

## The Challenge of Experimenter Psi

John Palmer

Rhine Research Center, Durham

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Abstract: The author suggests that parapsychologists in general have paid insufficient attention to the role of experimenter psi (e-psi) in parapsychology, arguing that the evidence dictates that the burden of proof should fall on those who maintain that e-psi is *not* at least a contributing factor in most successful psi experiments. E-psi is a special case of nonintentional and unconscious psi, which has received strong support from Stanford's PMIR model. E-psi is also a special case of the source of psi problem. Psi sources must be limited to account for what reliability there is between psi scores and human intentionality, and it is proposed that this limit be psychological involvement in the experiment as such. The standard interpretation of so-called fieldREG effects precludes such a constraint, and partly for this reason they seem best interpreted as e-psi. Recommendations are offered for assessing e-psi, which include orthogonal manipulation of the cognitive state of the principal investigator with behaviour of the experimenter toward the subject, and direct comparisons of personal psi-test scores between psi-conducive and psi-inhibitory investigators.

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### Introduction

There is no more reliable finding in parapsychology than the experimenter effect. Although there has never been a formal analysis, it is widely accepted, even by conventionalists, that some investigators have much better track records of obtaining significant evidence for psi and confirming their hypotheses than do others. It is a major source of variance in psi experiments.

Not counting the conventionalist argument that psi-conducive experimenters are sloppier or less honest than their less successful colleagues (for which there is virtually no evidence so long as the argument is applied exclusively to the fraternity of professional parapsychologists), there are two competing but not mutually exclusive explanations for the experimenter effect. The first is that some experimenters are better than others at putting their subjects at ease and inspiring confidence in task performance, either because the less successful lack the requisite social skills or they do not adequately apply them. I will hereafter refer to this as the *experimenter interaction* hypothesis. The second hypothesis is that

to varying degrees experimenters psychically influence their own experiments, either directly or by releasing the psi ability of their subjects. This is the *experimenter psi* (e-psi) hypothesis. [A third way experimenters might influence test outcomes is through selection of subjects (Morris, Dalton, Delanoy & Watt, 1995), but this is a matter of methodology that, in my opinion, should not be subsumed under the experimenter effect.]

Despite a compelling case for e-psi published over 20 years ago by Kennedy and Taddonio (1976), most parapsychologists have yet to give it the serious attention it deserves, even though they sometimes mention it in the discussion sections of experimental reports. This attitude expressed itself unwittingly in a symposium on the experimenter effect held at the 1996 Parapsychological Association Convention, which symposium served as the stimulus for the present set of papers. Although there was plenty of discussion of experimenter interaction variables, e-psi was barely mentioned at all. Having anticipated this allocation of priorities, I prepared a few remarks about e-psi that I delivered during

the question period. I am grateful to Deborah Delanoy, who chaired the symposium, for inviting me to expand on these remarks in the *Journal*.

The paper will include a discussion of the a priori arguments and empirical evidence for e-psi. Not only will I refute the argument (which I have heard expressed informally by more than one parapsychologist) that the e-psi hypothesis is objectionably ad hoc, but I am willing to go so far as to assert that the burden of proof now falls on those who would argue that e-psi is *not* at least a contributing factor in most, if not all, successful and methodologically sound psi experiments. I will then discuss possible mechanisms for e-psi and offer some thoughts on approaches we might adopt to address it in our experiments. But first, it is necessary to place e-psi in its broader context.

### The Source of Psi Problem

One of the most baffling conundrums confronting parapsychologists is the so-called source of psi problem. I define a source of psi as any living being who causes a psi interaction to take place, whether by acquiring information from the external environment, transmitting information to another living being, or affecting a physical object or process. The source of psi problem has traditionally been a major issue in research on post-mortem survival; for example, investigators have debated whether the source of evidential mediumistic communications is a discarnate entity or a living person with intimate knowledge of the deceased, as described by Gauld (1982). In later years, the argument was broadened to include discussions of whether the agent or the percipient was the most likely source of psi in cases of spontaneous telepathy (e.g., Hart, 1958; Rhine, 1957). The challenge presented by the source of psi problem was increased considerably by the discovery of precognition (Rhine, 1941). Persons unaware of the target identity at the time of the subject's response now had to be considered potential psi sources if they came to know its identity at some time in

the future, and it was never clear how far in the future that could be. Not surprisingly, the source of psi problem has never been satisfactorily resolved in any of its applications.

### Nonintentional and Unconscious Psi

It took a long time for these examples of the source of psi problem to be taken account of by experimental parapsychologists in their research. The first parapsychologist fully to appreciate its implications was probably Eisenbud (1963), whose insights have been further developed by Braude (1979). Until recently, it was almost universal practice implicitly if not explicitly to place a key restriction on the identity of the psi source: it had to be someone who was attempting to transmit or receive psychic information. This limitation is inherent in the very definition of the experimental subject, whose role is to attempt to produce psi at a particular time and place.<sup>1</sup>

This restriction of potential psi sources has always been implausible to the extent that credence could be given to the non-experimental literature of parapsychology. The term *spontaneous cases* highlights the fact that in the 'real world' ESP is ordinarily something that happens to people, not something they invoke. On the PK side, ostensible poltergeist agents generally claim that they were not attempting to create the mayhem attributed to them, and they seem to prefer (consciously) that it stop.

An important conceptual advance was made by Stanford (1974a) when he drew attention to anecdotal reports in which not only was there was no conscious attempt to

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<sup>1</sup> It is important to be clear exactly what is meant by the word *attempt*. It is not the same as *effort*. PK subjects might be told to adopt an attitude of 'passive volition' to bias the output of a random number generator (REG), but they are still attempting to exhibit psi, even if they are doing so with a minimum of effort. It can also be said that such subjects *intend* to produce psi, whether or not they exert effort to fulfill the intention. On the other hand, a person who is merely *hoping* for psi to occur would not be defined as *attempting* to produce it.

acquire information by ESP, but there were no relevant cognitions at all. Many such cases involve fortuitous timing, as in one of Stanford's examples where a couple seeking a restaurant for dinner happened during lunch to overhear the conversation of an adjacent party describing just such a restaurant, including its location. Assuming such cases are not chance coincidences, they describe a kind of psi that is both *nonintentional* and *unconscious*.

Stanford developed a model called Psi-Mediated Instrumental Response (PMIR) that translated these insights into a set of experimentally testable propositions. Subjects who were not even aware they were involved in a psi experiment completed a word-association test in which certain randomly selected response alternatives, if selected, would cause them or a partner subsequently to be assigned either a pleasant or unpleasant task (e.g., Stanford & Associates, 1976). In the methodologically simplest PMIR experiment, which differed somewhat from the above description, subjects could escape from a boring task whenever the output of an REG met a specified criterion (Stanford, Zenhausern, Taylor & Dwyer, 1975). Results revealed that on average the REG met this criterion significantly sooner than expected by chance.

The PMIR model also made specific predictions about what factors should influence this nonintentional and unconscious psi. Six of six predictions of this type were confirmed with respect to the direction of the effect, and three of the six were statistically significant (Palmer, 1985).

Data such as these force us to broaden our definition of who is a potential psi source in parapsychological experiments. No longer can we assume that consciously attempting to produce psi or having some experience of psi is necessary for a psi effect, yet these are the only attributes that distinguish subjects from other participants in most psi experiments. Does this mean that there are no limits at all on who is a psi source? I suppose this is theoretically possible, but if a large number of people were influencing every psi experiment, each in

their own way (which we must assume given the variety of different kinds of psi effects), the expected result would be chaos in the data. Although psi is admittedly 'elusive', the data show sufficient reliability and covariance with the intentions of particular individuals that this pessimistic prospect can be rejected. There must be limits, but what are they?

### **Psychological Involvement and Experimenter Psi**

As for possible physical limits of psi, there is little evidence that it is affected by distance (Palmer, 1978), and the evidence for precognition suggests that psi can operate in the future, although it weakens as the time interval between event and cognition increases (Honorton & Ferrari, 1989). A far better candidate is offered by the Observational Theories (Millar, 1978), which are based on quantum mechanics. According to these theories, potential psi sources are limited to persons who have sensorially observed the data from an experiment, although it is not entirely clear exactly in what form the data must be for the observation to be effective: must observers see the *raw* data, or is it sufficient to observe the statistical summary of the data, or even a reference to the data in a literature review? These issues aside, the Observational Theories are a serious candidate for dealing with the source of psi problem. However, their truth is not considered by most parapsychologists to be established, and I am not persuaded of their truth myself, partly for reasons to be discussed later.

As Rao (1966) and Schmeidler (1988) have pointed out, the variables that have most clearly (although not conclusively) been shown to influence psi are psychological rather than physical.<sup>2</sup> They cite a num-

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<sup>2</sup> Some very recent evidence suggesting a correlation between free-response ESP and geomagnetic field fluctuations in conjunction with local sidereal time (Spottiswoode, 1997) could prove to be the discovery of another physical mediator of psi.

ber of psychological variables that correlate with psi success, but these are not quite the same as limits. Until better data are available, I propose that we postulate just one psychological *limit* of psi in our experiments, namely that a psi source must somehow be *psychologically involved* in the experiment. Although I am not aware of any hard evidence for this assumption, it is plausible and I can find no evidence against it; that is to say, I am aware of no evidence that someone *not* psychologically involved in the experiment has ever been a psi source. This assumption immediately eliminates the vast majority of living beings as psi sources in any given experiment, something we must do if the source of psi problem is ever to become manageable. These potential psi sources overlap somewhat with those proposed by the Observational Theories because observers of data are most often persons psychologically involved with the research, a possible exception being low-level research assistants. Conversely, there may be persons psychologically involved with a study who never observe the data, at least in its raw form.

I intentionally restricted my limitation to psi *experiments*, because I am not sure it applies to spontaneous ESP experiences. There obviously are cases in which people gain psychic impressions of external events that are totally unrelated to them. The example that comes to mind are premonitions of disasters, like airplane crashes. If the problem population could be restricted to disaster cases, the argument could be made that the psi sources are the victims, who, the hypothesis would need to assume, are 'calling out' to 'anyone out there' in the midst of their plight. (Obviously, we cannot define the psychological involvement of telepathic agents simply as involvement in their immediate circumstances; by that criterion we would all be potential psi sources all the time and the limitation would be meaningless. We must assume some conscious or unconscious intent to communicate, even if to just anybody.)

Psychological involvement still allows a number of persons other than the subjects to

be psi sources in any given experiment. The most obvious example is the experimenter. However, the preceding discussion dictates that the experimenter, if defined as the person who interacts with the subjects, does not exhaust the possible psi sources. For example, if we agree that psi is not limited by distance, the psi source could be the principal investigator, who might be in an office three doors down the hall or even at home when the experimental session is being conducted. If we agree further that psi is at least somewhat independent of time, the psi source could be a data analyzer who does not become involved in the study until after the data have been collected. For ease of exposition, I will broaden the definition of experimenter in this paper to include these other possible sources unless otherwise stated.

It is reasonable to suppose that not just the fact of psychological involvement, but also the *intensity* of involvement, is a factor in determining the degree of psychic influence. Intensity of involvement is often reflected in the *need* of someone to achieve a certain experimental outcome. Interestingly enough, need is included in Stanford's model as one of the factors determining the strength of PMIR<sup>3</sup>, and empirical support exists for this proposition (Stanford & Associates, 1976). In many experiments, particularly those with unselected subjects (who may participate merely out of curiosity), the need for success might be much greater for the experimenter than for the subjects.

The reader may have noticed a certain paradox in my reasoning. I cited the results of subjects in PMIR experiments to support the notion of e-psi, but to accept this interpretation implies that the subjects may not have been the psi source in these experiments. In one of them (Stanford *et al.*, 1975), confirmation of the hypothesis depended on the scores of subjects tested by just one of two student experimenters. Because this experimenter was more extro-

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<sup>3</sup> In a revision of his theory, Stanford (1990) subsumed 'need' under the broader term 'disposition'.

verted than the other one, Stanford speculated that she was successful because of superior social skills. However, there is evidence that extroverts also might have more psi ability than introverts (Honorton, Ferrari & Bern, 1990), so Stanford's secondary finding could also be taken to support e-psi. But this 'evidence' regarding extroversion and psi could itself be attributable to e-psi, thereby rendering it worthless as support for e-psi in Stanford's experiment. No wonder thinking about e-psi can lead to vigorous hair pulling!

Some comfort can be derived from the likelihood that subjects contribute at least a portion of the psi in most successful psi experiments. All other mental faculties are broadly distributed in the population, even allowing for small extreme subpopulations such as idiot savants. It would be surprising if psi were any different. Nonetheless, the evidence for subject psi in studies with unselected subjects is far from conclusive. Even with selected subjects, e-psi might be necessary to release the psi of the subject. For example, the gifted subject Bessent provided straightforward evidence of intentional psi only in studies in which Honorton was involved as one of the experimenters (Honorton, 1971; Honorton, 1987; Krippner, Honorton & Ullman, 1972, 1973; Krippner, Ullman & Honorton, 1971). It is even possible, although in my opinion unlikely, that Honorton was the sole psi source in these experiments.

### **Empirical Evidence for Experimenter Psi**

In addition to the a priori arguments, there is a growing body of empirical evidence for e-psi. The best evidence comes from studies in which the experimenter does not interact directly with the subjects, thereby eliminating experimenter interaction as a counter-hypothesis. The classic example is the mail-correspondence ESP study by West and Fisk (1953), in which target packs assembled by Fisk (a reputedly psi-conductive experimenter) led to significant hitting, whereas those assembled by West (a reputedly psi-inhibitory experi-

menter) produced chance results. One might also place in this category studies confirming the so-called *checker effect*, in which results covaried with who analyzed the data (e.g., Weiner & Zingrone, 1986). For a good review of the evidence for e-psi up to the mid-1970s, see Kennedy and Taddonio (1976). I also discussed e-psi in an earlier paper of my own (Palmer, 1993).

### **FieldREG effects**

A recent group of experiments that also did not involve subject-experimenter interactions were designed to explore what I will call fieldREG effects (Bierman, 1996; Nelson, Bradish, Dobyns, Dunne & Jahn, 1996; Radin & Rebman, 1996).<sup>4</sup> In each experiment, one or more REGs were activated at a time corresponding to some event in which a number of people were focusing attention on the same thing, usually with a great deal of interest or emotional involvement. The hypothesis was that during the event, or more precisely during especially captivating epochs within the event, a sort of psi field was created that biased the output of the REGs.

Although the authors of all these studies acknowledged e-psi as a possible counter-explanation of their generally significant results, I find that a stronger statement on its behalf is warranted. The clearest example is an experiment by Radin and Rebman (1996), in which an REG was activated during a Las Vegas comedy show attended by two members of Radin's research staff. These investigators noted times during the show that were especially captivating and, sure enough, these were the periods during which the REGs behaved nonrandomly.

However, Radin and Rebman's results present a serious conceptual difficulty for the field hypothesis. Along the famous Las Vegas 'Strip' are a number of hotels in close

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<sup>4</sup> Different investigators describe this body of research differently. I will use the term introduced by Nelson *et. al* (1996) because, so long as 'field' is taken to mean the location of the REG, it provides a concrete and theory-neutral label for the effect.

proximity to one another that present nightly shows of top-rated entertainment, each of which surely creates various periods of intense audience involvement. The problem is that there is no reason to suppose that these intense periods at the different hotels are temporally synchronized. The audience at the comedy show attended by the investigators might react to a hilarious joke at 10:15, whereas an audience at another hotel that is relatively unfocused at 10:15 might be engrossed in a spectacular magic trick performed on stage at 10:33, when the comedy routine is in a lull. The only reason that the REG output covaried with the ebb-and-flow of the comedy show and presumably not the magic show (and/or several other shows in town) is that the investigators went to the comedy show. The alternative explanation that the effect occurred at the comedy show because that is also where the REG was located is ruled out because the effect is known from other studies (including some reported in the Radin and Rebman paper) to occur when the REG is located some distance from the putative psi sources. Thus, it is most likely that at least one of the experimenters was the psi source in this experiment.

The e-psi interpretation might seem less compelling in those cases where the event is witnessed by such a large group of people that contamination by equally large groups during control periods is unlikely. Examples chosen by the investigators include the 'Super Bowl' championship game of professional American football and the notorious O. J. Simpson trial. However, because e-psi is a viable explanation for all the fieldREG studies and the field hypothesis for only some of them, e-psi is preferable for reasons of parsimony. Moreover, the effect sizes in the large group studies appear comparable to those of the small group studies (one might expect the effect size to be greater when more people are focused on the same thing), as well as laboratory studies by the same investigators (Bierman, 1996).

The authors of all three published sets of fieldREG experiments suggested problems with the e-psi interpretation as applied to

their data. Radin and Rebman (1996) actually conducted a control experiment designed to test the e-psi hypothesis. Although the experiment provided results that seem to support Radin and Rebman's hypothesis, it focused exclusively on retroactive PK, which (as we shall see later) is only one of several mechanisms by which e-psi could manifest, and arguably not the most likely one. Moreover, if the Observational Theories (which supply the primary theoretical basis for retroactive PK) are true, Radin and Rebman's field hypothesis would have to be false, because the audience never observed the REG data. This same point applies to all the other fieldREG studies conducted so far.

Nelson *et al.* (1996) noted that their design allowed for persons not associated with their laboratory to 'install and operate the fieldREG equipment'. Such persons are indeed unlikely psi sources because of their lack of psychological involvement, but it follows from the above discussion that investigators more psychologically involved with the experiment need not have been present during the sessions or have interacted sensorially with the equipment to exert a psi influence. Bierman (1996), who came closer than the other authors to endorsing an e-psi interpretation, nonetheless made reference to the fact that in one of the two field studies he reported (a poltergeist case), the significant results were in the opposite direction from that expected by the experimenters. But significant reversals of hypotheses are not that infrequent in parapsychology, especially among investigators who fall in the midrange of psi-conduciveness. In any event, this reversal of direction is even less congenial to the field hypothesis, which has always predicted that the REG should be biased in the direction of increased 'coherence' (which it was not in the poltergeist case).

If the field hypothesis is true, one wonders why REGs do not more frequently produce identifiably biased outputs during randomness checks. Intense group focusing is likely to occur at some place(s) within the boundaries of the hypothesized field (except possibly late at night), and most

analysis programs can detect the squared deviation effects that are most commonly the dependent variables in fieldREG experiments.

The implications of the field hypothesis go much farther than REG randomness checks. Indeed, if the field hypothesis were true, we should expect a tremendous amount of nonrandom 'noise' in all our psi data. This includes ESP data, because even if Decision Augmentation Theory (May, Utts & Spottiswoode, 1995) does not carry the day, the mechanisms of ESP and micro-PK are likely to be closely related. The specific reason for expecting such noise is that the field hypothesis contradicts the assumption that psi sources are restricted to persons psychologically involved with the experiment. As I argued previously, I think that a much more restrictive assumption must be retained in order to explain what order we do see in psi data. If we retain such an assumption, the field hypothesis must be rejected.

The field hypothesis and the e-psi hypothesis converge in one important respect: each assumes that the psi can be both nonintentional and unconscious, as defined above. In fact, the fieldREG studies provide some of our best evidence that psi can operate in this manner, thereby confirming the foundation on which the e-psi hypothesis is built. From the theoretical standpoint, the reason for preferring the e-psi interpretation of these studies is that the experimenters are much more psychologically involved in the *experiment* than are the groups.

I understand that both Nelson and Bierman have conducted or plan to conduct more sophisticated studies designed more incisively to discriminate between the e-psi and field interpretations of the fieldREG effect, and we will have to take a second look at things after these data have been published. In particular, it is important to note that the two hypotheses are not mutually exclusive: experimenters and audiences might both be psi sources in a single study. Nonetheless, at some point it will be necessary for proponents of the field hypothesis to address the logical points raised in the

preceding paragraphs. For now, it is safe to say that Radin and Rebman's Las Vegas entertainment experiment, at least, provides empirical evidence for e-psi, even if there were additional psi sources.

### ***Experimenters as subjects***

An indirect but nonetheless important kind of empirical evidence for e-psi comes from studies which demonstrate that experimenters who are successful in eliciting psi from others are also highly successful as subjects themselves (e.g., Honorton & Barksdale, 1972; Radin, 1988; Schlitz & Haight, 1984). Such data suggest that these experimenters have the ability, at least, to contribute psi to their experiments. In a remarkable report, Schlitz (1987) described interviews she conducted with three psi-conducive experimenters, at least some of whom admitted that they intentionally entered psi-conducive states of consciousness during their experimental sessions. This sort of behavior makes e-psi a particularly tempting hypothesis.

### **A Personal Anecdote**

At this point, I would like to interject a personal anecdote that I must confess has influenced my personal evaluation of the e-psi hypothesis. Much has been made of how well the late Charles Honorton treated his research subjects, and this has often been cited as a major reason for his success in eliciting psi from them (e.g., McCarthy, 1993). Several years ago when I was living in California, I paid a brief visit to Honorton's lab in New Jersey as part of a trip to the east coast of the U. S. Although in many ways Honorton was a warm and congenial person, it is well known that he sometimes could be nasty with people if they did something he did not like (McCarthy, 1993). In my experience, this manifested as short, cutting comments that could occur at any time. As I anticipated I might be on the receiving end of one or more such comments, I approached the visit with mixed feelings.

Shortly after I arrived I served as subject in a ganzfeld session. Although I did not encounter any cutting remarks before the session, I did not expect them to occur then, and I was still apprehensive that they might occur later. The point, of course, is that I was not in the state of ease and comfort that was supposed to be key to Honorton's success.

As the reader has probably guessed, I obtained a direct hit. To my mind it was also an impressive hit, and I had at least two vivid images that were directly related to the target. Compared to other ganzfelds I had experienced, I found both the quality of my imagery and its correspondence to the target unusual.

However, what made the experience truly memorable was that two of my most distinct images that did not relate to the target were matched by sensory images I experienced shortly thereafter. The first image was of a cowboy riding a horse standing on its hind legs. It matched precisely a scene I encountered in the movie on my flight back to San Francisco that evening. The second image was of a sphere consisting of alternating red and white crescent-shaped wedges that I interpreted at the time as a beach ball. The day after I arrived back home I visited Golden Gate Park in San Francisco for the first time. Shortly after I arrived, I noticed some paper globes or lanterns hanging outside in an oriental exhibit. They looked exactly like the 'beach ball' of my ganzfeld session.

I am a hopelessly poor imager who almost never has precognitive impressions of any kind, either awake or in dreams, so I found this whole series of events quite extraordinary. Rightly or wrongly, I have always felt that some psychic input from Honorton was at least partly responsible for 'my' sudden outburst of psi. (By the way, the dreaded cutting remark never occurred during the visit.)

### **The Mechanism(s) of Experimenter Psi**

By what mechanism(s) might e-psi express itself? In PK experiments, the mechanism seems quite straightforward.

The experimenter does exactly what he or she asks the subject to do, except the experimenter does it nonintentionally and unconsciously. I have never fully understood why retroactive PK experiments (e.g., Schmidt, 1976) are interpreted as providing strong evidence for the Observational Theories, when a much simpler explanation is that the experimenter nonintentionally and unconsciously biased the REG output at the time it was recorded on the tape, before it was observed by the subjects. Although it is true that 'retroactive PK' effects were predicted by the Observational Theories, these same effects (objectively defined) follow just as readily from the e-psi hypothesis.

The possible mechanisms in ESP experiments are of necessity more complicated, but not exceedingly so. The most attention has been paid to the possibility that an experimenter might bias the supposedly random selection of targets by means of PK. This is especially likely when the targets are generated by an REG. It is noteworthy in this connection that in Honorton's automated ganzfeld experiments (Honorton *et al.*, 1990), one particular target pack was selected by REG much more frequently than expected by chance (Bierman, Bern, Berger & Broughton, 1996). Although this fact in no way invalidates the evidence for psi from these studies, one wonders if it might represent an e-psi effect. It would be interesting to know if Honorton or one of his experimental associates had a particular affinity for one or more targets in this pack. It must have had some salience for Honorton, because he selected it as the single target pack to be used in one of his later ganzfeld series (Honorton *et al.*, 1990). Other parapsychologists have proposed ingenious ways to frustrate psychic target selection bias by minimizing the effect of psi-influenceable 'random' decisions on the process (e.g., Stanford, 1981). However, such decisions are never eliminated entirely (if they were, the method would not be valid), so these procedures, worthwhile as they may be, are unlikely to eliminate e-psi. This is especially true if one accepts the no-



tion that psi is 'diametric' (Foster, 1940) or 'goal-oriented' (Kennedy, 1978).

Target selection is not the only possible vehicle for e-psi in ESP experiments. A more likely mechanism, in my opinion, involves two stages. First, the experimenter nonintentionally and unconsciously acquires the identity of the target by clairvoyance or precognitive telepathy. Second, the experimenter nonintentionally and unconsciously sends this target information telepathically (i.e., by Stanford's (1974b) MOBIA) to the subject, who is unaware of the source of the information. Although this process might be considered unparsimonious in the sense that it requires two steps, each step is no more demanding than what we attribute to subject psi in the same situation.

Finally, some approaches to explaining psi, such as Stanford's (1978) Conformance Behavior Model, postulate that there is no mechanism at all involved in the acquisition of psychic information, as the term mechanism is commonly understood. For example, the brain simply 'conforms' to the state of the REG. If we adopt such a non-mechanistic approach, any conceptual difficulties in accounting for ESP by e-psi recede even further.

### **Some Methodological Approaches for Addressing Experimenter Psi**

As is true for the source of psi problem generally, there is no way conclusively either to confirm or refute the presence of e-psi in an experiment. However, steps can be taken to estimate, and to some degree control, its likelihood. Even these more modest objectives are difficult to achieve. The best I can do is offer some general strategies that researchers might consider. All of these strategies assume that psi performance is influenced by certain cognitive and motivational variables, and their viability rests on the validity of these assumptions.

### ***Cognitive factors***

First, it is necessary to be clear about one's objectives. If the goal is simply to maximize psi and the researcher doesn't care who the source is, he or she should follow the lead of the psi-conducive experimenters interviewed by Schlitz (1987) and attempt to enter a psi-conducive state during the session. This could even go to the point of actively attempting to influence the outcome, although a passive 'hoping' might actually be more effective (Debes & Morris, 1982; Honorton & Barksdale, 1972; Palmer, 1996). Conversely, if the goal is to eliminate e-psi, the researcher should avoid deliberately entering a psi-conducive state during the session. This might be a good time to analyze data from another experiment or carry on an intellectual discussion with a colleague. Paradoxically, intense focusing on the session might have the desired inhibitory effect, but the researcher would need to maintain that focus throughout the session as well as immediately afterwards, to minimize possible release-of-effort effects (Stanford & Fox, 1975). Whether the goal is to facilitate or inhibit e-psi, it is obligatory that the researcher frankly acknowledge in the experimental report exactly what was done.

The best way for researchers to determine how to facilitate or inhibit their own psi in experiments is through self-testing with the same psi task to be given to the subjects. It is reasonable to suppose that the states which have the desired effect in self-testing will have comparable effects in experiments. To maximize the benefits of this approach, researchers should explore their psi performance in a variety of different states.

The third objective, which I would like to see more investigators adopt, is to assess the effect of e-psi as distinct from other factors such as experimenter social skills. The ideal way to achieve this objective is through experimental manipulation. For example, a researcher might implement a design in which two variables are manipulated orthogonally. The first is the experimenter (narrow definition) - subject

interaction. The researcher would need to recruit a tester who has psi-conducive social skills as best these can be currently defined. It would be desirable for such persons *not* to perform well themselves on the relevant psi task. A good method actor or actress is a possible choice. In one condition, this experimenter would maximize efforts to put subjects at ease, make the experiment fun, inspire confidence, etc. In the other condition, the *same* experimenter would minimize these skills, but not to the point of being rude or inspiring a lack of confidence. Not only would this latter approach fly in the face of how parapsychologists think subjects should be treated, but it would undercut the generalizability of the finding to well-meaning psi-inhibitory experimenters. The goal should be to do the 'right thing', but ineptly. The second independent variable would be the mental state of the *researcher*, the person who designed the study and presumably has the strongest stake in its outcome. In one condition, this person would enter a psi-conducive mental state whereas in the other condition he or she would not. The researcher should not be informed of the condition assigned to the tester, or vice-versa.

### ***Motivational factors***

Although cognitive states such as those discussed above are relatively easy to manipulate, the same cannot be said for desires and expectancies, which also could be expected to influence psi. Unless I am a very good hypnotic subject undergoing hypnosis, I can tell myself all I want to that I will like Brussell sprouts, but if I don't, I won't. Likewise, if I don't expect to do well on an exam, telling myself that I will is unlikely to make a difference, and my grade will still suffer. This could be a major factor that distinguishes psi-conducive and psi-inhibitory experimenters as their careers progress. Psi-conducive experimenters develop a track record of success that gives them confidence for their next study, while just the opposite happens for psi-inhibitory experimenters. This factor would be expected to impact both the likelihood they

will be able to communicate confidence to their subjects and the likelihood they will contribute their own psi to the outcome.

The only reliable way to address these motivational factors is through a correlational approach. For example, experimenters might keep track of their moods at each session and see how these ratings correlate with psi scores. Unfortunately, desires regarding outcome are likely to remain constant during an experiment, and this lack of variability dooms a correlational strategy. An exception might be if during the course of the study the experimenter sees the results reversing the hypothesis and thus hopes for this trend to continue so that significant evidence of psi, at least, will be obtained. The important thing to consider is what experimenters *actually* believe or want, which is not necessarily the same as the experimental hypothesis or what they 'should' believe or want. Finally, all these motivational variables could affect how the experimenter interacts with the subject, so this factor would need to be assessed as well.

It is possible that some experimenters might not be aware of their true desires, which also would sabotage the correlational approach. This is most likely to apply to psi-inhibitory experimenters who unconsciously might want a study to fail, either because they unconsciously fear the social consequences of being identified as a successful psi experimenter or because they unconsciously fear psi itself (Tart, 1984). (Let me hasten to add that the converse of this statement is not necessarily true; just because someone is a psi-inhibitory experimenter does not mean that the above psychodynamic factors are applicable.)

### ***Comparisons of experimenters***

A more indirect approach might also be of value. Although there is no official list of psi-conducive and psi-inhibitory experimenters, I am confident that a reasonable consensus exists within the parapsychological community about who at least some of these persons are. This raises the possibility that the two groups could be compared on

characteristics relevant to e-psi. Such an approach has already been used to assess the experimenter interaction hypothesis. Schmeidler and Maher (1981) and Edge and Farkash (1982) asked students to blindly evaluate tapes of psi-conducive and psi-inhibitory experimenters making presentations at a Parapsychological Association convention. These interesting studies should be followed up by taping experimenter performance in actual test situations, or even by having subjects rate their experimenters on relevant characteristics at the end of their test sessions.

This approach could be applied to the e-psi problem by having psi-conducive and psi-inhibitory experimenters serve as subjects in a common psi task. Although we already have evidence of exceptional psi performance from some psi-conducive experimenters (see above), a systematic comparison has never been attempted.

Unfortunately, motivational factors could compromise the efficacy of such an experiment. The reason is that a crucial number of psi-conducive experimenters might at some level want the experiment to fail, in which case they probably would not exhibit their true level of psi ability, even if they sincerely tried their best to succeed. This concern springs from my impression that most psi-conducive experimenters attribute their success to social skills rather than e-psi. There are at least three reasons why one might expect this to be the case, even in the absence of any data on the matter. First, e-psi is tantamount to self-testing, and self-testing is often frowned upon by other scientists. Although I have never heard a rational argument put forth in defense of this dubious proposition,<sup>5</sup> the fact that it has currency among mainstream scientists means that if a finding were attributed to e-psi it likely would carry even less weight outside parapsychology than it

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<sup>5</sup> I do not deny that there are circumstances in which an experimenter would not be an appropriate subject, as, for example, when it is necessary that the subject be blind to the hypothesis. I am referring to circumstances, which often occur in parapsychology, when such considerations do not apply.

would otherwise. Second, social skills are more highly valued in our culture than are psi abilities, so it is only human nature that psi-conducive experimenters would prefer the former as the reason for their success. Last but not least, the experimenter interaction hypothesis is plausible and not without its own empirical underpinnings (White, 1977). A complete understanding of the experimenter effect will require that we pay attention to both e-psi and social psychological factors.

### Conclusion

It is obvious that nonintentional psi and e-psi pose great difficulties for parapsychological experimentation. They also tell us something distressing about psi itself, namely that it is at least partly out of our conscious control and even our awareness. However, there is a brighter side to the picture, and I would like to close on a more positive note by briefly discussing what that brighter side is. Many people, including many parapsychologists, are attracted to the transpersonal viewpoint that we are all interconnected at some fundamental level. By broadening the range of potential psi sources in our experiments, nonintentional and unconscious psi lend credence to that viewpoint, even though they do not fully confirm it. For years, parapsychologists sympathetic with this perspective have complained that our research methods are inadequate to cope with psi's presumed holistic nature, but these complaints have never led to viable methodological alternatives that meet the epistemological standards of consensual evidence that are central to the identity of our field. Nonintentional and e-psi force us to recognize that these problems cannot be avoided in *any* of our experiments and thereby provide impetus to the quest for viable new methods, which could have the added payoff of making psi less elusive. I hope the ideas presented in this paper will help with this endeavor, but we have a long way to go. If we are to advance further, we must start to confront the e-psi problem more openly and directly than we have in the past, despite

any short-term advantages there might be to keeping it in the background.

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Institute for Parapsychology  
402 North Buchanan Blvd.  
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