "divergence problem" (Schmidt, 1975) which states that the psi effects of subsequent observers do not necessarily cancel each other out. Thus, if psi is independent of time, we may need to take account of possible effects from sources who are not even in existence at the time of the experiment.

While most parapsychologists restrict their concern about the source of an effect to an experimenter-versus-subject debate, this restriction owes more to convention than to theoretically justifiable reasons. If psi is independent of space, time, intentionality and task complexity, then theoretically any given psi effect could have been caused by anyone, acting at any time or from any place, and for any reason. This, then, is the dilemma confronting parapsychologists: without boundary conditions—circumstances under which psi theoretically cannot operate—we are in the scientifically untenable situation of trying to study a phenomenon which, *because of the very way we have conceptualized it*, appears to have an infinite number of causes.

As will be discussed below, there is reason to question how "boundless" psi really is. However, while it is unlikely to be as powerful as I've suggested, experimental evidence does imply that the problem is not entirely a hypothetical one. To cite some recent examples, the Kreitlers (1982) found that subjects made more references to anger on a variety of measures when "agents" in another building were role-playing anger-arousing scenarios in which, as part of the psychodrama, they were asked to "transmit" their anger. The agents were not trying to transmit their emotion to the subjects; in fact, neither they, the subjects, nor the experimenters working with each group knew of the other's existence or even that the experiment involved ESP. Solfvin (1982) told assistants taking care of mice that had been inoculated with *babesia rhodaini* (akin to malaria) that a psychic healer would be "treating" certain mice, though the assistants were not told which animals these would be. In fact, no healer participated in the study. Solfvin merely placed photographs of randomly chosen mice into envelopes labelled "H" ("healed") and "NH" ("not healed"). This simple sorting—the type of chore that might be assigned to a student assistant in another psi experiment—resulted in the experimental mice actually showing

significantly greater recovery from the illness. Lastly, Bierman and I (Weiner and Bierman, 1979) found that a manipulation carried out some six months after the completion of an experiment (Weiner, 1978) seemed to have had a significant effect on the data.

While these studies are not the only examples of the further reaches of psi functioning, they illustrate some of the practical problems of doing science on omnipotence. An implication of the Kreidler's study is that persons not even aware of the existence of certain individuals can remotely influence those individuals; Solfvin's experiment shows a powerful effect resulting from a very simple action; and the Weiner-Bierman finding suggests that if an event after "completion" of an experiment can have an effect, then the experiment is never really done.

Of course, whether psi is, in fact, so completely boundless is another question. Not all parapsychologists believe, for instance, that psi is independent of space and time (e.g., Morris, 1980, 1982; Osis, 1965). Further, if psi truly has no boundaries, if it is omniscient and can come from anywhere, then each parapsychology experiment would be bombarded with an infinite number of possible psi "inputs" and we would see no consistency across experiments. However, such is not the case. While we may not as yet have a cookbook formula for significant results, it is certainly not fair to say that the literature, taken as a whole, shows no generalizable trends. Therefore, it seems that *some* boundary conditions to psi exist; the question, then, is: what are they and how do we discover them?

Parapsychologists have generally dealt with the boundary problem in one of two ways. Some have developed new models that take the possibility of nonintentional and goal-oriented psi into account. Others have continued to conduct research as though the problem of nonintentional psi effects from experimenters and others is of minor consequence, presumably under the not unreasonable assumption that discovery of a repeatable experiment will obviate these headaches.

A third approach is to search across studies for conditions under which assumptions about the "boundlessness" of psi functioning seem not to hold. This approach is problematical, for the way in which we interpret the results of a study depends very much on our position regarding the suppositions described above. For example, how one might interpret, say, evidence of PK being independent of distance may depend on one's opinion about the possibility of nonintentional psi from someone closer to the REG than the putative subject. Similarly, Morris's (1982) argument against psi's independence of time is predicated on evidence for psi's independence of task complexity. Thus, taken together, the hypotheses that psi is independent of space, time,

intentionality and task complexity, form a tight web that makes the evaluation of any one of them contingent upon the evaluation of the remaining three. Somehow, then, we need to find a thread that we can follow into this web. While any one of the hypotheses might serve this purpose, the hypothesis that psi is goal oriented seems to be a fundamental one. If we can find evidence that in certain cases psi is not goal oriented, that its power to accomplish certain tasks is limited, then we may be able to use that information to develop tests of the remaining hypotheses.

This paper is an exercise toward this goal. Specifically, it reviews the recent experimental literature to see where the hypothesis of psi as a goal-oriented process might be challenged. This is done through a particular perspective, the rationale of which is developed in the next two sections.

Psi Errors as a Clue to the Boundary of Psychic Functioning

Parapsychologists sometimes comment ironically that psi having no apparent limitations, seems so powerful, yet it is also so elusive. This combination of characteristics is not necessarily paradoxical; we can imagine psi to be like a magical genie who is difficult to capture, but that once caught, it will grant our every wish, no matter how difficult that might be. However, if psi were completely goal oriented, then psi (when it occurs) should not be *wrong*. We should see each study with significant results coming out just as the psi source ordered—no reversals or contradictions of the hypothesis, no “displacements” onto another target. Thus, trying to determine the limits of psi by comparing nonsignificant with significant experiments will not be particularly informative, for we cannot know whether in the unsuccessful studies psi had “reached its limit” or had merely failed to appear. An examination of *psi errors*, on the other hand, may be more fruitful. While examples of psi errors cannot be said to represent boundary conditions to psi, they can (depending on certain assumptions discussed below) represent psi inefficiency in the process of accomplishing the psi task and, as such, may provide some insights into the conditions under which the goal-oriented hypothesis breaks down.

The Meaning of Missing

Before we begin, it is important to emphasize that a psi error is a *systematic distortion* of an apparent psi effect. If we use the old analogy of psi as a weak “signal” in a noisy channel, a psi error is not a case of the signal being buried in noise, but rather one of the signal being fed

into some sort of inverter. This point is obvious, but it becomes important when considering explanations for psi errors, for such explanations should clarify why an error as opposed to chance results would be expected.

In this paper I will be speaking primarily about one type of systematic distortion, psi-missing,³ though displacement effects⁴ will also be discussed. Whether these phenomena constitute an *error*, however, depends on one's assumptions regarding the purpose of psi. These assumptions need to be clarified.

Let us consider two interpretations of psi and psi errors. In the first interpretation, psi is defined unidirectionally; for example, ESP is operationalized as a statistically significant correspondence between a response and a randomly determined target. Under this view, only positive scores make sense as evidence of psi and any systematic distortion of scores such as significant negative scores, represents a mistake. Such a mistake may result from a cognitive-processing error when making this response, such as "consistent missing" (Timm, 1969), a guessing error in which a person systematically responds to a particular target with the same (albeit incorrect) call, or other cognitive mechanisms, such as systematic confusion and defense mechanisms (Irwin, 1979). Additionally, psi may mistakenly focus on the wrong target, leading to displacement. Missing can then result as a by-product of displacement if a subject who is showing displacement in forced-choice guesses also tends to avoid repeating the same symbol twice in a row, but displacement can occur in chance or hitting data as well and therefore be a psi error in its own right. At any rate, under the view that psi is a meaningful concept only when the information is accurate, these effects signal genuine mistakes and as such demonstrate psi inefficiency. I will refer to this view of missing and displacement as the "genuine-error" interpretation of scoring distortions.

The definition of psi as a correct match between response and target, however, assumes that the assigned task objective is indeed the source's real objective. In a second interpretation of scoring distortions, this assumption is not made and the "true" wish or intention is inferred from other information. For example, if the subject does not believe in psi, then we might expect that his or her "real" desire is to produce low scores. Psi-missing in a control condition may be interpreted as the subject's or experimenter's psi working to obtain the goal of a significant difference between experimental and control conditions. In this view, then, missing can represent the efficient operation of psi to fulfill the source's purpose; we have merely misidentified that purpose. I will refer to this second interpretation of scoring distortions as the

"motivated-error" interpretation, using the concept "motivation" *very loosely* to include a host of emotional or attitudinal factors from which a desire to score below chance is inferred. To say that a psi source is motivated to err does not necessarily imply that he or she is consciously trying to do so or is even aware of his or her motivation.

While many authors have referred to motivated-error interpretations of missing, the hypothesis is best capsulized in Schmeidler's recent writings on the topic (Schmeidler, 1982, 1983) in which missing is discussed in terms of the subject's motivation against the experiment, his or her need for punishment, or emotional resistance stemming from such factors as anxiety regarding success, dislike of the target and so on.

Treatment of Psi Errors in Theories of Goal-Oriented Psi

Understandably, theorists trying to deal with psi's boundlessness have not yet turned their attention to psi errors. Still, it is of value to review briefly theories of goal-oriented psi to see how they deal with the problem. Stanford's (1978) conformance behavior model structures psi events in terms of three elements: a "disposed system" which can be, but is not limited to, a subject with a particular target objective; an indeterminate process (i.e., a random event generator or indeterminate brain processes); and a relationship between the two such that the random process determines which one of a set of unequally favorable events will occur to the disposed system. According to Stanford, psi occurs as a consequence of a natural principle to bias the random events toward favorable outcomes. Stanford acknowledges the source-of-psi problem, stating that incentive value of the REG-determined outcome is probably important in deciding who the source(s) may be, but he is clearly leaving this issue for future refinements of the model. Psi-missing is not explicitly addressed, but the motivated-error interpretation of missing is consistent with the model if we posit that for some reason a disposed system may benefit from below-chance scores.

Braud's (1981) lability/inertia model is a refinement and expansion of the conformance behavior model. Braud conceptualizes psi in terms of a labile system (one with a lot of randomness) becoming reorganized so as to match a more structured, inert system. It is not immediately apparent how missing might be incorporated into the model (or even if this is Braud's intention), for missing would constitute an "antimatch" between the two systems.

Schmidt (1975) views a psi source as a system with a variable psi strength that serves to bias the probability of random outcomes away

from their a priori values. Missing is neatly defined as a psi strength less than unity and while Schmidt mentions that missing may result from psychological factors, he clearly holds to no particular position on the question. Walker's (1975) quantum mechanical theory of psi relies on a motivational interpretation for psi errors, though this is not a major part of his theory. In the theory, a necessary condition for psi to occur is for all observers of a random event to share the same "emotional set" (p. 10). Below-chance scores from skeptical subjects are explained by the discrepancy between the subject's and experimenter's emotional set, since the subject (unlike the experimenter) would feel displeasure at obtaining a hit.

Rao's (Rao, 1978b; Rao, Kanthamani and Krishna, 1979) volitional model is a theory of goal-oriented psi which holds that scoring is a function of *intention* and *attention*. Motivation is seen to influence intention, so the model might accommodate the motivated-error interpretation discussed above. However, in this model below-chance scores are not necessarily errors, but are also considered a manifestation of the nature of psi as a "balancing" function.

Given this brief overview, how might psi errors shed light on the limits of psi efficiency? Under the genuine-error interpretation of scoring distortions, the occurrence of missing and displacement in and of themselves would seem to contradict the usual assumptions of the goal-oriented model. Kennedy (1979c) however, has sought to reconcile ESP errors with goal-oriented models. He has argued that theories which consider the brain as a random event generator with indeterminate neural processes (e.g., Stanford, 1978; Walker, 1975) could explain ESP errors as cognitive or interpretational mistakes that occur when processing the ESP information into a response. While this is certainly possible, it does not deny the fact that *at the level of output* (i.e., the subject's response) some mistake has occurred. Cognitive errors then, may represent a "faulty component" in the brain-REG and, as such, may reveal constraints on the system. Thus, any suggestion from the literature that missing is related to cognitive error will suggest boundary conditions to the expression of ESP. A goal-oriented model which allows for motivated missing, on the other hand, would see such "mistakes" as the mechanism by which the source's purpose is achieved. These goal-oriented models would be drawn into question if missing occurred under conditions in which motivation for below chance scores could not be reasonably inferred.

In the next section I consider the experimental literature of the past six years as a region within which to explore these issues. In particular, I will examine cases of psi errors as a function of cognitive error or

motivation. The goal-oriented model will be challenged if the review uncovers evidence of (a) missing as a result of cognitive-processing mistakes; (b) missing where motivation is an unlikely explanation; or (c) displacement which cannot be considered a mechanism for motivated error.

Experimental Review⁵

Psi-missing as a function of a cognitive error. Earlier I mentioned consistent missing, a guessing error in which the subject systematically responds to a particular target symbol with the same (but incorrect) guess. The occurrence of this error would suggest some sort of cognitive "crossed wire" between the psi "input" and the guess. If we find consistent missing responsible for significant below-chance scoring, it would support a cognitive-error explanation for missing. Kennedy (1979a) reviewed previously published work with 11 subjects for whom consistent missing analyses were reported or for whom data were available to carry out the analyses. He found that consistent missing was not systematically related to scores: in some cases the calling confusion occurred with psi-missing, but it also occurred in chance and hitting data. Thus, from Kennedy's work we see that consistent missing may contribute to, but does not account for, below-chance scores. Further, in an experimental effort, Munson (1980) failed to find evidence of consistent missing in unselected subjects.

Rao's research on the relationship between ESP errors and cognitive (memory) errors was not promising for the cognitive-error hypothesis (Rao, 1978a; Rao and Weiner, 1982). The original report (Rao, Morrison and Davis, 1977) had found in an exploratory series that when hitters on a word ESP test guessed incorrectly, they tended to respond with a word of high semantic association to the target word (which implies memory retrieval processes at work in ESP), whereas missers tended to respond with a word of low semantic association. This finding was not replicated in a subsequent series nor in their second report (Rao, Morrison, Davis and Freeman, 1977), though the latter study obtained marginal evidence that subjects whose memory errors showed this response tendency also exhibited the tendency in their ESP errors. In the recent pair of attempts Rao's (1978a) effort can be said to have only weakly supported these results, at best, and the Rao-Weiner study was not significant.

Blackmore (1981) took a different approach to the study of ESP errors by seeing whether ESP "confusions" would resemble either perceptual or cognitive (memory) errors. In her experiment the targets

were either identical, perceptually associated, or semantically associated to the response choices. (For example, if the response choice was "black," "white" would be a semantically associated target, whereas "slack" would be a perceptually associated target.) She found significant differences among target types, but the differences were due to significant missing on the identical (i.e., no confusion) targets and therefore her study, too, sheds no light on ESP confusions.

On the other hand, Rao and Harigopal's (1979) experiment, while a test of personality differences and ESP, may support the cognitive-error hypothesis, as they found overall missing for Indian subjects who had scored high on *tamas*, a personality trait within the *guna* system, which is associated with a tendency toward cognitive mistakes. Further, a handful of studies can be interpreted retrospectively as showing missing under conditions of high cognitive load or interference. Kreiman (1978) confirmed his prediction that after memorizing a list of words (some of which were to be selected as precognition targets) subjects would avoid the ESP words at the beginning of the recall period when memory processes would presumably be competing with ESP for attentional capacity. This aspect of his experiment was not replicated by Weiner and Haight (1980) nor Schmeidler (1980), though it did receive marginal confirmation in a subsequent test (Schmeidler, 1981). High cognitive load may explain why Bosga, Smid and Bierman (1981) obtained significant missing with sleep-deprived subjects for free-response trials, but not for forced-choice trials; presumably, free-response trials would require more cognitive effort. Otani (1979) reported scoring declines in the one or two trials following an unexpected auditory stimulus, but it is not clear from the abstract whether the "decline" was psi-missing. In an ESP task involving the placement of letters of recalled words either above or below a dividing line so as to match randomly determined orders, persons who were "high imagers" psi-missed when recalling hard-to-visualize words (Sargent, 1978c). Levi (1979), on the other hand, obtained psi-missing in a PK condition ("goal-oriented" imagery and no visual feedback) which, to my mind, constituted the condition of *least* cognitive load, though Levi interpreted the situation differently.

Thus, the cognitive-error hypothesis for psi-missing was not well supported in recent research; consequently, it does not challenge the goal-oriented model. Consistent missing as a phenomenon suggestive of cognitive error was not systematically related to below-chance scores. Both Rao's and Blackmore's work failed to provide evidence that ESP misses or confusions resemble cognitive mistakes. There was retrospective evidence that missing may be facilitated by situations with high

cognitive load, but we cannot rule out that affective reactions that may accompany such load (e.g., frustration, lack of confidence) may be the real cause of the missing. For example, Sargent's (1978c) work with target-word imageability had subjects making ESP calls as part of the way they wrote their recall responses. Thus, the ESP test took place at the same time as recall but was not an integral part of the recall process. Momentary frustration among high imagers confronted with the task of trying to remember low-imageability words is as likely an explanation for their missing as a cognitive-processing interpretation.

Psi-missing as a function of motivation. With the exception of two partial reversals in which believers in ESP significantly missed (Thalbourne and Jungkuntz, 1983) and subjects with less belief in PK significantly hit (Weiner, 1979), lack of belief in psi continued to be related to missing. Goats missed in two series (Thalbourne, Beloff and Delanoy, 1982) and in a language ESP test Munson (1981) found that goats (as determined by a question emphasizing belief that the subject's ESP could "cross the language barrier") scored significantly below chance overall and especially so on the foreign-word targets. The first series of Solfvin's (1982) healing experiments obtained a reversal (control mice healthier than experimental mice) for the animals handled by a skeptical assistant. Persons who reported having had no ESP experiences in a study strongly emphasizing spontaneous cases significantly missed on a forced-choice test (Haight, 1979), which the author interprets in terms of a sheep/goat effect. In a clever design, Lovitts (1981) presented an ESP test to half her subjects under the guise of a subliminal perception experiment intended to disprove the existence of psychic phenomena. Sheep and goats who knew that they were taking an ESP test scored above and below chance, respectively, in accordance with the general trend in parapsychology, whereas scoring direction reversed between groups in the "disprove ESP" condition. Thus, scoring direction above or below chance was related to subjects' perceptions as to whether or not success would be congruent with their beliefs.

Like belief, expectation regarding success (or, rather, failure) was also found to be related to negative scores. O'Brien (1979) manipulated the expectancy of data checkers, obtaining in one of a pair of series significant missing for the checker given a negative expectation. Jacobs and Breederveld (1979) suspected that the psi-missing of only children in an earlier study had been an experimenter effect. They tested this supposition by replicating the study with two experimenters, each with different expectations about the results. Subjects tested by the negative-expectancy experimenter obtained significantly lower scores than those tested by the positive-expectancy experimenter. Sargent (1978a) scored

a set of precognition runs and then randomly divided them into high versus low expectancy conditions, finding a significant difference between conditions even after correcting for selection from multiple analyses. Missing, then, seemed to be successfully induced by expectancy manipulations.

In addition, two other studies yielded results that could be interpreted as expectancy effects. In a within-subjects design, five of six groups of meditators significantly missed in forced-choice tests given prior to meditation (Rao, Dukhan and Rao, 1978), perhaps as a result of their expectation that pre-meditation scores should be lower than post-meditation scores. McLaren and Sargent's (1982a) special subject missed on dream precognition trials for which he was not confident.

Turning now to research involving other types of motivation, we find that certain studies attempting to increase subject motivation for hitting through monetary reward (Thalbourne and Jungkuntz, 1983) or subject preference of target events (Solfvin, Krieger and Roll, 1979) actually obtained missing. Similarly, significant overall missing in Schmeidler and Borhardt's (1981) experiment was based largely on significantly low scores in a preferred condition.

Under the motivated-error interpretation of missing, low scores come about as a result of subjects' unconscious negative reaction to the experiment, a reaction which could be stimulated by bad mood, discomfort in the testing situation or some other such factor. The literature of the past six years presents a mixed picture on this question. Rao (Rao and Kanthamani, 1980a) scored significantly below chance, which could be anticipated from his self-report (made prior to knowledge of the results) regarding his mood and general attitude during the course of the experiment. The subject-discomfort interpretation may also be supported by Shrager's (1978) finding that young children significantly missed in a GESP test when the agent was not their mother, Tedder and Monty's (1981) observation of suggestive missing for subjects less familiar with the experimenter and post-hoc analyses showing missing for subjects who had never been to the laboratory previously (Child and Levi, 1981; Morrison and Davis, 1979). However, the Child-Levi and Tedder-Monty results were weak and the Morrison-Davis report also mentions significant positive scoring among first-time visitors in a similar experiment. Haraldsson (1980), re-examining old data in an attempt to understand why the first of a pair of percipients in a physiological-response ESP test would psi-miss, found that lack of familiarity with the test environment was not a viable explanation. A test of this "first percipient effect" reported in the same paper also showed that giving the first percipient an acclimation period did not eliminate

the effect. So subjects' discomfort was not necessarily related to negative scoring.

Finally and most importantly for the motivated-error hypothesis, there are many cases of missing in this body of literature which cannot be only explained by inferred motivation or emotional reaction. Missing has been associated with metabolic type (Ruiz, 1984), subjects who reported ESP experiences that were not of the intuitive type (O'Brien, MacDonald and Bibeau, 1979), low-anxious subjects given end-of-run feedback only (Haight, Kanthamani and Kennedy, 1978), drawings that had been in contact with a person (Knibbeler, Boshouwers and Bierman, 1981), later runs of a precognition (but not a clairvoyance) test (McLaren and Sargent, 1982b), the pre-ovulatory phase of the menstrual cycle (Keane and Wells, 1979), an intentional, as opposed to a nonintentional PK task with pre-recorded targets (Terry and Schmidt, 1978), and the intervals when, unbeknownst to the subject, his left cerebral hemisphere (as opposed to his right) was being monitored, but only when he received feedback on the PK task (Heseltine and Mayer-Oakes, 1978). Although it is likely that at least some psi-missing results are due to statistical fluctuation, it is still not clear how many of these results can be explained in terms of inferred motivation or emotional response to the test conditions.

In summary, there is evidence to support a motivated-error hypothesis of missing if we restrict our consideration of motivation to those motivations having to do with belief or expectancy. Inferences that go beyond these, for example those regarding motivated errors due to subject discomfort, lack of enjoyment of the task and so on are not so well justified. This suggests a limit on the extent to which a goal-oriented theory can explain cases of below-chance scores on the basis that at some level the psi source "wanted" the scores to be low.

Displacements. Displacements are of interest in the present review for two reasons: they can be the mechanism for motivated missing, but if not motivated, they can be considered a "misfocus" of psi and therefore an error in their own right. If displacement is a mechanism for motivated missing, then it is not inconsistent with the goal-oriented hypothesis. On the other hand, displacement that does not serve this purpose would seem to constitute a genuine psi error and contradict the hypothesis.

Let us first consider displacements in forced-choice testing. Working with unselected subjects, Crandall and Hite (1983) found that missers tended toward +1 or -1 displacements, whereas hitters did not. This experiment is difficult to assess for a variety of methodological issues too detailed to mention here. However, as we shall see later, it is in-

teresting that the displacement/missing effect was stronger in the task (clairvoyance or GESP) which the subjects preferred⁶ and that in some series, sheep showed significantly higher displacement than undecided subjects or goats. Crandall and Hite's findings would argue against the idea of displacement as an expression of motivated missing, for we would not expect sheep or subjects working in a preferred task to be motivated to miss. Either that expectation is incorrect or displacements under these circumstances constitute a genuine psi error.

Sargent (1978b) has found -1 displacement effects in much of his research, but the interpretation of them as a misfocus of psi is compromised by evidence that the effect may be an idiosyncratic pattern associated with Sargent himself. Sargent (1978a) tested this notion by carrying out the post-checking expectancy manipulation described above, which was successful for the -1 displacements. In a second study (Sargent, 1978b), he confirmed his prediction that significant effects would occur in the -1 displacements of previously collected data that had not been analyzed for displacements. Sargent (Rao, Sargent and Schlitz, 1983) has also produced results in $+1$ displacements when acting as a subject, though the form of the results is rather complicated.

The displacement literature, then, shows some evidence of displacement occurring with below-chance scores on direct targets and of displacement as an idiosyncratic psi effect. The first point, displacement occurring with missing, *may* be consistent with goal-oriented models if the source is motivated to miss and uses displacement as a mechanism for achieving that end. If, on the other hand, missing is not motivated, but is just a by-product of displacement, it would support the notion that displacement is a genuine error and contradict the goal-oriented hypothesis. One attempt to see whether such missing was motivated was carried out by Child and Levi (1981). These investigators had previously observed missing and displacement in a free-response experiment (Child and Levi, 1979, 1980). Their later experiment was designed to eliminate motivational problems that may have been the cause of the previous result; however, this experiment also produced psi-missing. Thus, they were not able to confirm that missing had been motivated, at least by the factors they had controlled. The fact that Crandall and Hite (1983) observed greater displacement/missing in sheep and in the task subjects preferred also undermines the motivated-error interpretation.

The second trend, that of displacement as an idiosyncratic experimenter effect, is consistent with some of the goal-oriented models, especially the observational theories. These theories hold that in ESP

tests the person who observes the call-target correspondence (i.e., the data checker), rather than the subject, is the psi source. The observational theories would expect that when Sargent is looking for displacement he could "induce" significant effects even when he is scoring other persons' calls. From this perspective, then, these displacements are not necessarily a misfocus of psi, for they could have occurred as he was checking the data for -1 displacements. However, the observational theories do not explain why Sargent would find effects in displacement analyses rather than in those with direct targets nor why he would find these effects much more often in -1 displacements than in $+1$ displacements (Sargent, 1978b). Tracking down the cause of such results may suggest limitations on the effect of observation on ESP. In short, it is not clear from the displacement literature whether or not displacement is a motivated error.

Summary and Discussion

Let us recap the issues. The dilemma facing parapsychology, as a result of theoretical and experimental advances over the past decade, is that the concept of psi has become too expansive for the bounds of scientific methodology, at least as it has so far been applied in our field. Yet there are indications that some limitations to psi exist. The investigation of psi errors was discussed as a way to understand those limits by suggesting the conditions under which the hypothesis that psi is goal-oriented breaks down. If one assumes that psi is unidirectional—that is, that only correct target-response matches are evidence of psi—then scoring distortions are genuine errors. Generally it has been believed that such errors occur as a consequence of either a cognitive mistake or misfocused psi. Errors, however, may also be motivated by attitudinal or emotional factors. From this analysis we argued that the goal-oriented model would be drawn into question if we found evidence of (a) missing as a consequence of cognitive error, (b) psi errors that could not be attributed to inferred motivation, or (c) displacements which do not fit the motivated-error hypothesis.

The review showed that the hypothesis of cognitive error as a cause of psi-missing was not well supported in recent research. Direct tests of the hypothesis produced mixed results and Kennedy's review showed that one type of cognitive error, consistent missing, does not always result in below-chance scoring. In a retrospective analysis, some studies show missing under conditions of high cognitive load or attentional interference, but alternative motivational factors may account for this trend.

Before abandoning the idea of cognitive error as the cause of missing, however, we should bear in mind that cognitive processes are exceedingly complex. Kennedy found that consistent missing was not only *not* similar across subjects, but varied over time within the same individual. The special subject B.D., for example, exhibited a different type of calling confusion on his low-scoring runs than on his high-scoring runs. Rao, Morrison, Davis and Freeman (1977) noted that there was no evidence that, overall, their subjects responded with a closely associated word when making a *memory* error. In other words, their experiment was attempting to compare response tendencies in ESP to response tendencies in memory, which, for their subjects at least, did not exist. Perhaps, then, parapsychology's failure to find support for a cognitive-error hypothesis of missing has as much to do with the vagaries of cognitive processes (or at least of current understanding of them) as with the elusiveness of psi. Thus, at present there is little support for the cognitive-error hypothesis, except in individual cases, but this situation may change in the future.

The motivated-error hypothesis could reasonably account for missing if we restrict our consideration to those motivations having to do with belief or expectancy. Missing as a function of subject discomfort, mood, or reaction to test conditions is less well supported. As we shall see later, this conclusion is supported by evidence of *negative* scoring among subjects who look favorably on the task. Finally, there is a large body of studies in which motivational interpretations for missing become strained.

A reasonable response to the failure of the motivated-error hypothesis to account for all cases of missing is to say that we are not addressing the motivations of the appropriate psi sources. If the experimenter is the real source of psi, for example, we should consider his or her comfort, attitude toward the experiment and so on. This response points up one of the problems of evaluating the goal-oriented model. If psi can be used nonintentionally, then we cannot pinpoint the source on the basis of some overt behavior, but must instead look to some other means to link the source to the psi effect. When the link is forged by inferences regarding motivation, comfort and the like, the goal-oriented hypothesis becomes difficult to falsify, for we do not know whose motivations to consider nor how to identify those motivations.

I have developed this point elsewhere (Weiner and Geller, 1984) so I will not elaborate here. More to the point of the present discussion is the following question: which came first, the motivation or the error? Are all psi errors motivated, but because of our limited understanding of motivation we cannot clearly see this or do we infer motivation from

the fact that the error has occurred? If the former, the goal-oriented hypothesis can be falsified only when we have complete information about conscious (and presumably unconscious) motivations, which is a formidable task. If the latter, we run the risk of our inferences serving to protect, rather than to test, the goal-oriented hypothesis. For example, Edge's (1978) experiments in plant PK have been cited more than once as evidence for conformance behavior. In his experiments, light-starved plants were placed in front of a lamp which would be randomly turned on or off according to REG decisions. The conformance behavior model would presumably predict an REG bias in favor of increased light (in accordance with the disposed system's need), but two of the three significant series in Edge's report showed a *negative* bias—the light was *off* more than it was on when in proximity to the plants.⁷

Edge noted that after the experiment the plants had shown a phototropic response, bending toward the light and becoming greener, so presumably the "need relevance" of the light is not in doubt, yet these studies are considered support for the conformance behavior model. Most parapsychologists would suggest a psi-based experimenter effect to explain Edge's results, but in doing so, are we offering an explanation or merely changing our inferences regarding the source in order to retain our assumption of motivated missing? This problem, of course, is a general one having to do with the evaluation of goal-oriented models by arguments based on inference and is not limited to the conformance behavior model.

The motivated-error hypothesis for missing, then, can become unfalsifiable unless it is restricted to motivations which can be inferred from clearly defined criteria: questionnaires, interviews and the like. Debes and Morris's (1982) experiment is a good example of how missing in a certain task condition can be traced through subjects' questionnaire responses to their reaction to task instructions; more of this sort of investigation should be carried out.

For displacement effects to challenge the goal-oriented model, we must establish that they are genuine psi errors and not motivated. Under some goal-oriented models, especially the observational theories, Sargent's (1978b) work can be interpreted as a motivated psi effect. Additionally, the free-response displacement literature supports the goal-oriented models, but in an indirect way. Both of the experiments testing to see whether missing in a free-response task was related to displacement onto other pictures in the target pool found this to be the case (Child and Levi, 1980; Thalbourne, 1980). Two arguments can be advanced to explain why free-response displacement such as

this would be the most efficient way for a subject to miss, if he or she were motivated to do so. One argument, proposed by Thalbourne (1983), is that in a free-response test, missing would more likely occur through displacement onto another target than by active avoidance of target-relevant mentation. His rationale has to do with the amount of cognitive effort involved in the two methods. Missing by avoiding target-relevant information would require not only the repression of images that directly match the target (such as avoiding "tree" if a tree is in the target), but the repression of a whole network of "tree-like" responses as well (i.e., responses related to "tree" through semantic association, such as "flower", or visual similarity, such as "umbrella"). If this network is not repressed, a correct target-protocol match could still be made on the basis of associated responses. Applying Thalbourne's argument here, then, we can say that the simplest and most efficient way to miss on a free-response task is to displace onto another target in the set.

The second argument was made by Kennedy (1979c) and stems from a different line of reasoning. Kennedy suggested that if psi operates efficiently in a free-response test, one's mentation would focus on those elements of the target that most distinguish it from the non-target alternatives. In order to do this, psi must obtain information about the entire judging set, a hypothesis implied by the displacement effect.⁸ Thus, from both lines of thought displacement in free-response studies is an efficient way to miss, if one were motivated to do so. This argument does not prove that missing in free-response experiments is motivated; merely that if this is the case, the motivation is fulfilled in the most efficient way. Free-response displacements, then, are in accordance with the goal-oriented hypothesis if we accept Thalbourne's and Kennedy's interpretations.

The argument that displacement is an efficient way to miss does not necessarily extend to displacement in forced-choice experiments. Unlike displacement in a free-response test, displacement in forced-choice data would not seem to be a particularly efficient means for missing. Displacement on forced-choice calls leads to missing on direct targets only if the subject has the particular response habit of not repeating the same symbol twice in a row. If one were motivated to miss on direct targets, why bother to produce this calling bias and also to displace? Would it not be much simpler to merely select one of the nontarget symbols? However, we know so little about what route psi might "choose" in certain circumstances, we are not justified in drawing hard-and-fast conclusions as to what constitutes an efficient or inefficient method of accomplishing a task.

What, then, do psi errors say about the limits of psi functioning? The review of cognitive error as a cause of missing did not provide any strong hints in this regard, but this may be due to science's lack of understanding of human cognition. Thus, while present evidence does not suggest boundary conditions to psi resulting from cognitive-processing constraints, it may be premature to abandon the notion completely. The displacement literature presents a mixed picture: The results either support or contradict the goal-oriented hypothesis depending on one's interpretation. Since so little work on displacements has been done, we cannot freely generalize, but this area of research may be a fruitful one for the exploration of boundary conditions. Finally, I had proposed that if psi errors are not motivated, then they are genuine errors and therefore contradict the goal-oriented model. While the review did show evidence of errors resulting from certain motivations, there are many cases of errors in which the motivated-error hypothesis falls flat. Further, our implicit assumptions and our tendency to make inferences regarding motivation require careful examination, lest they render the goal-oriented hypothesis unfalsifiable. In short, on both empirical and logical grounds motivation is not an adequate explanation of psi errors. This conclusion constitutes a serious challenge to the notion of psi's omniscience, for it undermines the only acceptable explanation for errors from an omniscient force: "They were meant to be."

Magnitude-Based Constructs of Psi

There is an alternative way to view negative deviations that casts quite a different light on the above discussion. In this construct, the measure of psi is the *magnitude* of the deviation of a score from chance expectation, irrespective of direction above or below chance. This construct follows from Palmer's (1975) Model I in which factors influencing direction are separated from factors influencing magnitude, but in this construct the focus of experimental interest is on the latter rather than the former.

Magnitude measures can be taken with the subject as the unit ("subject variance") or across runs with a subject's performance ("run-score variance"). High subject variance would suggest that experimental conditions were eliciting psi, but that the direction of scores above or below chance varied from subject to subject. High run-score variance would imply a fluctuation of scoring direction within a subject's individual performance. A substantial amount of research has been carried out using magnitude measures, which has been ably reviewed by Carpenter (1977).

Since 1978, a number of studies have employed a subject's deviation ("subject variance") as the primary or secondary measure of his psi ability.⁹ This research shows a striking trend of high variance (large deviations) for subjects who can be considered to have had a favorable attitude toward the experiment or for a particular condition, whereas low variance was associated with unfavored conditions. For example, McLaren and Sargent (1982b) found that subject variance correlated positively to self-reported motivation in one psi task. Ballard, Cohee and Eldridge (1983) reported that low-anxious subjects exhibited larger deviations than high-anxious subjects and, in a second study, that sheep had higher variance than goats. Weiner (1979) noted a similar finding for subjects with greater or lesser belief in PK. In another study, students with positive attitudes toward their teacher-experimenter showed higher variance than those with negative attitudes (Haight and Weiner, 1981). In Stanford and Lafosse's (1980) research, subject variance was higher for those persons assigned to a fast versus a slow target-programming condition; although subjects were blind to this manipulation, those in the fast condition had rated the experiment significantly more favorably than those in the slow condition.

The only studies which do not show this trend when they might be expected to do so are two experiments using emotionally arousing targets that showed no relationship between variance and anxiety (Ballard, Cohee and Eldridge, 1983 [Study 2]; Ballard, 1980). This trend is remarkably consistent with Carpenter's (1977) prediction from the earlier subject-variance literature that, among other conditions, large variance will be produced by those subjects who find the task emotionally involving.¹⁰

High variance from subjects favorably disposed to the task would seem inconsistent with goal-oriented theories that rely on the motivated-error interpretation of missing. If missing is motivated, then we would predict that subjects with favorable attitudes toward the task would hit. While some subjects in these high variance studies are producing large positive deviations, others are producing large *negative* deviations. Thus, these studies support the conclusion drawn above that motivation is not an adequate explanation for psi-missing. If the goal-oriented models are to explain below-chance scores, they will need to do so by other means.

Some parapsychologists have proposed such a means through a radical reinterpretation of the meaning of hitting and missing. Rather than considering below-chance scores as some sort of error (motivated or otherwise), missing is seen as an aspect of psi itself, a manifestation of psi's "balancing function" (Rao, 1978b), or principles of homeostasis (Carpenter, 1983) or symmetry (e.g., Heseltine and Kirk, 1980).

Such a view is attractive for it makes the problem of how to interpret missing a "nonproblem" in much the same way that uniting clairvoyance, telepathy, precognition and psychokinesis under the rubric "psi" made the search for distinctly separate mechanisms a less troublesome issue. Further, it unifies certain trends that might otherwise seem inconsistent. For example, of the ten studies since 1978 which reported psi-missing as an effect of chronological order, seven showed missing-to-chance "declines" (Delaney, 1982; Irwin, 1982; Kennedy, 1979b; Kennedy and Haight, 1978; Heseltine and Kirk, 1980; Krieger, Solfvin and Roll, 1979; Solfvin, Krieger and Roll, 1979), whereas only three showed scores declining into significant missing (Lowry, 1981; McLaren and Sargent, 1982b; Rao and Kanthamani, 1980b). Under a magnitude-based model this trend is consistent with the large body of data showing hitting-to-chance declines. A magnitude construct of psi would also explain the results of Kobayashi, Terry and Thompson (1979), who found similar EEG responses during the ESP test among a group of subjects whose scores deviated significantly from chance (high variance), but a different response for subjects whose scores were close to chance (low variance).

However, the construct also has disadvantages. By obscuring the distinction between hitting and missing we radically alter our concept of psi in a way that removes it further from the life experiences we are trying to understand. We also run the risk of disregarding important information about the controlling mechanisms of psi if we do not take scoring direction into account.

Conclusion

This paper is a review of current research in an attempt to address a current theoretical issue. If psi is omniscient, we may never be able to make sense of parapsychological experiments. One way to discover limits to psi is to find situations in which psi makes mistakes. These mistakes challenge the notion of psi's omniscience unless we assume that the errors are not really errors, but only seem so because of our limited understanding of psi.

There are two views as to how scoring distortions may not necessarily be errors. The first and most dominant one in the field is that such distortions act to fulfill the psi source's true purpose. This assumption is justifiable only when we have clearly defined criteria that allow us to infer that purpose; otherwise, the assumption is unfalsifiable and serves to protect our notion of psi's omniscience. The second view is based on an alternative construct of psi which eliminates the need to

explain below-chance scores. While this construct is fascinating and has led to interesting lines of research, I feel that it is premature to completely abandon the examination of psi missing. If errors constitute a crack in psi's omnipotence, as I am suggesting, then we would not want to pass up the opportunity to peak inside. What we see may tell us what sort of research questions are important or it may tell us how our assumptions about psi have unwittingly served to maintain our belief in psi's omniscience in the face of contradicting information. In either case, the process of exploring these issues can only lead to empirical and theoretical advances.

NOTES

1. I shall use the term "psi source" to refer to that system which in some way is responsible for the psi effect. For ease of exposition, "psi source" is anthropomorphized and is used in the singular. This usage does not imply any theoretical position on the questions of the range of possible sources or of multiple sources.

2. The term "goal oriented" has two slightly different connotations which have sometimes been combined or used interchangeably. Strictly speaking, the hypothesis of psi's independence of task complexity states only that psi works efficiently; that is, that effects occur on the final outcome ("goal") of an indeterminate process no matter how many intervening steps there may be. What a person *desires* that outcome to be (i.e., the subject's "goal") is a slightly different concept. The distinction between these two meanings is easily seen by considering the case of a PK competition between two subjects in which for the same random event, one is trying to obtain a hit and the other is trying to obtain a miss. Both are attempting to influence the same "goal" (the random event) but *their* "goals" (hit or miss) are opposed. This distinction is important when we consider the role of motivation in goal-oriented models.

3. When low scores are the assigned task, as in a low-aim test, significant above-chance scores constitute "missing." However, low-aim tests are rarely administered in parapsychology. For clarity, I will speak of missing as below-chance scores.

4. In parapsychological usage, "displacement" refers to two different ESP effects. In a forced-choice test, displacement means that the subject's calls systematically correspond not to the intended target, but to the one prior to it (-1 displacement) or following it (+1 displacement). In a free-response test, displacement means that the subject has obtained information not about the actual target, but about one of the nontarget alternatives in the judging set. The fact that the same term is used to refer to both effects does not necessarily imply that they are the same phenomenon.

5. The review covers experimental research reported or abstracted in English or Spanish from 1978 to the time of this writing from the following publications: *European Journal of Parapsychology*, *Journal of the American Society for Psychical Research*, *Journal of Parapsychology*, *Journal of the Society for Psychical Research*, *Parapsychology Review*, *Research in Parapsychology*. Details for studies to be published in *Research in Parapsychology 1983* were not available. Not all studies showing psi errors are discussed in this paper, but I have tried to include as many of those bearing on these issues as possible.

6. I am referring here to subjects' *individual* preferences (Crandall and Hite, 1983; p. 216), rather than group preferences, as the former is more appropriate when considering individual performance.

7. Edge (1982) later raised the possibility that the results of this study may have been artifactual. The point here has less to do with the actual results than with how they have been interpreted vis-a-vis the conformance behavior model.

8. There is some question as to at what stage in the process this displacement/missing

occurs, since it is sometimes seen in the scores of certain judges only (Palmer, Bogart, Jones and Tart, 1977; Thalbourne, 1980). Whatever causes this phenomenon, then, may be related to the judging process as well as to the subject's imagery process. This conclusion is consistent with the observational theories of Schmidt and Walker which consider the target/protocol comparison (i.e., judging) the crucial step in free-response ESP. However, as inter-judge reliability in free-response tests is usually rather low and so little is known about the judging process, it is premature to point to any particular cause of different results among judges.

9. A study with overall significant hitting or missing can be significant on a variance analysis as well. No study was included in this section if the variance analysis was clearly redundant of directional scoring.

10. This trend is interesting and deserves some comment. One possible explanation is that individuals are relatively consistent in their scoring direction, but need certain conditions to magnify their deviations from chance. In other words, people may be identified as "hitters" or "missers" and when they are tested under conditions which they enjoy, their psi is elicited. While this explanation may be supported by the correlation of scoring direction with personality traits (e.g., extraversion) reliability of psi scores is generally low and it is unlikely that a subject would consistently score in the same direction over many tests.

Crandall and Hite's (1983) observation of displacement and missing on direct targets for sheep and subjects working in a preferred task may be relevant here, for it suggests that subjects who look favorably on a task (either because of their belief in psi or their enjoyment of a task) might displace their calls onto other targets and get lower scores on direct targets. If this is occurring in other experiments, it might explain why some subjects produce negative deviations under favorable conditions.

This trend of large deviations under favored conditions is partially consistent with Palmer's suggestion (1978, 1982), based on extensive reviews of the ESP literature, that spontaneity and hypnagogic states influence magnitude of scoring whereas social-psychological variables influence direction. The trend toward large subject deviations under favored conditions supports Palmer's observation if we assume that subjects may behave more spontaneously in a situation they enjoy. On the other hand, the trend also contradicts his suggestion, since he noted that belief and anxiety affected scoring direction whereas in these studies these factors were related to magnitude. Perhaps the clue to this discrepancy lies in the fact that the present review includes PK studies and that it restricts its attention to only those experiments in which scoring direction was inconsistent across subjects in the *same* experiment and does not consider, as Palmer did, similarities across studies with high *overall* scoring, but which in some experiments showed hitting and others missing. Thus, we might consider not only those factors that affect magnitude and direction, but those that make for consistent scoring across subjects and those that do not. Interestingly, recent studies which analyzed for *run-score* variance did not necessarily show high variance under preferred conditions, which suggests that an additional set of factors may be responsible for within-subject directional fluctuations.

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DISCUSSION

PALMER: I much appreciate your providing a perspective on this omniscience issue, that whether or not psi is omniscient in principle, it is not omniscient in practice. There are plenty of studies in the literature which demonstrate the significant differences between psi in experimental conditions and control conditions. Sometimes that represents a reversal in directions and sometimes it does not. A lot of our process-oriented literature is based on the assumption that psi in practice is *not* omniscient. I think that is an important distinction to make and I am glad you made it.

Secondly, I am one of those people who is very intrigued with the homeostatic types of models. I have been seeing a lot of that kind of thing in my own data. But I do not think that they should be construed as solving any problem or putting us in a position where we can say there is nothing left to be explained, that psi-missing is a non-problem. I think that is quite spurious. The problem seems to me to be the same as in the other approaches, that is, where in the other approaches we have to explain why psi cancels, why it conforms to a homeostatic principle. I think the problems are isomorphic in the two approaches.

WEINER: I see the point, but I think that it is very hard to test these ideas. Rao has tested his volitional model, but we do not yet have ways to test other types of symmetry models. I am thinking of such theories as Eisenbud's, in which he says that psi cannot disrupt Nature too much; if there is too much disruption, then there is going to be a rebound. How one would go about testing that, I am not sure. I do know of one attempt to do so, an unpublished manuscript by Leonard George, which was quite creative, but he acknowledges that he had to make quite a few assumptions in his approach. On the other hand, if you say that psi-missing occurs because the subjects have a particular attitude about the test, you can then develop some ways to look at that. So a homeostasis model may not make psi-missing a non-problem, but I do not see many attempts to try to test such models.

PALMER: That may be possible to do, we just have not given it enough thought.

ROLL: In trying to figure psi out, we have customarily made a distinction between the reception process and the "transmission process." Several people in the field have found a similarity between the characteristics of ESP as a reception process and the characteristics of subliminal perception. For instance, in both cases there may be "missing" as well as "hitting" the target and both cases may involve right hemispheric processing. Do you see any aid from that direction in reaching a comprehension of psi, particularly this hitting/missing situation?

WEINER: Yes, I do. I am not very familiar with the subliminal perception literature, but I do know that researchers have found "subliminal perception missing" and I think that that is very curious. Perhaps in such cases the stimulus levels are at such a low level that the task is essentially a psi task rather than a subliminal perception test. Further, there has been some work to see whether ESP-processing distortions, mistakes and so on, might be related to the kinds of things that we see in perception in general, symbolic distortions and other defenses. That was the reason for my looking at the relationship between cognitive processing errors and psi-missing. However, in the body of literature that I reviewed I did not see a lot of evidence for the idea, but I think that potentially it would be a way to go for ESP research. Now, whether that would extrapolate to PK I would not know.

MISHLOVE: Debra, I really think you should be applauded for trying to sort through a very thorny theoretical question. It is almost like the Gordian Knot, an insolvable problem here. This has been an issue for me for quite a long time and basically the way I have tried to deal with it for myself is a little more radical, but one which I would like to recommend to others in the field. There are certain lines of research which are in principle impossible to deal with in a rigorous way any longer. What we ought to think about, perhaps, is de-emphasizing that research in a major way and focusing our efforts on the kinds of research questions, such as the example you gave of tests of novel target systems, which are capable of yielding solid answers so that we do not pose questions that lead us into this problem area. I think applied psi research is one such area where we are not so much concerned with the process as with the result. I think the field can be advanced that way. I also feel that the problem that you have posed further warrants a systems-theoretical approach, such as Bill Roll has suggested, where we are not concerned with the psi source as being an individual, but as being the system around the experiment, including the environment, with all of the participants as subsystems. While process-oriented research is all very interesting, I have come to view it at this point as being only slightly more scientific than ethnographic studies. It is very likely that,

in spite of the kinds of consistent results that we have seen, such as the sheep/goat effect and many other consistencies that John Palmer among others has found in the research, we may be able to look back on that in the future and say it was all culture bound.

WEINER: I certainly would not disagree that experimental parapsychology can be considered a very specific subculture and subgroup and that the way we get our results would not necessarily generalize to other groups or even to the academic world at large. Your comment regarding looking at this problem through a systems-theoretical view and taking into account the different sources is a good one. It gets difficult, though, to actually put that into practice because of the kinds of problems I have mentioned. What is the system? Under the observational theories, for example, one ends up with some pretty ridiculously extensive systems. The problem occurs outside the observational theories as well. I want to stress that this is all on the theoretical level and not necessarily on the practical level. One approach that parapsychologists have taken to this whole dilemma, which I do not necessarily disagree with, is to basically ignore it, to continue searching for consistent results. This will obviate the difficulty of where the psi is coming from. The only disagreement I have with this view is that we should not do that, on the one hand and, on the other, speak of psi's omniscient properties without considering their implications. My paper is addressing the mismatch between the assumptions of what we are doing and what we are saying.

MORRIS: The way you phrase the problem is that as of right now many could regard psi as independent of intentionality, task complexity, space and time. That creates boundary problems which may be real or just apparent. If they are just apparent, it would be nice to know where the boundaries can be drawn. We need sharper concepts involved in framing the very boundary question itself, especially in the areas of intentionality. There we get into the place within the system where intentionality is going on. All of the research that you have cited can still involve a set of intentions by some of the people in the system. With regard to task complexity, goal-oriented does not imply goal successful. I think this is important. My own preference would be to say that the boundaries in these areas simply seem to be uncertain, not that they do not exist.

One of the specific problems that increases the complexity of this is that boundaries if they exist may be gradient boundaries as in sports, where, for instance, we do not know exactly how far somebody can ever hit a home run. It may differ amongst psi skills. It is possible there are separate psi skills with different boundaries. The idea of viewing

boundaries within a system hierarchy, as Jeffrey mentioned, is very important. We may be dealing only with boundaries as we construe them within our own particular level of organization.

Also, there are a couple of problems in looking at the individual studies. One is that we should not assume single sources in assessing studies. I have done this too much myself. We may be dealing more with something like the electoral college model, where there are multiple sets of sources of varying strengths. These uncertainties in boundary are exactly what one might find if, in fact, psi did not exist and we are dealing essentially with artifacts in our studies. Although I do not buy that, I do buy the possibility that many of the studies which give us problems may, in fact, be studies that suffer from either an uncertainty in psi sources or problems of conductance, analysis and interpretation.

WEINER: Regarding psi as independent of intention, one can consider different people's intentions. However, I like to think of it also in terms of intentions at different *levels*. For example, there is a subject sitting in front of a computer terminal, watching a line blip down the screen. He is watching the line and engaging in some kind of behavior—wishing, yelling at the line, or whatever. We can say that he may be operating at a level of intention that perhaps is the most direct or focused. On the other hand, the principal experimenter, who may not even be in the building, has just a general or diffused intention to produce a successful result. In parapsychology we have discussed intentions from different individuals, but not different types or levels of intentions. My discussion here is quite vague and needs refinement, but my general point is that we should look at characteristics or categories of intention as well as from whom the intentions emanate.

I agree with your comment about task complexity having similar kinds of problems. One of the things that is very apparent when you look at this area is that task complexity is defined in terms of the steps one would take to perform the task through normal means. For example, let us say that one is doing a blind PK test in which one is to roll dice so as to match a hidden target objective. Perhaps inside an envelope there is a piece of paper that says "6." One is supposed to try for sixes, but the person does not know that; all he or she knows is that he or she is trying to make the dice land to match whatever target is inside the envelope. This has been considered in parapsychology to be a more complex task than the standard PK test with a known target objective. It is considered to be a two-step process in which ESP is used to find out the target and PK to make the dice land according to that target. This view, that blind PK is more complex, assumes that psi

operates the same way *we* would if we had to accomplish the task through normal means; i.e., find out the target and then manipulate the dice accordingly. We really have no reason to assume this and to decide that what is a complex task to us is, if I may anthropomorphize, a complex task to PK.

I like your ideas about the gradient boundaries and separate skills. I do not have any specific comments about them other than saying that I think they are very interesting.

One last comment regarding your point that these would be the kinds of effects you would expect to see if psi did not exist. I agree with you that I would not wish to go that far, but I do sometimes wonder if psi's "elusiveness" is a by-product of our attempt to integrate into the literature all significant findings that we discover. Sometimes we are trying so hard to make sense out of the totality of experimental results that it leads us to a concept of omniscience.

ALVARADO: This is regarding the history of the concept of psi errors. There are previous discussions of this idea in the old literature. Examples are Hyslop's writings of problems in the process of mediumistic communications and Warcollier's discussions in relation to his telepathy experiments with drawings as targets, where some of the percipients showed distortions of the targets in their responses (he mentioned effects such as synthesis and fragmentation). Also, Geley paid attention to the occurrence and significance of defective materializations. All of them were focusing on mistakes, errors, to see how we can learn more about the psi process from them. The whole concept of boundary and omniscience is very relevant to the survival issue in terms of the super-psi argument.

WEINER: Exactly.

ALVARADO: I would say perhaps that the survival context is even a better example than experimental parapsychology in this sense, because you know you are using omniscience really to kill all phenomena that are suggestive of survival. Also, how would you generalize the type of thing you have been discussing to spontaneous ESP?

WEINER: I imagine that the persons involved in survival research are having the last laugh, because super-psi has now hit the laboratory.

There are difficulties in extrapolating my paper to spontaneous cases. We can, I think, propose psi-missing in life situations. Louisa Rhine has talked about this in terms of missing in an experiment being a type of information blockage that can also occur in spontaneous experiences. Her views were based on a certain assumption about spontaneous psi which I do not know if I agree with. That is that in a spontaneous experience, at some level the percipient is psychically aware of all in-

formation about the target event and any distortions or "holes" in the information they can consciously articulate are due to some kind of processing error or blockage. I do not know if you can assume that, since this position would presuppose psi's omniscience, albeit at an unconscious level. But we can think of situations of psi-missing in everyday life: invariably making wrong choices when driving to a location one is not sure about. But one of the difficulties, as Patric Giesler has pointed out to me and as I mentioned at the end of the paper, is that laboratory psi effects are starting to get further and further away from what seems to be going on in life experiences. For example, if it turns out that parapsychology suddenly takes off on these magnitude-based constructs of psi, we might be dealing with an interesting characteristic of randomness that would have important philosophical implications, but which might have nothing to do with what happens to people in everyday life. It is a problem and I am not sure how we can make that bridge.

GIESLER: First, I have a brief comment on the question of task complexity. Each subject may have his or her own perception of a task as complex or not complex. What I found in the field setting, for instance, was that certain illiterates took a look at a task that I clearly thought was very complex and another task that I gave them that I thought was simple and they felt the reverse, based on other conceptions of what is complex and what is not. Thus, I felt that the *perception* of complexity will have to be taken into account in future research on task complexity.

Next, I have a question. You are addressing the anomaly (psi errors), as it were, in our research. Historically, the dominant or long-standing theoretical constructs push aside the anomalies. This is one we have got to jump on to make progress. What kinds of directions do you think we should take to more clearly challenge the goal-oriented model? I think based on what you said so far, it would have to do with looking for errors that could not be interpreted as motive-based errors. How do we go about that? That would be the first question.

A second one has to do with the magnitude model. It sounds great, because we have got some reversals, we have psi-missing, we cannot understand it, but here is a model that says it is all part of psi. But then on the other hand, as I recall, there have been some studies where you can distinguish extremely low variance associated with unfavorable conditions and extremely high variance associated with favorable conditions. There we are back again to a hitting/missing type of paradigm. In a sense, low variance would be like an error or missing.

WEINER: I do not have any specific research designs in mind at pres-

ent. As those who attended the recent PA convention know, I just completed a study with Nancy Zingrone in which we got psi-missing and so I was put to the task of trying to interpret it. I think the best way for parapsychologists to go about this proceeds on two levels, a conceptual level and a practical one. On a conceptual level, if one is testing a certain hypothesis, one has to decide what results will be taken as a confirmation of the hypothesis. What I am suggesting is a more intricate process than just specifying that one is going to use a one-tailed or a two-tailed test. It means studying John Palmer's 1975 paper, mentioned in the text and *really* deciding on one's personal model of psi—what hitting means, what missing means, how do *I* as a parapsychologist conceptualize psi—almost as though one were examining one's personal philosophy, and then to determine what results will confirm the hypothesis. And, of course, one must then avoid the temptation to interpret what was obtained, if this differed from the specified pattern, as supporting the hypothesis. So this is what I suggest for a conceptual approach to the problem. On a practical level, one could try to anticipate what sorts of factors might be involved in missing in a particular experiment and try to measure these beforehand. An example of what I have in mind is the Debbs and Morris study that Bob talked about in his paper, in which they found that when people strove for success they got missing, but when they did not strive they got hitting. He did not mention this in his talk, but in the original report they had asked the subjects a number of questions about their competitiveness, perceptions of the task, perceptions of the instructions and so on. Through these questionnaire responses the authors were able to come up with a fairly logical explanation for why those subjects obtained the results they did. This was very important. An experimenter, of course, cannot anticipate everything beforehand, but if we can think of some of the things that might be informative should missing occur, then we can add them into the experiment.

Let me elaborate your second point for people who are not familiar with the magnitude idea. In an experiment in which one uses a magnitude of scoring as the measure of psi, subjects can obtain large deviations above or below chance or they can obtain small ones. It is possible to get scores that are too small, i.e., significantly closer to chance than chance fluctuation would predict. James Carpenter, in his chapter in Wolman's *Handbook of Parapsychology*, has discussed what this might mean. For example, it may be a cancellation effect, that hitting and missing is oscillating rapidly and they cancel each other out and produce a significantly low variance. Patric, I think you are right to say that we could end up right back in the ball park, except at a higher

level of abstraction, when we start talking about significantly low variance versus significantly high variance. In the literature that I looked at for this review, I saw only two cases of significantly low variance, so it can occur.

GIESLER: . . . and these cases of low variance were associated with negative motivation?

WEINER: Yes, they were.

GIESLER: So a certain motivational factor could almost come in?

WEINER: Right.

BENOR: I am thinking here as a psychiatrist who often deals with averages and yet has to deal with a specific person who has come to me for therapy. I know what he ought to do according to his diagnosis, according to what he has told me of himself and according to hundreds of other cases that are reported. But I still have to deal with that particular individual who has his own peculiarities and may behave differently. My moves may lead to an error in the process of what we both intend to achieve and I have to go back and study what was wrong. When you talk about the omniscience of psi, I think we also have to remind ourselves that we are not omniscient in making these hypotheses. We may need to go back to the drawing board when we encounter these errors and just rethink what we are doing from the beginning. I read Hoyt Edge's study of the plant, which I found absolutely fascinating. If you start to attribute sentience to a plant that can somehow perceive on some level that it can affect a light mechanism, then why not postulate the feelings of the plant as well. Maybe the plant was depressed, maybe it was angry, maybe it was incensed at being placed in the situation. Maybe we need to look at this type of error and move in totally different directions for further research.

WEINER: In Edge's paper he said, in effect: "Well, we know that goats miss and I know it sounds pretty silly to say that plants might have certain beliefs about psi, but let's be open-minded." Edge is a philosopher who comes to parapsychology with a particular metaphysic under which this idea is not unreasonable. My comment is that it strikes us as odd to attribute attitudes to a plant, yet perhaps some parapsychologists have attributed attitudes to their subjects, attitudes that "explain" psi-missing, without having really found out from their subjects if they had such attitudes.

SCHOUTEN: I think you address certain important problems, but sometimes I wonder, Debbi, whether or not many problems including some you discussed are not artificial and our own creation. Basically what we do in experimental ESP research is apply a concept approach.

Now that means we start out with a concept and then we start experimenting. Since you do not know anything about the concept, you attribute omnipotence to it and everything goes, so to say. I always wondered why people considered psi so omnipotent. When I look at what happens in daily life and in the lab I see only restrictions. I see it happen only rarely. Another factor contributing to the confusion is that strong extrapolations are made from scanty data. There is a measurement problem involved in what we are doing. Science involves measuring, that we relate between measurements. Now to measure psi is a very difficult problem. The best we can do is to say there is a significant effect or not. The way we measure other variables is very, very bad. With all respect to experimental parapsychology—I am myself involved in it—we know how bad the instruments we have are. So the moment you find some results, you have to be cautious. Most of our results are not repeatable. I think you really are in danger, if you take just one or two experiments which show a significant result and make a big jump and say for instance, like our last example, if Hoyt Edge found a correlation in the study of the behavior of plants, whether they behave or not, I think to use this to start a discussion about consciousness in plants, is to my taste going much too far. At least there should be many, many studies which show a relationship like that. We should be able to exclude experimenter effects and only after that, I think, do you have a basis to talk about things like that. I am a bit pessimistic, perhaps.

WEINER: I have a footnote in the paper referring to a later paper by Edge in which he suggests that his earlier results may have been artifactual. The point I wish to make in the paper is not whether plants really have these motivations, but rather the fact that here is a study that showed negative results, significant missing, that is interpreted in at least three instances in the parapsychological literature as though it were consistent with the conformance behavior model which would predict hitting. I am concerned primarily with the way we interpret experimental outcomes.

SCHOUTEN: There are a lot of problems in the field which I think are the results of using a concept and attributing something like omnipotence to it. We do not have to prove that the concept is not omnipotent. If somebody tells me psi is omnipotent, I want proof of that. Given such an inflated concept, if you then also extrapolate from results I think you get a sort of mess as in the observational theories. It just starts as a mathematical model and suddenly you find that while they use a symbol for psi, it turns out that this symbol has properties that

in fact create problems. I think we should start the other way around. That is, be cautious about our data, first get solid data and not make these big deals.

WEINER: I think we are in agreement on that point. What I was trying to do in this paper was pull the reins in a little bit on this idea of psi's omniscience, because, as I said before, I do not think parapsychologists really think that psi is so omniscient or else we would not be doing any research. Yet on the other hand we talk as though we do think psi has extraordinary power. So I was just addressing that mismatch. But I agree very much with your basic point regarding caution in the extrapolation of data.