

## EXPERIMENTER EFFECTS IN PARAPSYCHOLOGICAL RESEARCH

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(Original publication and copyright: *Journal of Parapsychology*, 1976,  
Volume 40, pp. 1-33)

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**ABSTRACT:** This paper outlines the types of experimenter effects that occur in parapsychological research. A distinction is drawn between those effects that seem to be mediated by psychological variables and those that result from extrasensory processes. The term "psi experimenter effect" is introduced to refer to unintentional psi which affects experimental outcomes in ways that are directly related to the experimenter's needs, wishes, expectancies, or moods. Several channels for the operation of psi experimenter effects are discussed, as well as numerous studies which support their existence. A review of the literature suggests that experimenter PK can influence laboratory investigations of psychokinesis and precognition. In addition, psi experimenter effects are indicated in studies showing variations in the subjects' reactions to different experimenters and in studies involving unintentional psi tasks.

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The role of the experimenter in the successful elicitation of psi results has recently received widespread attention from the parapsychological community. The issue is not a new one, however, as the importance of the experimenter has been repeatedly underscored throughout the history of experimental parapsychology. In early comments meant to guide would-be psi investigators, the Duke University research team initiated what was to become a widespread recognition of the experimenter's importance (e.g., J. B. Rhine, 1934; J. B. Rhine et al., 1940; J. B. Rhine, 1948). These researchers believed that the kind of experimenter actually in contact with a subject was critical and that the experimenter's personality was a determinative factor in the psi-testing environment. It was assumed that subjects were *made*, not born, and that it was the experimenter's job to prepare his subjects for a psi test. Historical recommendations (J. B. Rhine et al., 1940) stressed the need for enthusiasm, interest and motivation on his part:

All the skills and methods that can be devised by the experimenter for conveying encouragement, inspiring confidence, implanting a realization of the importance of the tests, and arousing and maintaining an ambition to perform well in the tests will be decidedly to the point, (p. 341)

Reports appearing in the first two volumes of the *Journal of Parapsychology* have often been cited as evidence for this characterization of the experimenter's role. In 1937, Sharp and Clark noted changes in group ESP scoring that coincided with changes in the experimenter's attitude. They concluded that the attitude and personality of the experimenter had an important effect upon the exercise of ESP ability. Pratt and Price (1938) found a difference in the scoring rates in two very similar experiments that had been carried out by separate investigators. One experimenter achieved significant hitting while the other found chance results. Pratt and Price hypothesized that the scoring differences were due to differences in the experimenter's approach to and handling of the subjects. They further suggested that failures to find evidence for psi were probably due to the presence of unfavorable subject-experimenter interactions. A similar result was reported by MacFarland (1938), who arranged for a single group of subjects to be tested simultaneously by two experimenters. One of the experimenters had been previously successful in obtaining psi results and the other had not. The expected scoring differences were found, and MacFarland ascribed them to personality differences in the two experimenters. He also noted that these differences did not seem to depend upon the experimenters having direct contact with the subjects during the test.

Later investigations have served to strengthen these trends. In fact, the experimenter-difference work of the 1930's has received almost continuous support since it was conducted (e.g., Taves & Dale, 1943; West & Fisk, 1953; Michie & West, 1957; Fisk & West, 1958; Osis & Dean, 1964; Sailaja & Rao, 1973; Stanford et al., 1975; Tart, 1975). The apparent lesson to be learned is that an experimenter must be interested and enthusiastic about his research and that this must carry over to his subjects in order to obtain psi. In addition, an experimenter must be able to "mesh" psychologically with his subjects for a psi-conducive experimenter-subject interaction to be established.

These two factors—experimenter enthusiasm and a favorable experimenter-subject interaction—have been assumed to explain why some experimenters are able to obtain psi results and others are not. Rhine and Pratt (1957), for example, stated that:

those who never succeed at all may, of course, be suspected of not ever having felt some contagious or communicable interest as would help to create a favorable test environment for their subjects, (p. 132)

In addition, Rhine and Pratt recommended an acid-bath test for hopeful psi experimenters:

The stage has been reached at which it can be said definitely that the experimenter himself can be a limiting factor in the test situation, and, if he be, he had better find out by preliminary tests of himself *as experimenter*. The only rule to follow is that of the old motto: "Pretty is as pretty does." A psi experimenter is one who, under conditions that insure he is not fooling himself, can get results. All others should do something they *can* do well. (p. 132)

The assumed effect of the experimenter in eliciting psi revolves around communication—of interest, enthusiasm and self-confidence—to a subject. Though sensory in nature, this communication is often subtle or even unintentional; it is not something easily achieved with deliberate effort. To paraphrase Rhine, some experimenters have the knack, and some don't—and those who don't ought to reconsider doing experimental work, since the knack is not included with standard ESP testing kits.

Psychological factors have also been assumed to underlie chronological declines in significance. Rhine and Pratt (1957) and Thouless (1972) discussed long-term declines in terms of waning interest and enthusiasm on the part of the experimenter. Rhine suggested frequent procedural changes to release the experimenter from the fatigue and boredom that accompany prolonged testing. Short-term declines may result from similar psychological factors. Losses of significance often plague an experimenter's attempts to replicate his own work, and self-replication attempts seem to adhere to an unfortunate pattern. The first or second experiment generally works, but additional confirmations decline toward chance or show nonsignificant reversals of the original scoring direction. In their review of work up to the 1940's, Taves and Dale (1943) refer to this decline as part of an experimenter's "Midas touch":

A new experiment is begun; the initial total data from all subjects are positive, then the experiment collapses *in toto*, and no subject, experienced or naive, scores above the chance level, (p. 63)

The list of recent experimenters who have suggested similar interpretations for their results includes White and Angstadt (1965), Parker and Beloff (1970), Layton and Turnbull (1975), and Wiklund (1975). The usual suggestion is that declines in significance across experiments are due to a loss of motivation and interest on the part

of the experimenter. This lack of "contagious enthusiasm" apparently demolishes the psychological conditions necessary for the successful elicitation of psi.

Parapsychology is not alone in its recognition of experimenter effects; similar trends have been noticed in the experimental psychology literature. A recent model by Rosenthal (1966) summarizes the influential role an experimenter can play in the elicitation of successful psychological results. Attempts to explicate the role of the experimenter in psi research should begin by examining the similarities and differences between processes proposed by Rosenthal and those that seem to be operating in parapsychology.

Rosenthal's model is a model of *sensory* experimenter effects. Of prime importance to him is the concept of experimenter expectancy and its subsequent biasing consequences on empirical investigations. Rosenthal feels that in many cases experimenters obtain the results they expect to obtain. This effect is mediated by subtle sensory cues that are unintentionally communicated by the experimenter to his subject, and result in the subject's unconsciously structuring his performance to match the experimenter's expectancies. Rosenthal uses the term "communication" in its broadest sense to refer to both verbal and nonverbal signals that are passed between the experimenter and his subject. He extends the network to animal research, indicating that this type of communication often manifests itself through differential handling and feeding. It is not within the scope of this paper to present a detailed description of Rosenthal's experimenter-expectancy model. The model has been extensively investigated, however; and although some parts of it are not universally accepted (see Barber & Silver, 1968), the overall predictions seem to be in general accord with the psychological data (for a review see Rosenthal, 1967, 1968, 1969).

Investigations by several researchers (Waldron, 1959; Honorton et al., 1975; Layton & Turnbull, 1975; Parker, 1975; Taddonio, 1975, 1976) verify that some aspects of experimenter effects in psi research seem to be mediated by sensory processes similar to those proposed by Rosenthal. These studies have shown that the attitude, mannerisms, and expectancy of the experimenter can have a marked effect on results, and they also extend earlier research findings bearing on the importance of the specific attitude held by the experimenter toward individual subjects (e.g., Price & Rhine, 1944; Woodruff & Dale, 1950; Nash, 1960, 1968; also for reviews of the investigations of teacher-pupil attitudes see Anderson & White,

1958; White & Angstadt, 1965). As was previously pointed out, it has generally been assumed that the *psychological* impact of the experimenter-subject interaction is the critical component of experimenter effects. However, there have also been suggestions that experimenter effects may have a *parapsychological*, as well as a sensory or psychological, component. In foreshadowing this suggestion, Eisenbud (1963) noted:

Experiments are conducted on the curious assumption that the subjects in them will not use the very faculties they are being tested for . . . until they step across the threshold of the laboratory and hear the starting gong, and that they will use these faculties only within the confines of their designated roles in the particular design employed. . . .

By the same token, it seems implicitly to be taken for granted that experimenters . . . will not, for whatever obscure reason, use any psi faculties *they* may have to muddy the field. . . . Everyone behaves, in short, as if there were some sort of gentleman's agreement committing subjects, experimenters, judges and other participating personnel to stick faithfully to their assigned roles in the experiment as scripted and to neither take any notice of nor infringe upon what any of the others are doing, (p. 258)

Recent calls for an investigation of the parapsychological component of experimenter effects have been made by White and Angstadt (1965) and Honorton (1975). It is the objective of this paper to outline the nature of these effects. Several channels for the operation of psi experimenter effects will be discussed, as well as numerous studies which support their existence. Some attempt will also be made to assess the feasibility of extracting subject effects in parapsychology from those mediated by the experimenter in a psi capacity.

Throughout the paper, the term *experimenter* will be used loosely to refer to anyone connected with the experimenter side of an investigation, be he senior experimenter, assistant, judge, checker, or other. In addition, the term *sensory experimenter effect* will be used to refer to those effects that seem to be mediated through sensory or psychological channels, whereas the term *psi experimenter effect* will denote a parapsychological mediation of the effect. Specifically, the term *psi experimenter effect* will be used to refer to unintentional psi which affects an experimental outcome in ways that are directly related to the experimenter's needs, wishes, expectancies, moods, etc. Thus, psi experimenter effects can be viewed as a special case of the larger question of unintentional psi.

## I. THE CASE FOR EXPERIMENTER PK

*Psychokinesis Studies*

A clear channel for the operation of psi experimenter effects is evident in standard laboratory procedures used to investigate psychokinesis (PK). That the "cause" of a particular PK effect is the subject—rather than anyone else involved in the experiment—seems to be an arbitrary assumption. It can be questioned whether any PK experiment can be designed so as to rule out the influence of persons other than subjects on the results (J. B. Rhine, 1975). Specifically *experimenter* influence cannot be ruled out—even when the experimenter is separated from the targets; he can use his ESP to "see" the targets, and influence them by PK. There have been several successful empirical reports of such "blind PK" (e.g., Thouless, 1949-1952; Osis, 1953; Fisk & West, 1958; Forwald, 1963; Cox, 1974).

In order to assess the likelihood of experimenter intervention in PK studies, the question of motivation must be raised. As previously mentioned, the role played by motivation in the successful elicitation of psi has been strongly emphasized. Changing levels of motivation have traditionally been cited as the explanation for internal effects such as position curves and declines in performance (see Rhine & Pratt, 1957; Rao, 1966; Thouless, 1972). A question central to the issue of experimenter effects is: Who has the greater motivation in the experimental setting, the subject or the experimenter?

*Parapsychologists should face the fact that an experimenter is typically more motivated than his subjects to achieve successful results.* Heightened experimenter motivation is especially apparent in studies using unselected subjects: in such a setting the experimenter usually derives more benefits from positive results than his subjects do. This is because parapsychology is becoming more and more susceptible to the "publish or perish" syndrome plaguing other fields of modern science. But it is also the nature of psi, with its far-reaching implications, that holds a personal attraction for many experimenters in the field. Some well-known investigators were attracted to parapsychology for philosophical reasons, and others entered the field because they felt it held the key to personal experiences. An example is Laura Dale, who had several spontaneous PK experiences herself. In reporting the first PK work carried out at the A.S.P.R., Dale (1946) noted:

An experimental validation of these personal experiences meant a good deal to [me]. Motivation, then, was intense on the part of the experimenter; we doubt whether it was on the part of the subjects, (pp. 142-143)

Dale noticed that her subjects' scoring patterns seemed to follow her own levels of motivation and personal idiosyncracies. In explaining her successful PK studies, she postulated that:

the experimenter influences the dice, and . . . the subject plays his role in either liberating or inhibiting this ability of the experimenter, (p. 142)

Another factor which must be considered when evaluating experimenter effects is the role of *intent* in the elicitation of psi. In standard PK tasks, the subject intends to create the result, whereas the experimenter does not. The importance of intent can be evaluated by examining studies of so-called "unconscious" PK. Here, by definition, the subject has no conscious intent to produce a PK effect. The use of the term *unconscious* to refer to this type of PK studies seems a misnomer. Because psi itself is thought to be an unconscious process, the term *unintentional* will be used instead when referring to experiments during which either the subject is not aware that a PK effect is desired, or does not know that a PK effect will be to his benefit.

There are at least four reports in the literature that can be interpreted as studies on unintentional PK (Lewis & Schmeidler, 1971; Camstra, 1973; Schmidt, 1975; and Stanford et al., 1975).<sup>1</sup> The studies all report successful results, although the subjects involved had varying degrees of presumed motivation. Schmidt (1975) presents the results of unintentional PK studies as possible evidence for psi experimenter effects. He believes that since PK need not involve a conscious effort, an experimenter can influence his results unintentionally.

Work on unintentional PK presents an interesting dilemma. If it is the subject who "causes" the PK effect, then in any experiment, the person designated as the experimenter is as likely (if not more likely) to exert a PK influence as the person designated as the subject. This is because an experimenter also has motivation, but probably not conscious intent. However, if the subject is *not* producing the PK effect, and yet a PK effect exists, then the experimenter must be the cause, albeit unintentionally. With either interpretation the case for unintentional PK is clearly made in these studies: no one is consciously intending to cause a PK effect, yet a PK effect occurs. Further, some investigators have recently suggested that PK is in fact enhanced when it operates in an unintentional manner (Brookes-Smith, 1973; Stanford, 1974b).

<sup>1</sup> Lewis and Schmeidler's study is described as a precognition task; however, by arbitrarily defining one output of a Schmidt four-button RNG as a hit, PK becomes an equally viable interpretation.

If the hypothesis of experimenter PK is correct, one would expect successful PK experimenters to also be successful PK subjects. A preliminary look at the literature uncovered remarkably consistent support for this notion. For example, J. B. Rhine (1943; Averill & J. B. Rhine, 1945; J. B. Rhine et al., 1945), Nicol (Nicol & Carrington, 1946-1949), Humphrey (1947), McConnell (1955), and Forwald (1961, 1962, 1963; see also L. E. Rhine, 1970) were all successful PK subjects as well as successful PK experimenters. The same is true of W. E. Cox, long regarded as a high-ranking PK experimenter (Cox, 1962, 1965; L. E. Rhine, 1970). In addition, Helmut Schmidt (1973, 1974) finds he is often his own best subject.

More recently, Honorton (1975; Honorton & Barksdale, 1972) has attributed a PK effect on a random number generator to himself rather than to his subjects. In his report of the effects of muscle tension and relaxation on PK, Honorton notes significant psi-hitting for subjects following suggestions for muscle tension. This effect was present both when a group of subjects (with Honorton present) attempted to influence a random number generator, and when Honorton alone served as subject. The effect was lost, however, when an additional series was conducted by another experimenter. In explaining the results, Honorton and Barksdale concluded both that "traditional boundaries between subjects and experimenters cannot be easily maintained" and that the results could represent "a psi-mediated experimenter effect" (p. 213).

### *Precognition Studies*

The procedures basic to precognition experiments have also been regarded as possible avenues for psi experimenter influence. By definition, a precognition task requires the random generation of targets after the subject has recorded his calls. Such a process leaves open the possibility of a PK influence by the experimenter on the target generation procedure so that the targets will match the subject's calls. The standard precognition procedure, then, may be reformulated as a blind PK task on the part of the experimenter.

While the above contention may be considered nonparsimonious, reports as early as 1938 on "ESP shuffle" experiments forced serious consideration of the issue. The ESP shuffle is similar to a standard card-calling precognition task, except that the roles of experimenter and subject are reversed. The subject shuffles a deck of cards, attempting to "match" a series of calls provided by the experimenter. It has long been recognized that the ESP shuffle, although a highly



successful procedure, disallows conclusive evidence for precognition in a hand-shuffled card-calling procedure (Rhine et al., 1938). Any precognition experiment in which the targets are generated on a trial-by-trial basis may be plagued with similar difficulties. To overcome the problem of PK contamination of precognition studies, a now-standard technique was devised which involved generating precognition targets from a table of random numbers and determining the table entry point by a random process. The outcome of two throws of a triad of ten-sided dice was operated upon by a complex algebraic procedure, the solution to which determined the actual entry point (Rhine, 1971). Proponents of the "dice method" of randomization felt that if the experimenter was going to use his PK to match a subject's calls to a set of targets, this would occur while the target sequence was being generated. The dice method was presumed to block any experimenter influence, as the experimenter would be unable to carry out mentally the complicated mathematics involved. Also, his PK would not be reliable enough, nor efficient enough, to achieve the required result.

These assumptions can be questioned from at least two sources, however. Certain mathematical wizards are known for their ability to carry out complex algebraic and logarithmic functions. Mentally extracting the cube root of a ten-digit number appears to be a relatively straightforward exercise for these so-called "idiots savants" (Newman, 1956). Thus, it appears that the conscious mind does have the capacity to carry out such extremely complex calculations. But more importantly, however, is the fact that no evidence has accumulated which indicates that psi is *limited* by the processing abilities of the conscious mind; nor have limits been established on the reliability and efficiency of PK.

Morris (1968) designed a study to test whether the procedure involved in selecting random entry points effectively blocked the experimenter's ability to achieve a desired target sequence. He used himself as subject and compared entry-point determination by dice tosses with that resulting from his calling out numbers at random. Either the numbers generated by the dice tosses or by Morris verbally were plugged into the equations used to determine the entry point. The subject was to try for a sequence in the random number table that had more odd than even numbers, or vice versa; selection of odd-aim or even-aim trials was done in accordance with a prearranged order. Morris's results showed chance results with the dice method, but *significantly biased results* with his own numbers. Seven-

teen out of twenty sequences "matched" his aim for a majority of odd (or even) numbers. Though results with the dice method were at chance, the fact that he, as subject, could influence target generation at all led Morris to speculate about possible experimenter PK on the dice during entry-point determination. To Morris, the question became important when forming *conclusive* interpretations of precognition experiments.<sup>2</sup>

The actual results of this study are not as important as the principle it raises. Influencing the dice method by PK is a very complex, blind PK task. If psi is independent of the complexity of the task—as is often assumed—then it is as easy for the experimenter to influence results with PK as it is for a subject to guess targets precognitively. More general implications of this complexity issue will be discussed later.

#### *Experimental "Miscarriages"*

A third way for the experimenter to influence experimental results directly has been suggested by Eisenbud (1963). Various accidents such as equipment failure, procedural error, inappropriate randomizations, etc., are sprinkled throughout parapsychological work. Although these experimental "miscarriages" may produce unreliable or artificial positive results, these parts of the experiment have to be discarded. Schmeidler (1964a) and Rao (1968) have published instances of such "error phenomena," pointing out their marked coincidental—and perhaps even paranormal—aspects. However, the uncontrolled nature of these occurrences, as with spontaneous cases, makes them primarily useful for stimulating further controlled investigations.

## II. SUBJECTS' REACTIONS TO EXPERIMENTERS

Many studies have been reported whose results seem to indicate the existence of parapsychological experimenter effects. An exhaustive review of these reports is not intended, however. This section

<sup>2</sup> It is beyond the scope of this paper to discuss the methodological procedures geared toward proving the existence of precognition. However, it should be pointed out that using "random" processes which are generally regarded to be of wide concern to *many* people (e.g., weather reports, the stock market, etc.) would serve to diffuse, though not eliminate, some of the questions of experimenter motivation raised here. However, the use of such alleged multiple-motivation sources may not provide as clear a solution as one would desire. The most desirable outcome is to obtain very significant effects. *The Rand Table of a Million Digits* has but approximately a million entry points. Obtaining significant results with a  $p$  much less than  $10^{-6}$ , however, makes it unlikely that a needed target sequence would be found in the table.

will review only those studies which readily lend themselves to a psi experimenter-effect interpretation.

### *Experimenter Differences*

It is common knowledge that English investigators have been plagued with psi-elicitation troubles. An exception to this was G. W. Fisk, a retired businessman and inventor who became active in parapsychological research in the 1950's. Fisk's parapsychological career was marked by the attainment of consistently significant results. His counterpart was D. J. West, an experimenter who had been generally unsuccessful in his attempts to elicit psi. Not surprisingly, West suggested a collaborative effort between himself and Fisk aimed at discerning differences in the data collected by the two of them. Several studies were carried out which, by and large, conformed to a single pattern: Fisk's data yielded positive results and West's data were close to chance.

The first West-Fisk study was a home test for clairvoyance (West & Fisk, 1953). Packets containing clock-card targets were mailed by Fisk to twenty subjects, who proceeded to record their guesses for each clock card and return the record sheet and unopened target packets to him. (Clock cards consisted of a diagram of a clock face upon which the subjects made their guesses by drawing an arrow from the center to one of twelve sectors. This allowed an analysis of the degree of missing involved when a subject's response was not correct.) The clock-card targets had been generated from a table of random numbers, with each of the experimenters generating the targets for, and checking half of, the data. The overall results of the study showed significant hitting ( $p = .0011$ ), all of which had been contributed by Fisk's data (which were independently significant at the .00015 level). The results from West's half of the data were at chance. There were several weaknesses in the study: some of the subjects were close friends or relatives of Fisk, and the security involved in "mail-order" home testing of this type was generally not ideal. These weaknesses were offset by the fact that the subjects did not know that two experimenters were involved or that each of them would be preparing and checking half of the clock-card material. On the basis of their results, West and Fisk stated:

This suggests that DJW is a jinx and gets only null results, for the scores of his section have only diluted the better results obtained when was experimenter, (p. 186)

Another Fisk-West study (Fisk & West, 1958) also relates to the question of psi experimenter effects. A blind PK experiment was conducted which required that a "special subject" match dice targets displayed alternately in either Fisk's or West's home. The subject thought she was dealing only with Fisk and mailed the score sheets directly to him. Fisk scored the sheets for his targets and mailed the remainder to West for scoring. Fisk and West were unaware of each other's targets, and early results of the experiment showed positive deviations for both experimenters' results. West's data declined to nonsignificant missing as the test continued, although Fisk's maintained an overall positive direction ( $p = .013$ ). The difference between Fisk's and West's results was marginally significant ( $p = .05$ ). It is not clear, however, whether this difference may be attributed solely to a psi experimenter effect, inasmuch as the subject was expecting all of the targets to be in Fisk's home. The nonsignificance of West's data could reflect a failure on the part of the subject to locate the targets.

The existence of several anecdotal reports gave rise to a series of studies conducted by S. R. Feather and Brier (1968) on the role of the checker in ESP scoring. In group tests, Feather or Brier asked subjects to fill out four runs of ESP testing cards and then to guess which two of the runs would be checked by the person who administered the test and which two would be checked by someone else. After the call sheets had been completed, targets were generated for the series by the standard dice procedure. A random process was used to determine the runs to be checked by the test administrator for each subject. When S. F. acted as test administrator, the overall results were below chance ( $p = .02$ ). A significant scoring difference ( $p = .046$ ) was found between those runs that subjects accurately predicted S. F. would check and the runs not accurately predicted. This difference was present only in S. F.'s data, however; R. B.'s data were at chance. S. F. then administered the test to another group, with nonsignificant overall results. However, the same trend ( $p < .03$ ) for accurately predicted vs. inaccurately predicted runs was evident in her data. When R. B. acted as test administrator, the overall results were not significant, although again the difference in scoring between, accurately/inaccurately predicted runs was evident ( $p < .04$ ). In this series, S. F.'s data did not show a difference between these runs.

At first glance it appears that the checker effect may be psychological and that it relates to a subject's prediction of who will

score his data. The fact that the scoring difference appeared only in the data checked by the main experimenter (the test administrator), however, indicates that the person who actually checks the data has an effect on the outcome. The effect was small but consistent—few studies have shown exactly the same level of significance for three series. Additional analyses performed by the present authors on the published Feather-Brier data yielded further support for the checker effect. When R. B. acted as the "other checker," overall significant missing was evident in the data ( $CR = -3.04$ ,  $p < .002$ , two-tailed). With S. F. as the "other checker," insignificant hitting was obtained. The difference between R. B. and S. F. yielded a highly significant  $CR_d$  of 3.18 ( $p < .002$ , two-tailed). It should be remembered that the Feather-Brier series was a precognition experiment and that therefore PK by the experimenter at the time of target generation cannot be ruled out. This interpretation does not seem so unreasonable when compared with the alternative that the checker might have influenced the outcome of the precognition data.

#### *The Multivariate Approach*

Several studies have used multivariate analyses of several variables operating within a given experiment to assess the role of experimenter effects. One of these was reported by Price (1973) in a paper dealing with the effects of imagery and target types on run-position effects. The second study reported in the paper was conducted after Price had delivered a lecture about parapsychology. Each subject was given runs in which erotic targets were alternated with nonerotic targets in a standard envelope clairvoyance test. The symbols used for the study were the first letters of five erotic words. Price prepared the erotic targets, and his female assistant prepared the nonerotic material. The assistant was reported to have been in a very negative mood when she prepared the last three of her fourteen target sets. Price included her mood as a "last-minute" variable to be investigated in the study. The results of the study showed a significant interaction between her mood and the target types. Specifically, the average of the negative mood runs was 6.33 (where  $MCE = 5.00$ ), and the analogous average for the neutral targets was 4.91. The difference was significant at the .01 level. In addition, the erotic runs associated with negative moods were significantly different from the nonerotic runs ( $p < .001$ ). The "nonerotic agent" was probably not known to any of the subjects, was not present at the

time of testing, and had nothing to do with the scoring of the data. Price felt that his findings added "further weight to the proposition that *any* person connected with an ESP experiment may exert a significant influence upon the psi process" (p. 317).

Three reports have appeared using stepwise multiple regressions to look for relationships between psychological and ESP variables (Carlson, 1970; Osis et al., 1971; Osis & Carlson, 1972). Some of the many psychological variables included various mood scales for the experimenters who prepared the targets and were present with the targets during the experiment. The ESP variables included hitting and missing for forward and backward displacement as well as for direct scoring. The testing procedures were card clairvoyance tasks during which the subjects had no sensory contact with the experimenter. The overall scoring was not significant in any of the experiments. Stepwise multiple regression is used to indicate where the relationships among several variables seem to lie. However, it does not measure the significance of association between variables, and the resulting  $p$  values are not corrected for multiple analyses. Although several apparent relationships were found, they are difficult to evaluate, and the lack of consistent relationships across experiments makes it difficult to interpret the results. The reports do suggest that further work investigating the experimenter's mood is warranted and that with such mood factors individual differences may be important.

#### *Unintentional Psi Tasks*

Besides adding considerably to the evidence for psi experimenter effects, the following studies give some hints about the magnitude and range of such effects. They are different from the experiments reviewed so far, in that the subjects in these experiments thought they were participating in normal psychological experiments. They did not know that an ESP task was also involved.

The first experiments to be discussed involved an agent who was actively trying to influence a subject's response parapsychologically, while the last experiment involved only a psi experimenter influence on the subject's response. Since expected responses on the intentional, psychological aspect of the task were known, the extent of the unintentional—i.e., unintentional from the subject's point of view — parapsychological influence upon the subject's responses could be evaluated. In a purely parapsychological experiment, distinguishing an unintentional psi component from the subject's own intentional psi component is a difficult, if not impossible, task.

Several well-designed experiments have been reported by Kreitler and Kreitler, whose research to date has focused on the entrance of parapsychological effects into standard psychological experiments. Their first article reported three psychological experiments which involved agents who attempted to influence subjects parapsychologically during some of the trials (Kreitler & Kreitler, 1972). The psychological tasks selected by the Kreitlers were chosen because of their ambiguity, thus allowing for the maximal possibility for the entrance of psi. In all three experiments, the agent did not know the subject. None of the experimenters who actually administered the test believed in ESP, and all were given a negative expectancy for the outcome of the experiment. The senior experimenters were themselves neutral about ESP at the time.

The first experiment was conducted as a subliminal perception task. Latin letters were projected to subjects at a near-threshold level of speed and illumination. On certain trials, an agent (whose presence was unknown to the subjects) tried to transmit the correct letter to the subject telepathically. Each slide was presented twice, once with the agent, and once without. To counteract possible artifacts stemming from fluctuations of threshold, the Kreitlers scored their data in terms of the number of "incorrect" non-agent responses which changed to "correct" responses during agent trials. This analysis was significant at the .02 level. Further analyses showed that the effect occurred only for letters that had definitely been projected below threshold on non-agent trials (i.e., had been correctly identified less than 33% of the time). The results of this study received independent replication by Lübke and Rohr (1975).

The second study was an investigation of an agent's influence upon the perceived direction of autokinetic motion. There was no overall effect by the agent in the data, but additional analyses showed a significant ( $p < .01$ ) tendency for ESP to manifest in those cases when the subjects' tendency to call the target direction on non-agent trials was low. The third study investigated ESP influence on the Thematic Apperception Test (TAT). This task requires a subject to make up a story to describe a particular picture. In the experimental condition, the agent attempted to transmit particular words to the subject. Analyses were carried out blindly on exact-word and thematic-word correspondence between a subject's responses and the agent's transmission. Significant effects were found for certain words, although the overall results were weak. The ESP effects were again concentrated on those responses which occurred infrequently in the non-agent (control) conditions.

Kreitler pointed out that the ESP effects occurred between "average" people who did not know each other, that they occurred when the subject was not aware of attempted transmissions, and that they manifested even when the experimenters administering the test did not believe in ESP. Also, in all three studies the ESP seemed to occur only on responses that had a low probability of occurrence otherwise.

A subsequent study by the Kreitlers (Kreitler & Kreitler, 1973) compared ESP under two conditions: when an agent was merely thinking about the target, and when he was actively attempting to transmit it to a subject. The agent variable was investigated under three conditions: in the presence of weak subliminal stimuli conveying the same information as the agent was sending, in the presence of subliminal stimuli contradicting the ESP message, and in the absence of subliminal stimulation. Unselected subjects received supraliminal stimuli (circles or lines) supplemented by either (a) subliminal stimulation which produced an optical illusion, and/or (b) ESP transmissions from the agent. The major finding of the study was that ESP was most pronounced when it contradicted the information conveyed by the subliminal stimuli and was "transmitted" by the agent ( $p < .01$ ). The effect was especially strong for those subjects who were in general more sensitive to a transmitting agent. It should be noted that the subliminal stimuli were of very weak intensity, and that there was no indication of responses to them other than in the ESP interaction.

If artifactual ESP can enter into psychological experimentation, it seems reasonable to assume it will enter into parapsychological experimentation. One limitation in extending these findings, however, is the extreme ambiguity of the tasks. Although psi experiments may be construed as ambiguous tasks themselves, a stronger case for experimenter effects would be made if there was analogous evidence in existence for clearly unambiguous tasks. In fact, one of the strongest cases for psi experimenter effects would be made if it could be shown that a subject deviated significantly from a well-structured, non-ESP task in a way that was consonant with the experimenter's goals.

Exactly such a study was conducted by Stanford (1970). The purpose of the study was to discover the influence of ESP upon a subject's memories of previously learned stories. After participating in another psi test, subjects were asked (ostensibly) to evaluate several dream reports that had been collected for another experiment.



They were asked to listen to a dream report and then to evaluate the dreamer's personality. This was followed by answering a multiple-choice questionnaire to evaluate their memory of the dream report. Throughout the session, the subjects were not aware that they were participating in an ESP test.

Questions for the multiple-choice test were of three types: some had one correct answer, some had two partially correct answers, and some had no correct answers. Stanford superimposed a precognition task on the memory test by randomly choosing one of the alternatives as an experimentally defined "correct" answer. Thus some of the "correct" answers agreed with the story from the dream report and some did not. Significant ESP scoring was found on subjects' responses that deviated from the dream reports. But more important for psi experimenter effects was the question of whether the subject was "pushed" by ESP to give other than a pro-story response on those items which had a clear-cut answer from the story but for which the ESP target was counter to the story. The answer was found by comparing the number of counter-story responses when the "correct" target was in agreement with the story with the number of counter-story responses when the "correct" target did not agree with the story. When the targets agreed, only 1 out of 51 responses (1.96%) were counter-story. When the targets were in opposition, i.e., did not agree, 31 out of 189 responses (16.40%) were counter-story. The difference between these conditions was significant ( $p < .008$ ). ESP, then, apparently did "push" the subject away from his conscious task. Again, it should be remembered that Stanford's study was a precognition task, and that the possibility of PK influence by the experimenter at the time of target generation can not be ruled out.

### III. DISCUSSION

How strong is the evidence for psi experimenter effects? Our review of the standard procedures used in PK and precognition research clearly points out the role an experimenter can play in contaminating his results paranormally. The case for experimenter PK seems clearly drawn when one considers that experimenters are typically more motivated than their subjects to achieve good results, that PK need not involve a conscious intent, and that most successful PK experimenters are themselves successful PK subjects.

The ESP studies which examined subjects' reactions to experimenters suggest another type of psi experimenter effect. Although the studies are few in number, their real impact lies in the

consistency of the results, both within studies and across experiments. The results of Fisk and West fit in nicely with the overall pattern they were investigating. The checker effect, springing from strong anecdotal evidence, held up consistently in the laboratory. Rex Stanford, a consistently successful experimenter, found that his success was not diminished when a subject's psi task and conscious task were in opposition. The Kreitlers reported similar "opposition" interactions in three of their experiments. Likewise, the Kreitlers' study involving subliminal perception of letters, replicated by Lübke and Rohr, found ESP effects only in those conditions that showed a low probability of correct responding without ESP.

It is interesting to note that six studies which examined the interaction of parapsychological with psychological tasks from three independent laboratories showed the strongest psi effects when the experimentally defined "desired" response was in opposition to the subject's own tendencies (Stanford, 1970; Kreitler & Kreitler, 1972, 1973; Lübke & Rohr, 1975). This evidence suggests that *the parapsychological influence of the experimenter may be strongest when the experimental hypothesis is, in fact, "false";* i.e., in the condition where the subject would make different or less significant responses in the absence of experimenter influence. The combining of psychological and parapsychological tasks is an important step for those interested in the examination of experimenter effects. Although it seems closely allied to the response bias hypothesis, applying the principle inherent in these studies to purely parapsychological experiments requires caution and should involve a detailed consideration of psi differential effects. This principle, however, may allow one to speculate about the loss of significance and the reversals in scoring so common to parapsychological research. An experimenter may influence his subjects in a direction away from that which they would normally follow. If the experimenter's influence is strong enough, the result may become a significant effect. As the experimenter continues his investigations, however, his influence may decline because of boredom, lack of motivation, etc. Such a decline could result in the subject's becoming "more influential," and hence, scoring may revert to its "normal" direction.

While other studies that show differential experimenter and subject effects exist, they have classically been regarded as indices of sensory experimenter effects. Results such as those reported in the introduction are generally felt to conform to differences predicted by the Rosenthal model. (It should be recalled that the

experimenter-bias effects hypothesized by Rosenthal are purely psychological and based on sensory cues given off during the experimental session.) Experimenter communication of a subtle sensory form is known to enhance a subject's interest and confidence, resulting in a heightened desire on his part to score well. Declines in significance occurring when experimenters replicate their own work may be a direct indication of the role of the experimenter in "setting the mood"; declining interest results in declining psi performance.

But does the presence and absence of experimenter enthusiasm, with its corresponding inclines and declines in subjects' performance, bespeak a process truly mediated by sensory cues, or is there an equally subtle paranormal component involved? The dividing line between a psychological and a parapsychological effect is ill-defined at best. In principle, however, it seems possible to distinguish the two in terms of the fundamental processes underlying both. Although not yet regarded as conclusive, present indications are that psychological experimenter-biasing abilities can be *learned* (Rosenthal, 1966, 1969). Rosenthal reports that the process operating in experimenter expectancy is evidently unintentional, yet his "experimenters" get better at biasing their subjects. In addition, some individual differences are evident in his data, in that certain "experimenters" show a particular talent for biasing their subjects. A basic prediction from the Rosenthal literature is that, with practice, some experimenters should get better at eliciting desired performance in their subjects.

If the basis of experimenter effects in parapsychology is a process similar to Rosenthal's expectancy-bias model, one would expect an increase in positive results as the experimenter becomes practiced. Therefore, one would expect experimenters to obtain increasingly better results each time they attempted to replicate their own previously successful work. What is typically found, however, is that self-replication attempts do *not* yield better results. As mentioned previously, the typical trend is a steady decline across replication attempts, resulting in drops to chance. If subsequent reports of Rosenthal effects substantiate it as a learned phenomenon, one will have to assume that the standard experimenter effects operating in parapsychology are manifestations of a fundamentally different process.

#### IV. PSI-MEDIATED INSTRUMENTAL ARTIFACTS: NOTES TOWARD A RESOLUTION

Whatever the amount of parapsychological experimenter effect in a given situation may be, it is desirable to understand the

dynamics underlying the process. Particularly one wonders who has the more "active" psi role, the subject or experimenter? We have delineated two directions for a psi experimenter effect—one between the experimenter and his equipment and one between an experimenter and his subject. In the first case, when his apparatus can be directly affected by PK, the experimenter can clearly take an active psi role. Those cases that involve apparent paranormal communication between the experimenter and subject are not as easy to classify, however. In the case of experimenter-subject involvement, does the experimenter influence the subject, or does the subject respond to the wishes of the experimenter? This question is directly related to the controversial issue of "active-agent"- "active-subject" telepathy (see Carpenter, forthcoming, for a recent discussion). Strong cases have been made for both positions. Rather than attempting to resolve the controversy, it may be more fruitful to examine the dynamics of parapsychological experimenter effects within the context of a model that incorporates both possibilities.

Many ideas scattered throughout the parapsychological literature were drawn together and formalized by Stanford (1974a) in his PMIR ("psi-mediated instrumental response") model, which emerged from an attempt to describe the operation of psi in spontaneous (i.e., nonintentional) settings. Assumptions 1 and 5 of the PMIR model<sup>3</sup> provide a possible interpretation of psi experimenter effects. In discussing the experiments reviewed in this paper, Stanford postulates that the subject scans his environment with ESP and forms a disposition toward the "correct response." A "correct response" is defined by the experimenter in his formulation of the hypothesis under investigation. Presumably, the subject's predominant need comes from his desire to do well in the experiment. This need can extend to situations where "doing well" on the psi task is at odds with "doing well" on the conscious task. Stanford's approach readily explains any study designed to show a difference in scoring between conditions, including those studies whose differential results relate to experimenter effects. However, if parapsychological

<sup>3</sup> Assumption 1 is as follows: "In the presence of a particular need the organism uses psi (ESP), as well as sensory means, to scan its environment for objects and events relevant to that need and for information crucially related to such objects and events" (p. 43). Assumption 5 is as follows: "PMIR can (but need not always) occur: (a) without a conscious effort to use psi; (b) without a conscious effort to fulfill the need served by PMIR; (c) without prior sensory knowledge of the existence of the need-relevant circumstance; (d) without the development of conscious perceptions (e.g., mental images) or ideas concerning the need-relevant circumstance; and (e) without awareness that anything extraordinary is happening" (p. 45).

influence is the dominant component of experimenter effects, Stanford's approach would have difficulty explaining an experimenter's ability to elicit psi in the first place. According to Stanford, subjects respond to the hypothesis devised by the experimenter in a given study. For this to play a role in psi elicitation, the subject would have to be responding to more than the experimenter's conscious definitions and wants. Most serious experimenters want results, yet some get neither "good" psi results nor results that are consistent with their hypothesis. This approach also has difficulty in explaining declines in significance that result from replication attempts. If subjects are responding to the experimenter's hypothesis, new subjects should be as likely to respond to replications as the original subjects were to the initial success.

The form a PMIR can take is variable. Stanford's original formulation of the model included behavioral and cognitive responses as PMIR (1974a), and his recent extension of the model allows for both PK and active-agent telepathy as PMIR outlets (Stanford, 1974b). Stanford points out that his assumptions concerning PK as PMIR are more speculative and controversial than those concerning ESP and that, in some cases, they seem to go against the trend of laboratory studies. Much of the evidence for his assumptions is anecdotal in nature. Some of the assumptions are a precise conceptualization of the heretofore fragmented observations of Schmeidler (1961), Batchelder (in Brookes-Smith, 1973) and others. Several others are derived from the common elements of popular—and presumably successful—religious and quasi-religious practices.

Assumption 18 bears most directly on the issue of psi experimenter effects.<sup>4</sup> Stanford discusses this assumption in the context of animal psi experiments:

<sup>4</sup> Assumption 18 (Stanford, 1974b) is as follows: "When persons consciously wish to produce effects upon physical or biological systems, the following factors when jointly operative, increase the probability of the occurrence of PK and increase the amplitude of the effects: (a) strong motivation; (b) the cognition that one's personal efforts have proven incapable or are known to be incapable of meeting the need; (c) the cognition that one has properly appealed for a solution (through prayer, rituals, trances, etc.) to agencies, powers, supernatural beings, or forces believed to exist independently of oneself and to be fully capable of producing the needed outcome; (d) the feeling that, granted one's earnest and suitable appeal under these circumstances, responsibility is now fully given over to the aforementioned agency (or whatever) which can handle the matter at its discretion or according to its own disposition or nature; and (e) one thus appropriately reduces one's level of concern and ceases to focus attention on the problem as such. Additionally, the cognition that agencies such as those referred to in item c are already working and producing effects can serve to release PK effects in an individual" (p. 351).

When one has a need with respect to a problem which can be solved by PK, it would be foolish to say, "Out of sight is out of mind" or "... out of range of PK influence." An experimenter preparing his apparatus, getting his animals ready, and then leaving them with some feeling of assurance that the experiment will run and the animals will appropriately "do their thing" cannot but remind us of certain aspects of magic, ritual, or perhaps petitionary prayer. Something is done with confidence that it will produce a desired result, and the participant, once he has done this, psychologically puts a distance between himself and the outcome. He is not trying to *make* things happen, but just trusts that they will. Again, such circumstances may provide an optimum opportunity for psychokinetic intervention, (p. 338)

Stanford's "ritual" obviously applies to experiments using human subjects as well. In most psi experiments, the experimenter meets more of the criteria prescribed for successful PK than does the subject. It is generally true that experimenters design their study, construct the needed material, instruct their subject, and then *leave*, feeling fully confident that the subject will take care of everything. Following the PMIR model, the resulting detachment on the part of experimenters is a near ideal condition for their taking an active parapsychological role. An "active-experimenter" interpretation readily lends itself to explaining failures to replicate. It also explains why certain experimenters consistently get positive results. However, at present, one cannot be sure that these effects are not at least partially sensory in nature. Either an active-experimenter or an active-subject interpretation fits the PMIR model, and it seems impossible at the moment to distinguish between them.

Perhaps the confusion arises in looking at the issue from one extreme or the other. More versatile concepts may be necessary in order to interpret experimental results correctly. Steps in this direction have been taken by those who view a given experimental result as representative of the experiment as a whole. Nash (1974) coined the term "experimental autonomy" to refer to his belief that the experiment as a whole elicits the phenomena it is designed to test. Although the concepts are not readily discernible to those unfamiliar with quantum mechanics, Walker's quantum theory of parapsychological phenomena (Walker, 1974, 1975) leads to the similar view of psi effects as collectively produced by all those involved with an experiment.

How "subject effects" mesh with "experimenter effects" is a matter not easily explicated. The magnitude of a subject's effect is assumed to depend upon his psi ability, motivation, and interest. Likewise, the magnitude of an experimenter's effect can be assumed

to relate to his psi ability, motivation, and interest. *The experimenter-subject interaction may result in a trade-off between experimenter influence and subject influence that determines whether the experimenter's contribution to the experiment as a whole will predominate, become negligible, or fall somewhere in between.* Nash (1975) recognized the apparent psi potency of certain individuals and used the term "dominant participant" to describe the individual or individuals (be they subjects, agents, experimenters, assistants, etc.) who seem to control an experimental outcome. Special subjects are assumed to be the dominant participants when they serve as subjects in the psi laboratory. There are a few reports of "Westian" experimenters who have attained success with the "stars" but not with unselected subjects (e.g., West & Fisk, 1953; Pratt, 1973). In some cases, however, an experimenter can apparently inhibit or reverse the scoring direction of a special subject (Ryzl & Beloff, 1965; Fisk & West, 1958). Likewise, there are a few cases of "special experimenters" who seem to be the dominant participants in studies using unselected subjects. The combination of special subjects with special experimenters can result in *CR's* that register off the Richter scale (J. B. Rhine, 1934; Kanthamani & Kelly, 1974).

It should be noted that just as many (if not most) successful PK investigators are themselves successful PK subjects, many other psi researchers at some time have achieved significant above-chance scoring on psi tasks. While this listing is no doubt incomplete, it includes Stuart (in J. B. Rhine, 1934), Woodruff (Humphrey & J. B. Rhine, 1945), Osis (1953), Chauvin (1959), Rogers (1966), Carpenter (1966), S. R. Feather and L. E. Rhine (1969), Stanford (Stanford & Stevenson, 1972), Van de Castle (in Ullman et al., 1973), and Braud (in Honorton, 1975).

#### V. SUGGESTIONS FOR FURTHER RESEARCH

The purpose of this paper is not to simply protest "It's all experimenter effect." Can we examine existing studies in the light of our present knowledge of experimenter effects? One approach that becomes important in dealing with this issue is the re-examination of existing data. Although many types of psi experimenter effects are possible, post hoc analyses should cut down (though probably not eliminate) contamination of results by those effects due to experimenter expectancy.<sup>5</sup> The most solid evidence for an effect

<sup>5</sup> It is possible that an experimenter is unconsciously aware of ideas he will follow up in future analyses; also, precognitive experimenter effects cannot theoretically be ruled out.

comes from the combination of *consistent* post hoc findings with successful confirmations of the effect as a main result. The classic QD declines reported in the psychokinesis dice work provide a perfect example of the strength of this approach.

Several suggestions are listed below which stem from the ideas discussed in this paper, and which combine post hoc evidence with ad hoc confirmations. Suggestions are, of course, necessarily speculative, and so are not meant to be complete in and of themselves.

#### *Experimenter Patterns*

Subjects have been assumed to show particular characteristics or patterns in responding to certain test situations (i.e., displacement effects, psi-missing, position effects, chronological declines, differential effects, etc.). The evidence for many of these effects meets the highly desirable criterion of post hoc discovery followed by ad hoc confirmation. As Rhine has pointed out, these "signs of psi" seem to be one of the strongest methods for identifying the source of psi effects (Rhine, 1975). However, all the known signs of psi relate to identifying effects produced by the subject. At present these signs should be interpreted with caution, since *the role of the experimenter in their elicitation is unknown*; i.e., not all experimenters obtain them in their data. The signs of psi due to the experimenter remain an unexplored area. Unfortunately, losing significance may be one such sign. Another experimenter-effect pattern may be evident in the evolution of specific lines of research. It seems that the person or group responsible for the development of specific techniques for psi testing is often the one who achieves the best results using the techniques. This seems to have been the case with the Duke ESP-card testing work, Schmidt's four-button electronic RNG, and the Maimonides dream research. These and other patterns need much additional investigation.

The principle of looking for inherent characteristics or patterns in psi data seems to be the best way to identify the dominant participant in an experiment. Special subjects have long been known to exhibit characteristic idiosyncracies in their scoring patterns (e.g., J. B. Rhine, 1934; Soal & Bateman, 1954; Pratt, 1973; Kelly et al., 1975). Likewise, a search through the literature aimed at discerning idiosyncratic characteristics or scoring patterns in the experiments of individual researchers would help to identify and evaluate experimenter effects. Patterns may exist both within and across the work



of experimenters. Eisenbud (in Ullman et al., 1973) has suggested that experimenters leave their "personal mind prints" on experiments; these may manifest as personalized experimenter patterns. Humphrey (1943) noticed an interesting scoring pattern in her Earlham College series. She reported a scoring incline to the third quarter of the ESP record sheet, followed by a fourth-quarter decline. Though the data were collected from several types of ESP card tests, the results all showed similar position effects. Humphrey pointed out that not all subjects saw the record sheets (an assumed prerequisite for standard position effects) and that she, as experimenter, was a common factor across experiments. Recently, an apparent "experimenter pattern" has been noticed by John Stump in some of Honorton's PK data (Honorton, 1975). Further investigation may reveal many other examples, and may in fact allow for the identification of experimenters as dominant participants.

#### *Interaction of Participants*

Although one or more participants in an experiment may be "dominant participants," it cannot be assumed a priori that the other participants play an insignificant role. Investigating the psi effects produced by the interaction of various known dominant participants (e.g., a "special" experimenter working with a "special" subject or with another "special" experimenter) may allow insight into how various participants interact in forming an experimental result. Such interactions have been very difficult to interpret because of personality factors and other psychological variables also operating in the experimental environment. However, a new review of the data and additional research may point out useful trends.

#### *Parapsychological Demand Characteristics*

Experimental hypotheses should be examined for the operation of parapsychological demand characteristics which artificially link the subject and the target. Examples from the experimental literature will clarify this point. Several studies have investigated the effects of erotic vs. nonerotic targets on ESP performance. An ESP card and an erotic picture are placed in an envelope which then constitutes an erotic target. (ESP cards plus blank cards generally constitute the nonerotic target.) An assumption underlying this paradigm is that clairvoyance is like vision and that two things in close proximity will be clairvoyantly seen and associated together. However, unlike vision, ESP is independent of distance, and it is not clear what close proximity means when one is using clairvoyance.

Furthermore, significant results obtained in studies in which computers generated the targets and scored the data (Schmeidler, 1964a, 1964b; B. J. Feather, 1962) suggest that ESP is nothing like vision. ESP allows proper responses to targets that exist only as complex algorithms and electromagnetic pulses. The idea that erotic stimuli and ESP cards are "together" is quite possibly more an impression in the experimenter's mind than a representation of the process of psi. Nash (1974) has described similar problems with other differential tasks. Further thought and research are needed to help clarify this issue.

*"Active" Experimenter Possibility*

To the extent that declines in significance that accompany successive repetitions of an experiment are due primarily to active experimenter effects, one should be able to distinguish between these and declines due primarily to the subjects. Providing one uses "new" subjects each time, subject effects should hold up with repetition, whereas experimenter effects should decline.

Also, the PMIR model contains specific suggestions for procedures that should enhance or inhibit an experimenter's "active" psi abilities. Reference to assumptions 17 and 18 of the model<sup>6</sup> (Stanford, 1974b) should provide one with a base from which to work.

*The Complexity Factor*

The role of complexity in psi phenomena is an aspect of parapsychology that has not received the attention it deserves. This fundamental property of psi has great practical implications for psi experimenter effects—as well as for other important areas within parapsychology.

Foster (1940) used the term "diametric" to indicate that "ESP proceeds (diametrically) to its end quite independently of the ordinary circumferential steps of logic" (p. 327). This idea was supported (although not as clearly as one would like) by comparing scoring in card tasks requiring different amounts of information for success, e.g., blind matching vs. open matching (Rhine et al., 1940).

<sup>6</sup> Assumption 17 is as follows: "If a person focuses his attention on the target situation in a PK task and wishes for a particular event to occur, the probability that the event will be produced by PK will be reduced during that period of focused attention and wishing. An event which would eventually occur for non-psi reasons tends to be retarded as to its occurrence if a person focuses his attention upon the possibility of its occurrence, recognizes it is not occurring, and wishes it to occur" (p. 350). For assumption 18, see footnote 4.

This was also supported by investigations comparing high-aim and low-aim runs. Such experiments often report the same deviation (as reflected in the obtained  $p$  value) for the two conditions. The information needed for comparable deviations in the two cases is different, however, if one conceptualizes the process on a trial-by-trial basis (e.g., Thouless, 1972, p. 105). Here again, results are apparently related to the desired outcome and not to the complexity of the process involved.

An often implied assumption in parapsychology is that eliciting psi is difficult but that once elicited, it will follow reasonably consistent laws. This assumption must be questioned, however, if ESP achieves an outcome independent of the complexity of the process involved. If, as present evidence indicates, a subject can decode the electromagnetic impulses and complex algorithms of computer-generated targets (that never exist outside of the computer and are never seen by anyone), then it seems likely that the subject can just as well decode the experimental algorithm (i.e., the hypothesis). This becomes a severe problem in experiments where the subject lacks an identifiable experience of his own; that is, when the subject must depend on the experimenter to tell him if he has done well on the task. In these cases, the subject's ultimate goal may be to please the experimenter and thus receive a favorable report of his performance.

The complexity factor also plays an important role in the possibility of "active" experimenter effects. For laboratory work, *a PK trial may be defined as a random process which has an outcome that someone wishes to influence or would benefit from influencing*. This definition implies that *PK manifests itself only in the observed outcome of a process*—an obvious assumption borrowed from modern physics. That we know only of events that are observed has important consequences for PK as well as for physics. Apparently PK can influence any observed outcome. This may limit the questions that can be asked about the process leading to that outcome. It may not be possible, for example, to determine ad hoc which bounces of a die are influenced in order to arrive at the desired outcome of a roll. Monitoring dice bounces simply adds new observations, each of which is susceptible to PK influence. Support for this idea comes from present evidence which indicates that the complexity of a process is not a significant factor in determining the magnitude of a PK effect (L. E. Rhine, 1970; Schmidt, 1974).

Any psi experiment (whether PK or ESP) can be viewed as a series of random events which culminate in a single random out-

come. The outcome is the significance level of the experiment, and it certainly bears a ^direct relationship to the desires of, or benefits to, the experimenter. From the experimenter's point of view, *any experiment may be modeled as a single complex PK trial*. From this perspective, losses of effect and PK declines can be seen as functionally equivalent results which stem from a common cause.

Furthermore, if psi is independent of complexity, something critical evidently occurs when an outcome is "observed." An experiment reported by Schmidt (1974) will help to clarify this notion: Schmidt compared the effect of PK on two random generators under psychologically equivalent conditions; i.e., his subjects received the same feedback regardless of which RNG was in use on a given trial. One RNG was "simple" in that the output depended upon a single random binary event. The other RNG was "complex" and the output depended upon a majority vote of one hundred random binary events. Very significant results were achieved with each RNG. The "complex" condition showed only insignificantly lower scoring. The point is that, if one uses a majority-vote procedure before he looks at the outcome of each event, the result is simply a more complex process, and an increase in scoring rate is not expected. However, if one uses a majority-vote procedure *after looking at each event*, an increase in the scoring rate is expected. Again, if psi is independent of complexity, something critical apparently occurs when an outcome is observed. This may help to explain the "lab lore" superstition that it is better not to look at data until they have all been collected (and until the experimenter is in a good mood).

If psi is *not* independent of complexity, however—and it should be noted that the evidence the authors are aware of is far from conclusive—one should be able to establish limits on its operation. Fortunately, with modern technology—and, of course, the existing literature—these questions can be readily investigated. It is clear that the complexity issue has important implications for many areas of parapsychology, as well as for the problem of experimenter effects.

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